
OLD
AND
NEW
ASTRONOMY
R A PROCTOR

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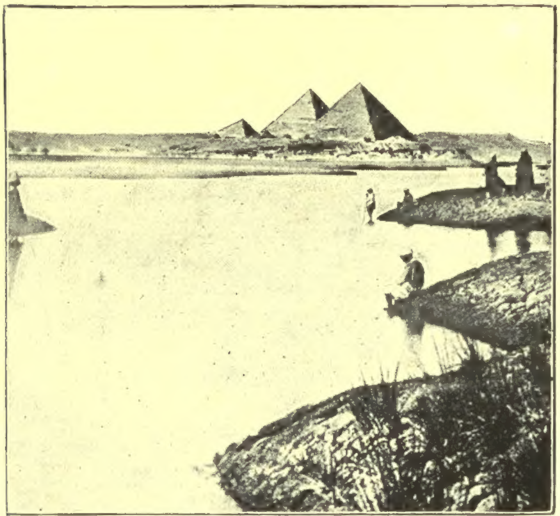
PLATE I.

1. ANCIENT TOMB-TEMPLE OBSERVATORIES

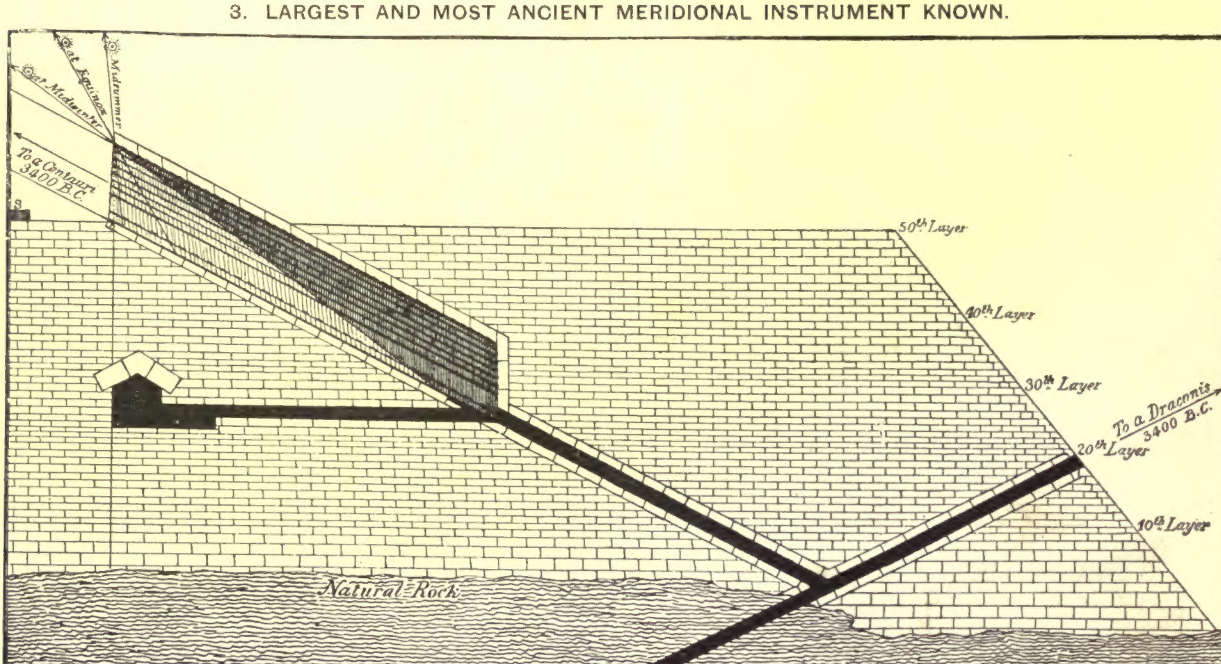


PYRAMIDS OF GHIZEH, SEEN FROM NORTH-EAST.

2. EARTH-RECORDS—HUMAN AND MORE ANCIENT

FIRST AND SECOND PYRAMIDS AND OLD TERTIARY SANDS.

3. LARGEST AND MOST ANCIENT MERIDIONAL INSTRUMENT KNOWN.



VERTICAL SECTION OF THE GREAT PYRAMID, SHOWING THE ASCENDING AND DESCENDING PASSAGES, GRAND GALLERY, AND QUEEN'S CHAMBER.

1893 Thos H. Coward.
Silecroft
- Cumberland

OLD AND NEW

ASTRONOMY

BY

RICHARD A. PROCTOR

AUTHOR OF 'ASTRONOMY' IN THE ENCYCLOPÆDIA BRITANNICA
AND IN THE AMERICAN CYCLOPÆDIA

COMPLETED BY

A. COWPER RANYARD

WITH NUMEROUS PLATES AND WOODCUTS

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PREFACE

The 'Old and New Astronomy' was planned by Mr. Proctor more than a quarter of a century ago. In an article printed in 'Knowledge' for March 1888, he says that, while writing his first book on 'Saturn and its System,' which was published in 1865, he proposed to himself to follow up his book on Saturn by a series of similar monographs on Jupiter, Mars, the Sun, the Moon, Comets and Meteors, the Stars, Star Clusters, and Nebulæ. After the publication of 'Saturn and its System,' he deferred the execution of his general plan for a while, in order to prepare a series of star charts and a 'Handbook of the Stars'; but, having completed these, he commenced a monograph on the planet Mars, on which he was occupied when, in 1866, the failure of a bank, in which he was a considerable shareholder, deprived him of the means which would have enabled him to carry on the work he had begun without anxiety as to the commercial success of the volumes required for its completion.

His monetary losses obliged him to turn to more popular writing and to lecturing for the support of his family; this afforded him an excellent training in the clearness and simplicity of expression that is needed for popular exposition. He found, however, that the general public could not be attracted by writing which required a prolonged effort of reasoning or arduous study to understand, and, though the idea of the separate treatises was speedily given up, he always kept in view the time when he should be able to devote himself to writing a more serious work on general Astronomy, giving the history of each subject dealt with, as well as the latest results arrived at. Thus the idea of the 'Old and New Astronomy' gradually developed itself, and, with this book in view, he continued, during the whole of his literary life, to collect material for the work, which he intended to be his *magnum opus*, but which he unfortunately did not live to complete.

The publication of the 'Old and New Astronomy' was announced in 1887, and the First Part was published in March 1888. At the date of Mr. Proctor's death, in September 1888, Part VI. had been issued and Part VII. was in type. The chapters on the Planets were in manuscript, and appeared to be nearly ready

for publication; but, as they had been written at Mr. PROCTOR's home in Florida, where he was at a distance from libraries, more work was required to complete them than I expected when I undertook to finish the volume. The manuscript, as far as it went, ended with the description of the discovery of Neptune, the outermost member of the solar system. Unfortunately, Mr. PROCTOR had written nothing with regard to the Universe of Stars, the Distribution of Nebulæ, and the Construction of the Milky Way, though it was known by his Widow and friends that he intended to make these sections a special feature of the book.

It was in this department of Astronomy that he had done his most original and lasting work, work by which his name will probably be long remembered. I have, therefore, endeavoured, in the Stellar section of the 'Old and New Astronomy,' to give as complete a review as I could of the various theories which have been advocated with regard to the Milky Way and the distribution of Stars and Nebulæ.

As no chain can be stronger than its weakest link, no conclusion can be more sure than the weakest of the premisses on which it is founded. Hitherto it seems to me that sufficient discrimination has not been exercised in selecting data about which there can be no doubt, on which to found conclusions with regard to the Stellar Universe. I have, therefore, in discussing the distribution of matter in the space around us, and other problems of the New Astronomy, avoided making use of data which depend upon observations bordering on the limit of visibility, such as the parallax of any but the nearest stars, or the motion of stars in the line of sight, with regard to which there is still considerable uncertainty and a wide difference in the results obtained by different observers. The photometric observations, the stellar photographs, and observations of proper motions, and stellar distribution, which have been made use of, afford a much surer foundation for cosmical deductions.

I have not attempted to draw up a list of the errata which must inevitably occur in a work of this kind. In the parts already written by Mr. Proctor I have left him to express his own views without material modification, though in a few cases I do not agree with his conclusions, or with the justice of some of his remarks with regard to persons. In completing the volume, I have to apologise to the subscribers for the long delay in issuing the later parts, which has been necessitated by the endeavour to make the work as complete as possible.

A. COWPER RANYARD.

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OLD AND NEW

ASTRONOMY.

INTRODUCTION.

(1.) Astronomy stands first among the sciences in the grandeur of the relations of time and space with which it deals. The vistas of time which geology opens to our view are perhaps more impressive to our finite minds than the eternities suggested by astronomy. But as regards extension in space, the domain of geology is utterly insignificant by comparison with even the threshold of the infinities into which astronomy invites us. The geologist's field of research is but a small part even of the earth; and astronomy teaches us to regard the whole system to which the earth belongs as occupying the merest speck of space by comparison with the visible portion of the stellar domain; while the sphere enclosing all the stars visible to the naked eye is small by comparison with the spaces revealed by the telescope, and infinitely small by comparison with those spaces whose existence is suggested by telescopic research.

(2.) Nor is even the vastness of the domain of astronomy the most impressive feature of the science. The wonderful variety recognised within that domain is but faintly pictured in the solar system with all its various forms of matter—sun, primary planets, and moons; major planets, minor planets, and asteroids; planet-girdling rings, meteoric systems, and comets; with perchance other forms of matter hitherto unrecognised. Beyond our system lie giant suns, suns like our own, and minor suns; double, triple, and multiple suns; all orders of star-clusters and star-clouds; streams, branches, nodules, and gathering aggregations of suns in endless variety; and great masses of glowing gas, occupying regions of space compared with which the domain of the mightiest sun is but as a point.

(3.) And beyond the wideness of the domain of astronomy, and the

amazing variety recognised within that domain, there remain the yet more impressive lessons taught by the infinite vitality which pervades every portion of space, and by the vast periods of time over which astronomy must extend its survey.

If such powers of vision, and also (for they would be even more needed) such powers of conception, were given to the astronomer, that the extent of the domain which the telescope has revealed to man could be adequately recognised, while he further became cognisant of the way in which the various portions of that domain are occupied, I conceive that, deeply as he would be impressed by the amazing scene, the sense of wonder he would experience would sink almost into nothingness by comparison with that which would overwhelm him could he recognise with equal clearness the movements taking place amongst the orbs presented to his contemplation—could he see moons and moon-systems circling around primary planets, these urging their way with inconceivable velocity around their central suns, while amid the star-depths the suns were seen swiftly travelling on their several courses, star-streams and star-clusters aggregating or segregating according to the various influences of the attractions to which they were subject, and the vast spaces occupied by the gaseous nebulæ stirred to their inmost depths by the action of mighty forces whose real nature is as yet unknown to us. During millions, during hundreds, thousands, millions of millions of years in the past, such movements have been in progress, and they will continue during countless millions of years which are yet to come. Nay, it may well be that to speak of time, thus viewed, as measurable by years, even though we number them by millions, is as idle as it would be to speak of miles when dealing with the measurement of star-strewn space.

(4.) The mind cannot but be strengthened and invigorated, it cannot but be purified and elevated, by the contemplation of a scene so magnificent, imperfect though the means may be by which the wonders of the scene are made known to us. The information given by the telescope is indeed but piecemeal, and as yet no adequate attempts have been made to bring the whole array of known facts as far as possible into one grand picture ; but, seen as it is only by parts, and (even so) only as through a veil and darkly, the scene presented to the astronomer is grander and more awe-inspiring than aught else which man is privileged to contemplate.

(5.) For the study of this, the noblest of all the sciences, absolute independence of mind is essential. The student should not, indeed, be unduly ready to dispute the dicta of those who have devoted themselves to the advancement of astronomy ; nor again is it fitting that he should attempt to make independent inquiries into matters belonging to such branches of the

science as he has not yet thoroughly examined. Neither dispute nor cavil is desirable: but all statements submitted to the student's consideration should be carefully examined and analysed. The student must attempt to render the subject as far as possible his own by such a survey of the evidence as will suffice to give him independent reasons for believing in the correctness of what is taught him. It will not unfrequently happen that while thus engaged he will detect, or imagine he has detected, errors of greater or less importance. He should be prepared to find that in most cases these seeming errors have no real existence, but arise from misapprehensions on his own part—a circumstance which will of itself serve to convince him of the value of the kind of investigation by which such misapprehensions have been brought to light. But in other instances he may find that there have been real errors in the books—a fact which will equally convince him of the importance of the careful analysis of all statements lying within his range of investigation.[1] I may quote here the words of Professor Huxley, both as to the value of scientific doubt, and as to the nature of that sort of doubt which the student should alone permit himself: 'There is a path that leads to truth so surely that anyone who will follow it must needs reach the goal, whether his capacity be great or small. And there is one guiding rule by which a man may always find this path, and keep himself from straying when he has found it. This golden rule is, "Give unqualified assent to no propositions but those the truth of which is so clear and distinct that they cannot be doubted." The enunciation of this first commandment of science consecrates doubt. It removes doubt from the seat of penance among the grievous sins to which it had long been condemned, and enthrones it in that high place among the primary duties which is assigned to it by the scientific conscience of these latter days.' But 'you must remember that the sort of doubt which has thus been consecrated is that which Goethe has called "the active scepticism, whose whole aim is to conquer itself;" and not that other sort which is born of flippancy and ignorance, and whose aim is only to perpetuate itself as an excuse for idleness and indifference.'

(6.) It is not merely necessary that the student should so examine astronomical facts as to become possessed with a feeling of their reality, but he cannot be rightly said to 'have astronomy' at all (to use Shakespeare's apt expression) until he is capable of picturing to himself, however inadequately, the truths of the science. A man may have at his fingers' ends the distances,

[1] The necessity of such inquiry is increased by the circumstance that too often the statements made in one work on astronomy are repeated without modification or examination in others, thence to be re-quoted in other works, with perhaps fresh errors due to misprints, misapprehension, &c.

volumes, and densities of all the planets, the rates at which they move, the physical features they present, and a hundred other facts equally important in astronomy ; but, unless he has in his mind's eye a picture of the solar system, with all its wonderful variety, and all its yet more amazing vitality, he has not yet passed even the threshold of the science. He must strive to picture in his mind the mighty mass of the sun, ruling from the centre of the scheme the whole of that family to the several members of which he distributes their due proportion of light and heat. Close round the sun the student should see the family of minor planets ; small Mercury lit up resplendently by the sun, round which he speeds with unmatched velocity ; Venus and Earth, the twin planets of the solar system, alike in all features, save only that Venus has no satellite ; and lastly, ruddy Mars, the miniature of our own earth, with his two minute attendants. Then, beyond the path round which Mars urges his course, the student must picture to himself the interlacing paths of hundreds of asteroids, tiny orbs compared with even the least of the minor family of planets, yet each pursuing its independent course around the sun, many doubtless approaching almost within hail (if one may so speak) of their fellow-orbs, and many free to depart far more widely than any of the primary planets from the general level near which the planetary motions are performed. Then lastly, he should picture to himself that wonderful outer family of planets, the least of which exceeds many times in bulk the combined volume of all the minor planets and asteroids. The vast globe of Jupiter circled about by his symmetrical family of satellites ; the complex system of Saturn, with his marvellous ring-system and a family of satellites the outermost of which has an orbit range of more than four and a half millions of miles ; Uranus and Neptune, brother orbs, almost lost in the immensity of their distance—all these planets, and all the wonders which the telescope has taught us respecting them, should be clearly pictured. In particular, the enormous distances separating the paths of these outer giants from each other, and from the sun, should be clearly apprehended, and that strangely incorrect picture which defaces so many of our books on astronomy, wherein the paths of the planets are seen separated by nearly equal distances from each other, should be as far as possible forgotten. When the student has apprehended the fact that the whole family of the minor planets could not span the distance between the orbits of Jupiter and Saturn, while the distance between the orbits of Saturn and Uranus, or of Uranus and Neptune, almost equals the full span of the orbit of Jupiter, he has already made an important step from mere book knowledge, almost useless in itself, towards that clear recognition of actual relations which should be the true end of scientific study.

(7.) But beyond the solar system the thoughts of the student of astronomy should range until he begins to apprehend to some extent the vastness of those abysses by which our solar system is separated on all sides from the realm of the fixed stars—that is, of the orbs which are the centres of other systems like itself. And I know of no consideration better calculated to bring this idea clearly before the mind of the student than the thought that our sun, with its attendant family of planets, is speeding through space with a velocity altogether past our powers of conception, while yet no signs of his motion, and our motion with him, can be recognised, even after the lapse of centuries, save by taxing to the utmost the powers of our finest telescopes. The clear recognition of this fact, and of its real significance, enables the thoughtful student to become conscious of the vastness of the depths separating us from the nearest fixed star, even though he can never form an adequate conception of their tremendous proportions. That within the region which forms his present domain our sun traverses hundreds of millions of miles each year, while yet he seems always to hold a fixed place in that domain,—this is the great fact which serves most strikingly to impress upon us the vastness of the interstellar spaces.

(8.) There is another, however, which deserves mention. We commonly find those comets which sweep round the sun in parabolic or hyperbolic orbits spoken of as visitants from the domain of other stars. And so in truth they are. But we must consider also the enormous intervals of time which have elapsed since these startling visitants were travelling close round some other star, making their periastral swoop before setting forth on that enormous journey which had to be traversed before they could become visible to our astronomers ! Taking into account the directions in which certain comets have reached us, and assigning to the stars seen in such directions, the least distances compatible with known facts, it is certain that millions of years at least must have elapsed since those comets were last in periastral passage. When we consider how some comets have flitted from star to star during a long interstellar existence, the mind shrinks utterly before the contemplation of the vastness of the time-intervals which have elapsed since those journeyings first commenced : yet the distances suggested by these time-intervals afford but an imperfect means of estimating the scale on which the sidereal system is built.

(9.) I will not dwell here on those further conceptions—equally necessary to complete the picture which the true student of astronomy should have present in his mind—which relate to the constitution of the sidereal spaces, to the motions and changes taking place within them, and to the relations which the various forms of matter existing within those spaces

bear to each other, or to the forms with which we are familiar. It is to be remarked, as regards many of these conceptions, that their nature will depend on the views entertained by the student as to the accuracy of the theories formed by different astronomers respecting the way in which the various objects revealed by the telescope are distributed throughout surrounding space. Doubt rests on many points, and must ever rest on some; yet what is actually known is sufficient to form a picture full of interest as respects all its visible details, and not the less impressive, perhaps, that a large portion of its extent is still hidden in darkness and mystery.

(10.) It is little necessary to point out that the course of study by which astronomical relations may thus become clearly pictured forms a valuable mental training. Whether we regard the careful analysis of the evidence on which astronomical facts rest, the study of the various facts as they are brought one after another to the student's knowledge, the due co-ordination of each with its fellows, or, finally and chiefly, that *intention* of the thoughts on the complete series of facts by which alone their real significance can be apprehended, we see in astronomy the apt means for disciplining the mind and fitting it for the highest work of which it may be capable.

(11.) But, besides the study of astronomical facts, we must consider here the actual study of the heavens, either with the unaided eye or with the telescope. I speak of the study of the heavens with the unaided eye, though many in this age of cheap telescopes may be inclined to smile at the thought that such study can have any value either to the student or to the science of astronomy. As a matter of fact, however, I am of those who believe that much may still be learnt even from the study of the stellar heavens without optical instruments of any sort. Setting aside the fact that it is in the astronomer's power to add by such study to our store of knowledge, it is of the utmost importance that he should become directly cognisant of astronomical facts, whether those facts be the seeming motions of the celestial bodies, the telescopic aspect of the sun, moon, planets, stars, and nebulæ, or the statistical relations, changes, motions, and so on, of the stars of various orders. A student of astronomy whose knowledge is founded on actual observation holds all his knowledge with far securer grasp than one who has devoted his attention, however earnestly, to the study of book-knowledge alone.

(12.) I have spoken of the value of astronomical research. No one is likely to dispute the assertion that in our highly utilitarian age the practical applications of astronomy subserve highly important purposes. The whole system of commerce, for example, depends on the accuracy with which the astronomers of national observatories note the apparent motions of the stars. The survey of land districts cannot be efficiently carried out without astrono-

mical observations and a careful consideration of astronomical principles. And besides a number of other instances in which astronomy is directly applied to practically useful purposes, it is only necessary to consider how many and what important interests depend on the commercial relations between different countries, and on the careful survey of the earth's surface, to see that astronomy holds almost as high a position among the useful sciences as among those which relate chiefly to the extension of our knowledge.

(13.) It is not to this utilitarian aspect of astronomy, however, that I refer, in speaking of its value, but to its use as a means of mental training;—whether as affording subjects of profitable contemplation; or as offering problems the inquiry into which cannot fail to discipline the mind; or lastly, as suggesting the actual application of methods of observation by which at once the patience and ingenuity of the observer may be exercised, his knowledge extended, and his mind supplied with fresh subjects for study.

For, whatever those may think who have not familiarised themselves with the teachings of astronomy, there can be no question that the highest place is given by astronomers themselves to those rather who have advanced our knowledge of astronomical facts—whether by careful observation or by judicious investigation of observed relations—than to those who have applied astronomy most successfully to practical purposes. If we take the names which are most highly honoured by astronomers, and consider why they are honoured, we shall see that this is so. The labours of Copernicus, Kepler, and Newton were by no means primarily directed to practical astronomy. Their effect in advancing the study of practical astronomy may be regarded as, in a sense, accidental; or rather this result affords an illustration of the fact that, in scientific research, we need not keep continually before our minds the question '*Cui bono?*' since gains which the student of science himself had not perceived have resulted from even the least promising researches.

(14.) We know that Copernicus only sought to explain observed appearances by a simpler theory than that which was in vogue in his day. To Kepler, perhaps, the idea may have suggested itself that the laws he sought so earnestly, in order to explain the movements of Mars as traced by the best observational methods yet applied, might result in giving to astronomers a new power of predicting the motions of Mars and of the other planets. But certainly the object which Kepler set himself was to replace the disorder of the Ptolemaic system and the partial symmetry of the system of Copernicus, by a harmonious series of relations. When he had succeeded, his boast was, *not* that he had shown astronomers how henceforth they might confidently predict the motions of the celestial bodies, but that he had 'found the golden vases of the Egyptians.' Nor is it possible to read Newton's own

account of those researches by which the law of gravitation was established without feeling that, to himself at least, the practical application of the law in after-times was of secondary import. It was the law itself, regarded as a discovery respecting the manner in which the bodies distributed throughout space influence and are influenced by each other, which he valued.

(15.) If we turn our thoughts to the astronomy of the past century, we recognise the same fact. It would be difficult to find in the whole of that noble series of papers which William Herschel contributed to the pages of the 'Philosophical Transactions' a single paragraph directed to the application of astronomical discoveries to practical purposes. The discovery of Uranus, which so many suppose to have been Herschel's noblest work, was undoubtedly full of interest, but it certainly was not a practically useful discovery. And to turn to that which was in reality the noblest work achieved by Herschel—his researches into depths lying far beyond the range of the unaided vision—in what sense can the counting of myriads of stars or the discovery of thousands of nebulæ be regarded as advancing in the slightest degree the material interests of mankind ? Even if it hereafter happened that the discovery of Uranus or the processes of star-gauging should indirectly lead to some practical results of value, it would still remain certain that William Herschel had had no such results in his thoughts when he prosecuted his researches.

(16.) In our own time Sir John Herschel was justly held by all to be the leading astronomer of his day ; yet it would be difficult to find in a single astronomical research of his the least practical value ; while certainly in that long series of observations on which astronomers base their high opinion of him, there was no practical value whatever. Sir John Herschel had already devoted eight years of his life to the re-examination of his father's work, with the chief end of acquiring a mastery over his telescope, when at the Cape of Good Hope he began a series of observations which formed the exact counterpart of his father's observations in the northern skies. Star-gauging, the noting of double stars, the search for nebulæ—all such researches must needs advance the science of astronomy, but not one of them has any practical utility.

(17.) Even if we take the well-merited fame of departmental astronomers (so to distinguish the workers in special branches from men who, like the Herschels, have made all astronomy their subject) we cannot recognise the title to such fame in practically useful work. When Adams and Leverrier by subtle processes of research showed astronomers where to turn their telescopes to detect the planet whose influence had disturbed the motions of Uranus, they were not in any way advancing the material interests of the

human race. It may happen, indeed, that some of the mathematical processes devised or developed by those astronomers may one day be applied in some practical manner; but no one will, on this account, assign such practical results as the real title of Adams or Leverrier to astronomical fame.

(18.) It is as a subject for study and contemplation, as a means for training and exercising, but likewise for ennobling and purifying the mind, that astronomy should be studied by all except those whose duty it is to deal with the heavenly orbs in another way. The astronomical surveyor must work unmoved by such thoughts, or he will scarcely work in an effective manner. The meaning of the stupendous celestial mechanism, the beauty and harmony of the celestial architecture, it is not for the Flamsteeds, the Maskelynes, and the Airys—useful, nay, essential though their work may be —but for the Newtons and Herschels of astronomy, to investigate. It is the celestial scene as viewed and studied by philosophers such as these, not merely as surveyed in Government observatories, that I propose to contemplate in the present volume: for astronomy, regarded as a means of philosophic training, owes almost all its value to men of the former type, scarcely any (though commerce owes much) to those of the latter.

CHAPTER I.

ANCIENT AND MODERN METHODS OF OBSERVING THE HEAVENLY BODIES.

(19.) Formerly it was not held an idle task to attempt to determine how astronomy began and what people were its first teachers. But the inquiry belonged to times when erroneous ideas were entertained about the past of the human race. It was in reality scarcely more idle to attack such a problem than it would have been then (as it is now) to endeavour to ascertain what is the real influence of Jupiter or of Saturn on the fortunes of men and nations. The belief that the problem might be solved was based on notions belonging to the days when judicial astrology was regarded as a sound science. When men imagined that the human race dated back but a few thousand years, when they supposed that to the various branches of that race definite pursuits could be assigned, and when therefore it seemed to them that astronomy necessarily had its origin among a certain people however widely the science had subsequently spread, it seemed worth while to endeavour to find out to what race astronomy was due, and under what conditions its first steps were made. Knowing what we do now, we might as well ask whether speech was first practised by Indians or Chinese, by Egyptians or by Mexicans, and under what conditions the use of spoken words spread from some first originators of language until it became common to the human race. Astronomy, regarding the word in its widest meaning, did not begin here or there among the nations and races of the earth, but everywhere and among all races. Its first teachings were only developed into a system among the nations which rose to something like civilisation : but these also were many ; and regarding astronomy as a science having its origin in civilised times, we still recognise that its origin was multiform.

(20.) The wildest and most savage races must in some degree have noted the appearance, the movements, nay, the influence also, of the heavenly bodies. The hunter cannot but recognise the power of the sun as day by day its orb returns to the skies ; he cannot but note how the moon comes back month by month to be a light by night ; he must be a dull and unobservant savage

if he does not recognise also the annual growth and decay of the sun's influence; and lastly, though not all the wilder races of savages may have observed how special star-groups rule by night at special seasons of the year, yet we know that even among those races which never rose above a life of hunting and fishing, the more remarkable star-groups were recognised and roughly associated with the progress of the year. This could not be done without some notice being taken of the planets, whose movements carry them from time to time athwart remarkable star-groups. We may well believe that no race of mere hunters ever recognised the periodic character of the planetary movements: yet must men even of the wildest and most savage races have observed that some among the stars seem free to wander about over the domed surface on which most of their fellow-stars appear to have fixed and determinate places.

(21.) Doubtless the actual observation of the heavenly bodies would begin in each race with the pastoral stage, which may be regarded as the first step from savage towards civilised life. Pastoral races would not long have failed to observe the orderly nature of the movements both of the sun and moon. The steady diurnal motion of the sun, and the equally steady though slower diurnal motion of the moon, would first no doubt have attracted their attention. Then the orderly return of the moon after equal intervals of time to her rule over the night would be recognised. The value of the moon as a measurer of time, a great celestial index whereby days and weeks might be recorded, would be a matter of very great interest to races in this stage of their progress, the stage when first the measurement of time had become a matter of importance. For, with the pastoral period came not only occasion for careful watch on breeding cattle, and therefore for some means of measuring such periods as months, but also the rise of hired labour, and therefore occasion for measuring suitable periods for hiring herdsmen.

(22.) The oldest records which have reached us present pictures of pastoral life such as we might have anticipated in these respects. We find the cares and anxieties of that life first described in company with the employment of hired labour, and the period of time employed is forthwith mentioned. The first hiring time noticed at all in human records is the *week*, the natural division of the still more natural and obvious *month*.[1]

[1] The earliest record we have of hiring is that contained in Genesis, chap. xxix.; and we find the references to hiring, wages, &c. in company with much curious matter in regard to breeding; showing that, at any rate, the observation of natural facts had begun, however mistaken may have been the ideas about natural laws to which such observation had led. Laban (Lábân) asks Jacob (Ya 'aqôb), 'What shall thy wages be?' who replies, 'I will serve thee seven years for Rachel (Rá'hel).' The seven years are presently spoken of as a week (of years); 'Fulfil the week of this one,' says Laban, 'and we will give thee the other also for the service which thou shalt

The moon then almost from the first came to be regarded as a ‘measurer.’ [1]

(23.) But probably it was not until agricultural pursuits began to occupy the chief attention of an advancing race that the heavenly bodies were specially observed. The hunter may be impressed by the phenomena of nature, but the attention he gives to them is only such as is in a sense actually compelled from him. The herdsman, again, is led rather to recognise the periodicity of natural processes on which his welfare depends, and the power of measuring time-periods which the heavenly bodies afford, than closely to observe times and seasons in order that his operations may thereby be regulated. But the tiller of the soil must determine these times and seasons accurately, or his work will be thrown away. He must sow at the right season or he will reap no harvest. He must not only learn how vegetation obeys the influences of the sun, but he must find out also how the movement of the sun may be measured and followed. The stars which are rising and setting with the sun in spring and summer, autumn and winter, must be noted in order that the progression of those seasons may be duly recognised. Before this can be done it must be ascertained that such indications are regular, and therefore may be trusted. The movements of the moon on the star-sphere serve to show

serve with me yet seven other years.’ Later Laban says, ‘Appoint me thy wages and I will give it,’ and we find Jacob planning what among ancient pastoral races would probably have been thought a clever method of securing the most favourable increase among the herds after their months were fulfilled. In verse 14, the month had already been mentioned, in obvious connection with hiring; and doubtless the month was even older than the week as a time measure. Yet throughout Genesis xxix. we find all actual hiring, with direct reference to wages, associated with the week, the natural subdivision of the month.

It should be mentioned that, although the story of Laban and Jacob belongs to the Jehovistic or later portions of the book of Genesis, it bears manifest signs of having been borrowed from a much older Elohistic document. Lenormant remarks that the Jehovist compiler has followed the older narrative step by step, for it can be recognised with certainty in several passages. It is necessary to note this, as otherwise the antiquity of the references to the month, the week, pastoral pursuits, and so forth, would not be obvious.

[1] The use of weeks and months in measuring time can be recognised, however, in the still earlier story of the Deluge. It is seen, also, in the very names by which among men of old time she was called. If ancient races, as Whewell considers likely, had a name for her as a conspicuous object before they began to regard her as a measurer of time, that name either fell out of use, or else we find it (save only in Latin) in company with another name relating to the moon's use as a celestial index. Thus we have in the Sanskrit *Mâsa*, in the Zend *Mas*, in the Persian *Mah*, in the Gothic *Mena*, in the Erse *Mios*, in the Lithuanian *Mienu*, in all of which the root indicating measurement, and found in *measure*, *mensuration*, *mensa*, *mensis*, *metior*, &c. is found. If the Greeks had Σελήνη connected with Σέλας, light, and the Romans *Luna* connected with *lux*, yet the Greeks also had Μήνη, associated with Μήν, a *month*—as our own *moon* with *month*—and with Μέσος, *mean*; while, though among the Romans the measurement name no longer appeared as directly belonging to the moon, we find it in *mensis*, the month. The connection between the measurement of time and the lunar monthly motion is rendered obvious by such words as *mensio* and *mensura*, a measuring, *mensor*, a measurer, *mensum*, a measured quantity, and so forth. In the Hebrew we have *manah*, to measure, and *maneh* or *mna*, a measured portion—probable evidence that the Hebrews borrowed their names for measurement from Aryan races. The same root appears in *mene* (*mene, mene, tekel, phares*), *al-manac*, &c.

the fixity of the stars' positions *inter se*; and if the study of those motions be suitably combined with observations of the sun, the stability of the stellar positions in regard to the sun's annual and diurnal career can be ascertained.

(24.) Such observations early served, no doubt, to show that the planets cannot be trusted to tell the progress of the seasons. Yet the planets, even after their unsatisfactory character as season-marks had thus been ascertained, would still remain the objects of careful study. No race which had recognised how the sun rules the day and the year and the seasons and with them agricultural operations, and how the moon, another voyager round the star-sphere, rules the month and measures time conveniently for man (to say nothing of her observed influence on the tides), could fail to be impressed by the thought that the five other voyagers round the celestial concave must have their special work or purpose also. If it had been worth men's while to determine the laws according to which the sun and moon pursue their course, it must surely, they would infer, be worth their while to ascertain in like manner the laws according to which the bright and shifty Mercury, and Venus the star of morn and eve, with ruddy Mars, resplendent Jupiter, and gloomy Saturn, various in aspect and in rate of motion, pour their influences upon the earth, unknown though the nature of those influences might be.

(25.) That with the gradual progress of such observations religious or superstitious ideas should have been associated with the heavenly bodies, and especially with the sun, moon, and planets, was natural enough. We need by no means suppose that, because not only such ideas prevailed among many ancient nations but even in details their fancies were alike, the astronomical and astrological ideas of ancient nations had a common origin. Or rather we must recognise that common origin in the identity of the observed phenomena, not in the existence of one set of astronomical teachers, from whom all the doctrines as well as all the fancies of ancient races had been derived. The daily contest between the sun's light and the gloom of night, the annual contest between the sun's heat and the cold of winter, the monthly changes of the moon's shape and her regularly renewed death and restoration, were phenomena observed by Egyptians as well as by Chaldæans, by Indians as well as by Persians, by Chinese, Peruvians, Mexicans—by all races, in fine, which ever rose from savagery to civilisation. As a story of a deluge by which the waters of the ocean had been carried above the highest hills, could not fail to be common among all races who looked upon the earth and saw sea-shells and kindred marine substances in the rocks they quarried for their buildings, so the story of an ever-renewed contest between light and darkness, between heat and cold, between life and death—a contest on which all the influences affecting the fortunes of the human race depended—could not but be common among all

races who looked upon the heavens and there saw the sun and moon contending with darkness in their behalf. The daily birth of the sun as god of the day, his birth with each midwinter as god of the year, could not fail to suggest ideas common to all ancient races, and tinging not only the superstitious fancies of their youth (as races) but even the religions of their later careers.

(26.) Again, as gradually the power of exact prediction was acquired, as the watchmen of the night learnt to determine by the appearance of such and such stars or star-groups the progress of the sun as god of the year, the hope would arise that predictions affecting the fortunes of men in detail, as well as generally, might be obtained, if the planets were watched in the same way. The influence of the sun on men's fortunes was manifest enough and could be predicted ; the moon almost as clearly played her part in influencing men, and her movements could also be predicted.[1] Why should not the nature of the planetary influences be determined in like manner ? If this could be done the study of planetary movements would be as useful, men supposed, as the study of the sun's movements had already proved. Hence it became worth men's while to inquire what are the special influences exerted by the planets ; so that by carefully studying their movements and learning how to predict them, events affecting the fortunes of men and nations might be anticipated, and even to some degree controlled.

(27.) We touch here, but need only touch, on the relation between astrology and astronomy. It would be as idle to overlook the influence which the study of astrology has had upon the progress of astronomy as it would be to overlook the influence of alchemy on chemistry. As alchemy was the parent of chemistry, so was astrology the parent of astronomy. The alchemists hoped that by continued and profound study they might discover the means of transmuting baser into nobler substances, obtain the art of prolonging life indefinitely, and detect other secrets, hidden as yet, but as they supposed discoverable. Nothing but such hopes as these encouraged them in their arduous and even dangerous labours. It was the same with the study of astrology. Men believed in those days—they could not help believing—that the heavenly bodies were potent in influence, were signs which might be read (could men but discover their secret), were orbs whose powers might be used to aid the ambition of princes, the search for wealth, the struggle for influence, the conduct even of the ordinary pursuits of daily life. The belief

[1] There are reasons for believing that men early supposed the moon influenced the minds of men as directly as the sun influences their bodily fortunes. Her influence in a bad sense is indicated by the association supposed to exist between lunar influences and lunacy; the word 'mania' is as clearly connected with the other name of the moon as 'lunacy' with *luna*; but *mens* also is a moon-derived word, associated with her supposed influence on the mind in general, as well as on the mind diseased. The very name Man—the 'measurer,'—is akin to moon, *mens*, &c.

was certain to grow out of the observed influence of the sun and moon, out of the power men had obtained of predicting their movements, and out of the advantage which could be taken of the knowledge of solar and lunar changes to derive wealth from pasturage and tillage. So certain was this false belief to grow and to be developed in special directions, that all the nations of antiquity fell naturally into the same error. They even made similar mistakes in many matters of detail. But into such points we need not here make special inquiry. All we need note is, that none of the races of antiquity rose above a certain level in civilisation without developing a belief in the influences of the heavenly bodies, and devising systems for reading and ruling the planets. Out of the researches for forming such systems, and out of the observations made in accordance with them, the science of astronomy took its origin.[1]

(28.) And while we must remember how much astronomy owes to astrology, with its natural but erroneous fancies, we must not forget that in every nation of olden times, the studies of the heavenly bodies was a most important part of religion. Observation of the heavenly orbs signified observance of the gods of heaven. Last, but also highest and purest of all forms of nature-worship, was that which had the dome of heaven for its temple-roof, which saw its deities visibly enthroned there or moving in solemn order above the bowed heads of their worshippers, while all earthly temples were but as altars within that glorious abode of the gods. Cloud and storm might for a time veil those gods from view : but the power of the divine ones manifested itself again and again. Ever the same orbs returned to power ; but the storm-clouds overcome by their might were not restored in the forms they had before had. Night might overcome the sun, but he returned in triumph day after day. The moon might wane, but she was restored to power month after month. The sun as god of the year might succumb to the cold of winter, but at the winter solstice the year-god was born again. His power grew till,

[1] Should any deem such an origin as this less worthy of the noblest of all the sciences than one free from considerations of ambition, greed, craft, or self-seeking in any of its various aspects, let it be remembered that to this day the professional study of astronomy is directed solely to commercial pursuits. The one object with which all our national observatories have been built and are at present maintained, is to advance the interests of commerce, by measuring more carefully year after year the positions of the index-marks on the face of the heavens, and the movements of those bodies which travel over the star-sphere like hands moving athwart some complex dial. It is to no zeal for the study of natural phenomena that national observatories are due now, any more than those observatories which the kings and rulers of Babylonia, Egypt, India, China, and other nations, erected in past ages for the study of the heavenly bodies, were due to interest in pure science. That this is so is shown when appeal is made by certain modern mendicant orders for the alms which they demand in the name of the endowment of research ; for, like the Egyptians of old and the gipsies of to-day, they accompany their appeal always by a promise that the hand which crosses theirs with gold shall receive vast benefits from their researches in return. The promise is false, but the fact that it is made is not the less significant.

at the vernal equinox, he rose out of the tomb (pictured in the southern or under half of the star-sphere), and advancing thence in right ascension (as astronomers still describe his motion, borrowing from the religious phraseology of more ancient days) passed to his full summer glory as ruler of the heavenly host.[1]

(29.) In such movements and changes among the orbs of heaven, men recognised no mere sequence of phenomena. They saw, as they imagined, clear evidence of independent will, of overruling power. They feared, and therefore they worshipped, the heavenly bodies, watching for all the various indications of approaching phenomena presented in the heavens, preparing to 'blow up the trumpet in the new moon,' to offer sacrifice to the new-born son of the year, when the heliacal rising of his herald star had been observed by their astronomer-priests or magi, to time his passing over the equator, alike when ascending towards his summer glory and descending towards his wintry tomb. We need observe only, how astronomical relations find their way (to this day) into the determination of religious festivals,[2] and to consider

[1] Multitudes of solar myths attest, amon all nations, the prevalence of ancient sun worship. The Greeks had their Herakles, the Persians their Mithras, the Egyptians their Osiris and Horus. Even the Hebrew records present solar myths, though chiefly borrowed. For instance, we have, in Samson, the Babylonian sun-god Shamash. Shorn of his rays by the cold mists of the departing year (Delilah, or the Languishing One), he is so weakened that his enemies, the Cloud-foes he had so often overcome, blind him and enclose him as in a prison. But he gathers strength in his wintry prison, and at length destroys his enemies yet again, slaying more at his death (the end of the sun-god's yearly career only) than in the days of his full strength.

In the Hebrew, Samson is Shimshon. Our bibles, in the multitude of their marginal notes, seem a little chary of the true interpretation of this name, which means 'glowing sun.' In this particular case, the Hebrews got the right equivalent for the Babylonian name they had found in a borrowed story. Usually some similarity of sound misled them, as in the well-known case of Babel, really 'gate of God,' confounded by the Hebrews with Balal, to confound.

Other solar myths are scattered through the Hebrew records. It is as easy to recognise the history of Oannes, the wintry sun-god, in the story of Jonah, as that of Shamash, the summer sun-god, in the story of Samson.

[2] There can be very little doubt that the *timing* of all the festivals, not only of Jewish, Buddhist, and Mahomedan ceremonial, but also of Christian worship, had an astronomical origin. The astronomical priests or magi announced the birth of the sun as god of the year when they first saw a particular star rising heliacally—that is, just visible at its rising but presently lost in the glory of growing day. This indicated the birth of the sun-god in the cave of winter. The forty days of Lent correspond to the forty days of watching before the sun-god passed over the equator, rising to his summer reign. Then followed forty days of watch as he advanced in right ascension till the time corresponding to our Ascension Day, when he entered the region of his glory on passing the circle fifteen degrees above the equator : and so on through the year.

This has been partly lost through the later substitution of a time roughly indicated by the moon for the true day of the sun's equinoctial passage—the passover or equatorial crossing of the sun-god. Originally, the astrological priests independently determined this time—the crucifying followed by the resurrection of the sun-god of their worship—just as they independently determined the day of his birth about December 25, and the corresponding days for the moon. We have traces of this more careful usage in Lady Day, March 25 ; John Baptist's Day, June 24 ; and Michaelmas Day, September 29. According to this usage, May Day is the true or astronomical Ascension Day ; and it is noteworthy that the ancient manner of celebrating both May Day and Ascension Day corresponded with the solar character thus assigned to the festival. [More on this in dealing with astronomical ascension.]

how unlikely this would have been had not the celestial bodies been themselves the objects of religious adoration in the old times when the various festivals came into existence, to see how universal must once have been the worship of the heavenly host. Men had been so long accustomed to see their temples turned towards the place of sunrise, to have their festivals and fasts ruled by the sun and moon, their weeks numbered by the seven planets and measured by lunar movements, that even when they purified their religions of all actual worship of the heavenly bodies, they could not get rid of the old ordering of the fasts and feasts, of the symbolisations belonging to the old system, or even of many of the myths whose original meaning is now known to have been purely Sabaistic. The rejection of sun-worship and planet-worship, however, should not blind us to the interesting significance of the retention of the times and seasons, the forms and symbols, of the old religion. We cannot rightly apprehend either what ancient races achieved, or what they failed to achieve, in astronomy, we cannot rightly understand the edifices they erected or the labours they undertook in pursuance of astronomical observations, unless we perceive and admit that, among them all, astronomy was the servant of astrology, and astrology the high-priest of religion.

(30.) Let us consider how the heavenly temple appeared to the observer of old, reverent because considering the heavenly orbs as deities, earnest in observing them because he supposed that in their movements his own fortunes and the fortunes of his race might be read and the will of the heavenly powers was recorded.

There was one region of the heavens where all the objects of his veneration attained their highest position and whence, he assumed, they exerted their chief influence. Sun, moon, and planets alike, in their course day after day above the horizon, attained their greatest glory in the south. Facing this portion of the heavenly temple, the observer had on his left the region of the horizon where the heavenly bodies rose, and on his right the region where they set. Behind him was the part of the horizon beneath which he believed the sun and moon and planets passed to the lowest portions of their diurnal circuit. On the left, then, the heavenly orbs were in their *ascendant*: towards the south at their *culmination*; towards the west *decadent*; and towards the north in the tomb of darkness. To this day, expressions derived from the supposed influences of the celestial orbs in these several positions, remain in use among races which have long since rejected all belief in astrology—so widely ranging, so long lasting, and so potent while it lasted, was the faith of men in that most natural and, in a sense, most reasonable of superstitions.

(31.) What men recognised then in the movements of the heavenly bodies is true now and true for all time. And even in dealing with the limited knowledge and the imperfect methods of ancient astronomers, we need not hesitate to consider these movements as they are now recognised and understood.[1]

Let us suppose O, fig. 1, the station of an observer around whom can be seen some such small portion of the earth's surface as is within the range of a single station. Beyond is the celestial horizon E.S.W.N., in a plane passing through his station and square to that vertical direction which would be determined by a plumb-line there let fall. Overhead is the dome of the heavens, which, so far as it is determined by the actual positions of visible bodies, like the heavenly orbs, or by the blue of the sky, or by clouds, has in reality no definable shape, but which may be geometrically regarded as a hemisphere around O as centre. This is not, indeed, the natural idea of the observer, to whom the dome of the heavens appears to have a somewhat flattened form, the horizon being between three and four times as far away as the region overhead. And in considering a cloudy sky we have to remember, if we would rightly understand what we see, that usually the curved surface at which we look is much flatter even than this. We mistakenly attribute to it the domed shape of the heavens, whereas in reality its curvature is no greater than the curvature of the earth itself, which under ordinary conditions is imperceptible.

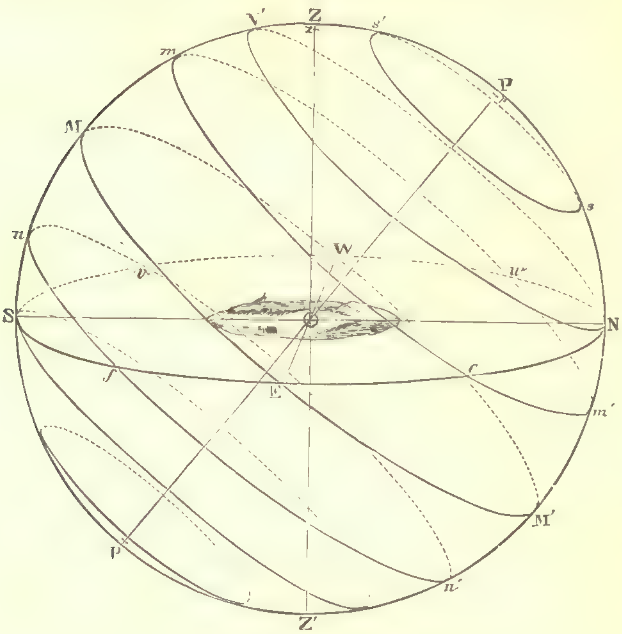


Fig. 1.—The Celestial Sphere.

[1] Yet I may remark in passing there are few researches more instructive or more suggestive than an inquiry into the probable procedure of men examining the celestial phenomena with gradually increasing knowledge, improving instruments, and growing insight. Apart from the thorough grasp of astronomical phenomena which such an inquiry is calculated to give, I believe that the birth and growth of many, if not most of the ideas, customs, familiar superstitions, nay, even religions of ancient times may be more thoroughly understood through such an inquiry as this than in any other way. What light such results may throw on the prevalent ideas even of our own times, those alone know who understand how much of what men regard as the product of advanced thought is in reality but the development of ancient superstition.

But to the astronomer observing the orbs of heaven, differences of distance seem lost in the general effect of the skyscape. For him there is neither the actual flattening of the cloud surface nor the apparent flattening of the sky, but a spherical hemisphere on which the stars appear fixed, and over which the sun, moon, and planets appear to move.

(32.) Standing beneath this hemispherical dome of the universe temple, the observer, watching hour by hour, and day by day, sees all the heavenly bodies without exception carried to the highest part of their career towards the south. The highest point, called the *culmination*, lies on an arch extending from a certain point, S, of his horizon to the point Z overhead, his *zenith*, and thence—always in one vertical plane—to a point N directly opposite S.

Thus the sun, if he rises at E on the horizon, midway between N and S, passes to his highest point at M on the arc S Z, and thence to W, midway between S and N on the part of the horizon opposite to E. If he rises at *e*, between E and N, he is carried to his highest at *m* on S Z, and thence to *w*, on the arc S W N, as far from N as *e*. If he rises at *f* between E and S, he is carried to his highest at N on S Z, and thence to *v* on the arc S W, as far from S as *f*. If a star rises at E or *e* or *f* its course is similar to the diurnal course of the sun, after he has risen from these points respectively. If a star rises anywhere on the arc E N it reaches its highest point on the arc S Z, somewhere between M and N, according to its rising place. This highest point is on the same side of Z as S, if the observer's station is at such a place on the earth as London, for which Fig. 1 is drawn: but elsewhere, as will presently appear, the place to which a star rises highest from N may be on the same side of Z as N. These stars return to the horizon between W and N, each setting at a point as far from N as its rising point. Other stars, not rising or setting, but passing from the lowest point, along P N,[1] of a complete circuit, are carried thence round to its highest point, on the arc P Z N', and so back to their starting-place on P N, so moving as to be always at the same distance from P, and continually above the horizon.

(33.) These movements, more and more carefully observed, were recognised more and more clearly as absolutely regular so far as the stars are concerned, and so far regular in regard to the sun and moon and planets that the arc S Z P N always includes the place of culmination. The movements of the stars, again, were found to be absolutely uniform, and in parallel circles,

[1] The mathematician will not fail to notice that, as usual with the picture illustrating the celestial sphere, fig. 1 is incorrectly drawn. The zenith ought not to be where shown at Z, nor ought Z O N to be a right angle. The zenith should be on an elliptical arc from Z to E and thence to Z', while the *nadir* should be on an elliptical arc from Z' to W, and thence to Z. But a correctly-drawn figure showing the zenith, nadir, poles, &c. correctly placed, would have involved peculiarities which might perplex the learner.

around an axis O P—as uniform as though the stars were nails set in the inside of a hollow globe, turning around an axis P O P′, and carried round by some absolutely uniform mechanism. The same mechanism might be regarded as carrying round the sun, moon, and planets also ; but not as if they were nails in the inside of the great celestial sphere : rather as if they were living beings on that surface, and capable of moving upon it according to certain laws more or less simple, to be determined with more or less accuracy according to the instruments employed in measuring these orbs' positions.

(34.) Hence arose the division of the horizon by the four points, E. the *east*, S. the *south*, W. the *west*, and N. the *north*, determined by such observations as I have described. P, the point towards which the axis of the celestial sphere seems to be directed, is the *pole*, and Z the *zenith*. A point Z′ on the unseen half of the heavenly sphere, opposite Z, is called the *nadir*.

(35.) I pause here for a moment to note that all the peculiarities which the earliest observers of the heavens noticed can be observed—and I think should be observed—by the student of astronomy to-day. Only very simple instruments are necessary, such as any ingenious lad could construct in an hour or two :—

For instance, let A B, B C, fig. 2, be two rods at right angles, and of such length that a line from C to A may be inclined at an angle of $51\frac{1}{2}$ degrees to A B for an observer in London (elsewhere he must ascertain the latitude of his place, which he can do from any atlas, and make the angle C A B to correspond). D E is a cross rod for supporting A B C in an upright position. F G is a rod revolving easily in the pivot-holes at F and G. H K is a circle of card or other material, marked round its circumference with degree-divisions. L M is a rod turning round on a pivot at O (the dotted figure indicates the nature of its motion) and bearing a small card-circle, M.

Fig. 3.

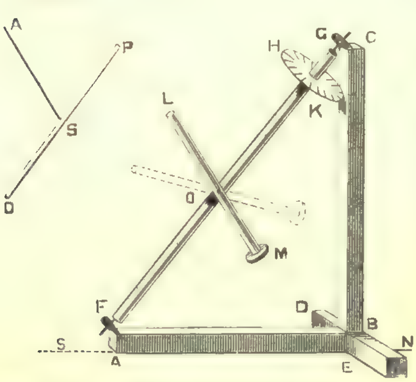


Fig. 2.—A simple instrument for studying the motions of the heavenly bodies.

(36.) Let this little instrument be set upright, A B pointing due north-and-south as shown. Turn the rod F G on its axis until L M, suitably rotated on the pin at O, points to the sun. It will be easy to know when this is the case by observing that when L M is pointing directly towards the sun, L M will throw no shadow on the card-circle M. (It is well to let the pivot at O be long enough to keep L M an inch or so from G F ; when this is done it is

easy to direct L M towards the sun, by observing its shadow and making this disappear.)

(37.) The instrument is now ready for use. We note the division in the card H K which is highest, or else the one which is opposite a suitable mark down the middle of the face G E. An hour later, inspect the instrument, noting that now the rod L throws a shadow on the card M. But do not shift this rod round the pivot O ; turn, instead, the rod F G on its axis. It will be found that by this movement alone the shadow of L can be made to disappear again, and on inspecting the index-card H K, it will be seen that it indicates a rotation through fifteen degrees. At the end of another hour the same amount of rotation must be repeated to make the shadow of L vanish—or, if one may use here the poetical expression employed by the Egyptians in regard to the pyramids, this amount of rotation will make the rod L M 'eat its shadow.'

(38.) In due course the sun sets. Until he is near the horizon there is no change in the amount of rotation ; but when he is very low, the same atmospheric effect which distorts his shape on the horizon, slightly affects the steadiness of his rotational movement ; (for observe : the steady rotation of the rod F G by which we keep L M pointing towards the sun indicates a correspondingly steady rotation of the sun around a heavenly axis coincident in direction with F G.) Taking it, as he assuredly may for granted, that this peculiarity is atmospheric only, amounting to a sort of lifting up of the sun above the horizon by an amount equal at its greatest only to his own diameter, our student of astronomy, whether a clever lad of the nineteenth century or an ancient observer in the days when observation meant reverential observance, may in the night hours carry F G round so that the degree-marks on H K are carried on fifteen degrees per hour. If he does so, or in the early morning hours of the following day gives F G the corresponding amount of rotation, he will find that he has the rod L M again pointing towards the sun. If his little sun-tracker has been very delicately and carefully constructed he may find, especially if his observations have been made in spring or autumn, that the rod L M requires to be slightly moved round the pivot at O to get the right inclination ; but the rotational motion of the rod must remain appreciably uniform by night as well as by day to keep the sun constantly aimed at by L M.

(39.) These observations might readily be continued during a whole year, in the course of which the rod L M would have to be pivoted on O through about the range indicated in the figure, the dotted picture presenting the midwinter position, the other the midsummer position, and the position in spring and autumn being midway, or square to the length of F G. In the course of the year it would be noticed that, though the necessary rotation of H K day by day is appreciably uniform, there is a slight change from day to

day, corresponding to a total range of time by about fifteen minutes on either side of the mean position for absolutely uniform motion ; but such slight peculiarities as these, corresponding to the varying rate of the sun's annual motion, need not at present be considered.

One day's observation of the sun with such an instrument as this teaches more about his diurnal motion than any amount of mere descriptive reading.

The moon can be conveniently observed and the general nature of her monthly movements determined by the use of the same instrument.

(40.) The stars can be observed in the same way by removing the card M and looking along M L at whatever star is selected for observation, or through L M if a tube be substituted for the rod. But for observing the stars an instrument without such supports as E C, A B is more convenient. If the student consider that all he requires is some sort of pointer, as S A, fig. 3, turning on an axis having a fixed position such as O P, and capable of being inclined at any angle to it, he will be able easily to invent convenient ways of supporting this axis and attaching to it a suitable pointer, so as to be able to follow and measure the motion of any star on the heavens. A very convenient plan may be thus indicated. Suppose G F and H K of fig. 2 fixed, F G hollow, an axis running through F G, and projecting out above G. Let a tube or pointer like L M (or A S of fig. 3) be pivoted to this axis, and let the axis, as it turns, carry a pointer travelling round the face H K above G. With such an instrument a star can be readily followed.

(41.) It will be found that each star is carried round with absolute uniformity, its direction as measured from the axis of rotation remaining absolutely unchanged day by day and year by year. But instead of a circuit being effected in 24 hours, which is the average time for a solar circuit, the star circuit, it will be found, is accomplished in about four minutes less.

The planets can be conveniently observed in the same way, and the peculiarities of their planetary motion readily recognised.

(42.) In very ancient times such instruments as these were devised and improved upon, until the movements of the heavenly bodies, being more and more carefully observed and measured, men recognised the phenomena described in Art. 32. They would doubtless have been content with this, or even with much less, so far as the science of astronomy was concerned. But, as I have said, it was chiefly for what they might gain from the orbs of heaven that they made their observations. Recognising in the sun and moon and planets bodies which had great power, and might be made very useful if duly propitiated and watched, they made instruments on a grand and imposing scale, very solid (which was essential to exact observation),

very carefully planned, and so large as to render possible the precise measurement even of small differences or changes of position.

(43.) We have a rough example of an ancient observatory in the Tower of Babel. But although the tower was intended for the observation of the heavenly bodies (and their worship), and was no doubt duly oriented in its original state, yet it was a comparatively rough work, and according to the cylinders of Nebuchadnezzar was unscientifically built, so that rain had penetrated the brickwork, and the terraces of crude brick as well as the burnt brick casing had to be rebuilt. Diodorus Siculus tells us of an observatory-temple at Babylon, built by Semiramis, and dedicated to Belus or Jupiter. Here a college of priests had been instituted by the monarchs of Babylonia. Their temple was this observatory. It was quadrangular, and no doubt its four faces were directed to the cardinal points, like those of the Egyptian pyramids. Observers called out, Diodorus says, when a star was passing (doubtless some kind of transit observation is referred to) and the priests recorded the star's position.

(44.) In the pyramids of Egypt, however, especially in those of Ghizeh, and pre-eminently amongst these in the Great Pyramid, we have examples of buildings which every astronomer perceives to have been built by astronomers and for astronomers. The Egyptologist is possibly right in recognising the chief purpose of all those structures to have been that they should serve as tombs, each as a separate tomb for one tenant only. He is right also, no doubt, for he is so far speaking on a subject which he has especially studied, in saying that, being tombs, they also had a religious significance, and were in point of fact temples. But when in dealing with such an edifice as the Great Pyramid the Egyptologist undertakes (as Lepsius has done) to question the astronomical character of qualities which none but astronomers and skilful ones could have given to those structures, and to deny the astronomical purpose of features as significant to an astronomer's eye as the Great Meridian Circle at Greenwich, he is passing outside the department he has studied, and his opinion is no longer of weight. The astronomer does not need the evidence of Proclus or of Diodorus Siculus[1] to tell him that

[1] We have such evidence, however, and doubtless it is trustworthy. Yet as those writers knew nothing of astronomy, their account is naturally somewhat inexact. Thus we are told by Proclus that the priests observed from the summit of the pyramid, when that structure terminated at the top in a platform. M. Flammarion, in his *Myths of Astronomy*, has a marvellous picture representing certain muscular astronomers perched at the top of a mass of masonry on a surface apparently about eight feet square, while they stare at a comet and a few stars which they could have seen quite as effectively and far more comfortably from the ground. Unquestionably, Proclus must have been referring to a tradition relating to the time when the grand gallery of the Great Pyramid opened out on a large square platform, where priests could be stationed in order to observe and to record observations, timing them carefully, and deducing from them the positions and the movements of the heavenly bodies. Proclus, by the way, gives for the Great Pyramid

at one time the Great Pyramid was an observatory; for some of its instrumental arrangements remain still in existence, every detail of their structure being obviously astronomical in character.

(45.) What arrangements the pyramid astronomers made for extra-meridional observations we do not know, and probably never shall. But the arrangements for observing the heavenly bodies when passing across the meridian were effective in the extreme, and no astronomer can doubt their significance.

(46.) Fig. 4 presents a section of the Great Pyramid taken through the descending and ascending galleries, which all lie in the plane of the meridian. The entrance

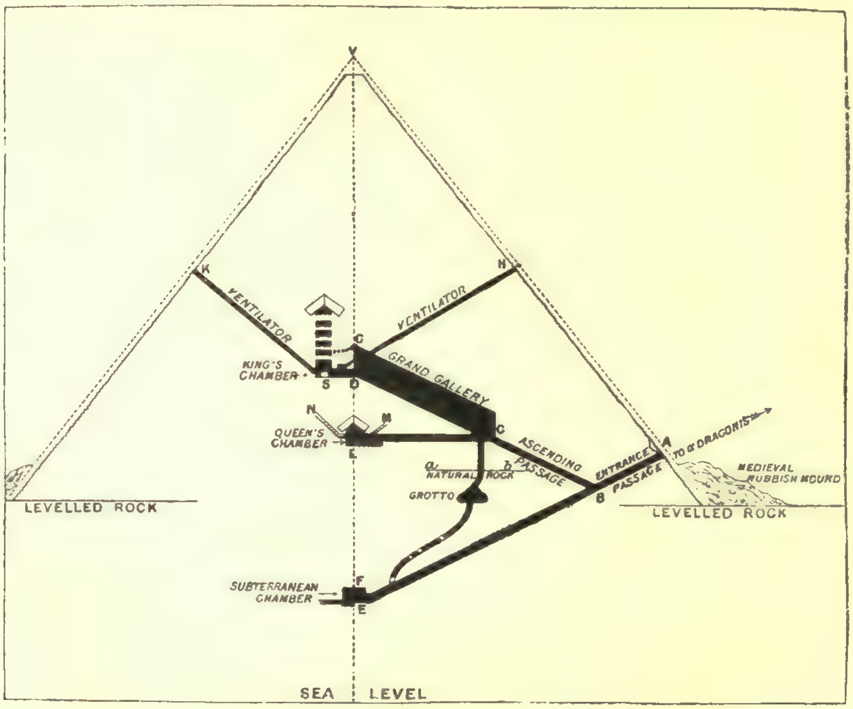


FIG. 4.—South-and-north section of the Great Pyramid, showing its character as an Astronomical Observatory.

passage is the prolongation of a long tunnelling cut into the solid rock to below the middle of the pyramid's base. Herodotus tells us this tunnelling was cut before the pyramid was built; and I have shown in my treatise on the Great Pyramid that in this way only could the pyramid have received its marvellously exact orientation—certainly ten times more exact than the astronomers of Europe in the time before

the date 3,600 years before his time, or say about 3,150 years B.C., a date corresponding well alike with the results of Egyptological research and with the astronomical evidence as to the date of the pyramid. Simplicius, in his commentary on the first book of Aristotle's *De Cœlo*, states that in his time the Egyptians claimed to have made astronomical observations for more than 5,000 years, a claim which the Sakkara pyramids and other tombs of the earlier dynasties appear amply to justify.

the telescope could have attained. This passage was in all some 380 feet in length, and four feet square ; carefully lined, along a portion of its length, with fine granite from the quarries of Assouan (500 miles away). It bore on the pole star of the period, the star Thuban (our θ Draconis) when on the meridian below the pole, and doubtless served for the perfect orientation of the building (used in combination with a plumb-line in the direction L F E) until the level A had been reached. For higher levels a similar ascending passage was taken up from B, southwards, into the very body of the pyramid, the rays of the pole star being doubtless reflected from a water surface temporarily formed at B. Once, perhaps, each year the observation was renewed, a fresh layer of massive stones being added to the growing pyramid yearly during the life of Cheops, according to Lepsius. We have evidence of this process, in the curiously close fitting of the stonework near B (according to Professor P. Smyth 'a secret sign,' but more naturally explained as intended to meet the requirement I have indicated). There is also evidence to the same purpose in the fragments of stone found in the subterranean chamber, as if large blocks, after being used to temporarily stop the passage and hold water at B, were allowed to slide down to E, breaking in their fall.

(47.) Up to C, the internal passages were for constructive purposes alone, or chiefly. But at C begins the finest pre-telescopic transit instrument ever made, the grand gallery from C to G D, bearing directly upon the meridian. It was about 156 feet long (four times the length of the great Rosse telescope), 28 feet high, and in its widest part 6 feet 10 inches wide.

(48.) Fig. 5 is a section of this magnificent transit tube, which was lined with the finest and most beautifully polished stone. It is obvious that vertical sides would have been unsafe. Slant sides, such as are shown in fig. 6, would not have been suited for astronomical observation, as anyone who compares the paths $p_1\ p_2$, $q_1\ q_2$ across the section of sky in the direction of the diurnal motion and the track $P_1\ P_2$ in the direction of planetary or solar motion, as shown in fig. 6 and fig. 5, will at once see. The section in fig. 5 shows the ingenuity with which the builders combined sloping sides, essential to safety, with vertical walls, essential to astronomical accuracy: every part of each side of the section is vertical, yet either side slopes from A B, the narrowest part, to C D, the widest. In watching a star carried in transit by the diurnal motion from p_1 to p_2, from q_1 to q_2, or from r_1 to r_2, the observer had no difficulty, so long as he preserved his distance from the mouth of the gallery unchanged; had the sides been sloping, any change in the height of the observer's eyes, by changing the position of the points on the slope where the star appeared and disappeared, would have affected the accuracy of the result. The observation of the progress of the sun or moon eastwards on the star-sphere day after day, as along the path $P_1\ P_2$, would have been rendered even more unsatisfactory had the sides been sloping. So also would have been the study of the planets—the other five planets, as they were then considered—the keys or interpreters of the will of the gods.

(49.) The interior of the grand gallery is shown as through an imaginary vertical section cutting off one-fourth of its upper portion, in fig. 7. The black sky of night is supposed to be seen through the tube, a long vertical slice of the meridional part of the sky being commanded. The ramps all along either side of the lowest portion of the gallery will be noticed. In each are seen seven out of twenty-eight rectangular openings cut into the stone. I forget what special mystical meaning Professor Smyth finds in the ramps and holes. I am content, myself, with the comparatively common-

place idea that they were intended for the convenience of observers, cross-benches being made with attached blocks at each end. These blocks could be slipped into any two holes facing each other, so that a comfortable and safe thwart seat would be provided, whence an observer (one perhaps of several working simultaneously along different parts of the gallery) could observe the transits of the heavenly bodies.

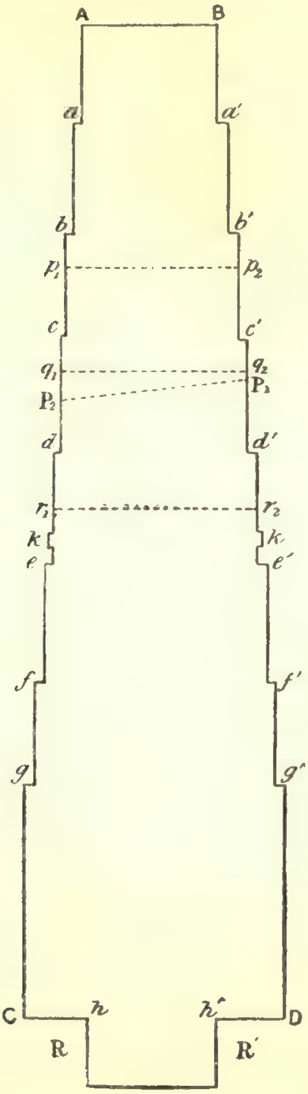


FIG. 5.—East and west section of the Grand Gallery in the Great Pyramid.

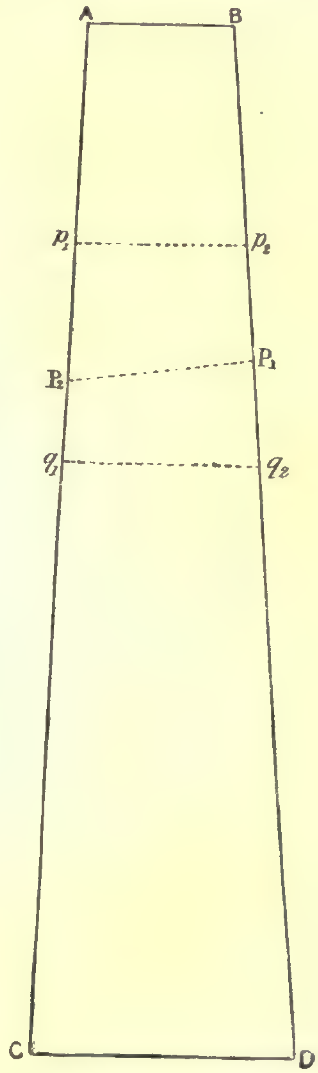


FIG. 6.—Suitable section for the Grand Gallery if not intended for Astronomical Observation.

(50.) In the lower figure of Plate I. the gallery is shown on a larger scale than in fig. 4, by a vertical section through its length, with the smaller ascending passage, and a portion of the descending passage. A horizontal passage leading to a chamber which has been called (but only by the moderns and without assignable reason) the Queen's Chamber, is also shown.[1]

[1] It has been suggested that one chief use of this chamber was for the storage of blocks of stone suitable for plugging the slant passages; a use to which such blocks, apparently provided in

(51.) It will be observed in fig. 3, Plate I. that the layers of the Great Pyramid from the base to the sarcophagus-level number fifty. They are of unequal thickness, and probably designedly, since there are reasons for supposing that the whole work of building proceeded not only under the direct supervision of the astrological priests, but in strict subordination to the supposed indications of the heavenly bodies.

(52.) Fig. 8 represents the appearance which the Great Pyramid would have presented at the time when the grand gallery was completed. The square surface there shown, and not the useless small square depicted by Flammarion, represents the platform mentioned by Proclus, 3,600 years or so later. It will be noticed how carefully the requirements of observatory work were considered, in placing the gallery slightly to one side (the eastern) of the central and vertical axis of the pyramid, so that the central point of the square platform, commanding the angles precisely towards the S.E., S.W., N.W., and N.E., should be free to be occupied by suitable instruments for sweeping the horizon, taking altitudes, and the like. We can understand how the work of observers stationed near this central point could be noted by persons appointed for that purpose, who would also record the moments of the appearance and disappearance of stars in the transit field of the great gallery (fig. 7) as proclaimed by observers situated at different points of that gallery's length.

Fig. 7.—The interior of the Grand Gallery, showing one-fourth of its length; illustrating its Astronomical Character.

considerable numbers during the building of the lower layers of the pyramid, were certainly applied. At last, indeed, the ascending passage was plugged along the greater portion of its length with such stones, all of which had to be removed before entrance could be made to the King's Chamber (see fig 4), where was the sarcophagus (S in both fig. 4 and the third figure of Plate I.), which no doubt eventually held Cheops' body. But *these* plug-blocks at any rate could not have been stored in the Queen's Chamber, being an inch each way—*i.e.* in breadth and height—too large to pass along the horizontal passage. There are reasons for believing that the Queen's Chamber was the sepulchre of Khnumu-Khufu, Khufu's co-regent.

(53.) What means the transit observers had for taking altitudes we cannot now determine. Probably the double groove, *k k*, shown in section, fig. 5, may have been used for carrying sliding frames by which altitudes and possibly true transit epochs too were determined. So far as the time of transit was concerned, there would be no difficulty, as the moments of a star's ingress and egress would, of course, precede and follow the true transit epoch by equal intervals, giving the mid-moment for the moment of transit. Still, in special cases, a vertical line or edge dividing the field of view exactly in half might have been used with advantage. Horizontal cross-lines or edges would give the meridional altitude very satisfactorily. We may even imagine, seeing how ingenious the astronomers of the pyramid manifestly were, and how admirably the grand gallery would have lent itself to such work, that screens may have been

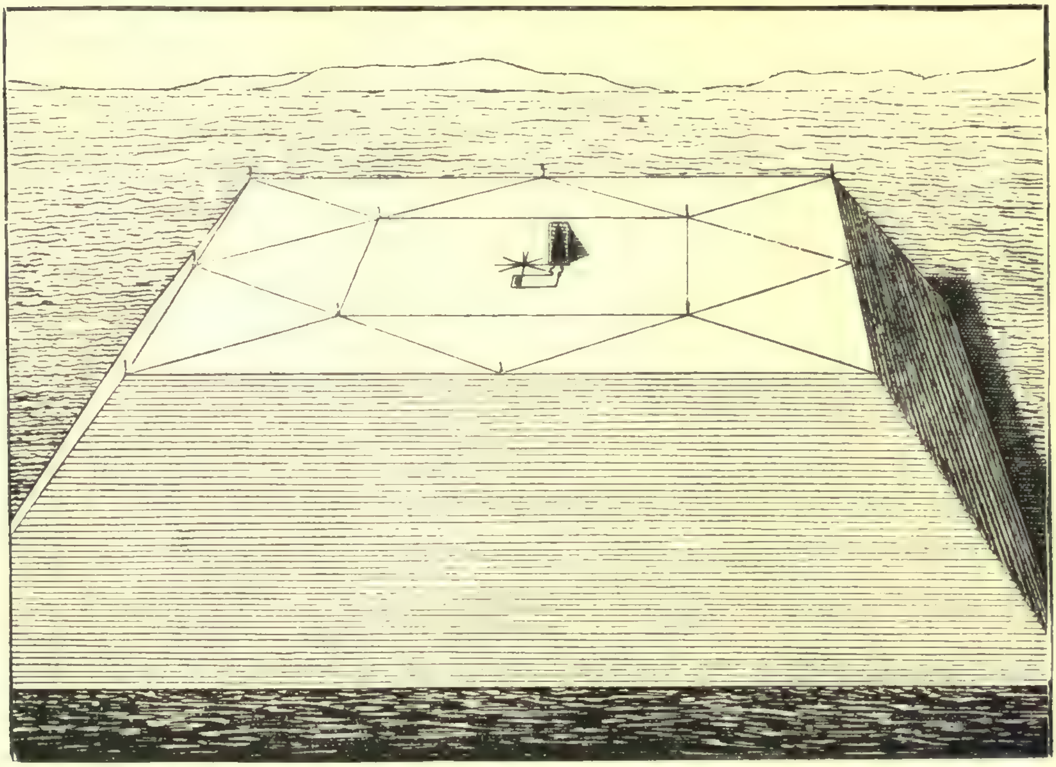


Fig. 8.—The Great Pyramid Observatory.

used for solar observations. By using at the upper end of the gallery an opaque screen with a small aperture (preferably but not necessarily circular) and receiving the sun's light upon a smooth white surface placed at right angles to the sun's direction, a much magnified image of the sun would be formed, on which any spots which chanced to be present on his face would be well shown. The Egyptians were certainly acquainted with the fact that in this way an inverted image of an object can be formed; and if they ever tried the experiment in the case of the sun, especially in the winter months, when, as shown in Plate I., his rays penetrated far down the gallery, they could not have failed to detect the sun-spots.[1]

[1] I have seen large spots myself in this way in an inverted solar image formed at one side of an ordinary sitting-room, by rays admitted through a quarter-inch hole in an opaque blind. The

(54.) The slant of the southern face of the grand gallery in Plate I. seems to correspond with the belief that, on occasion, screens were used in some such way as I have suggested.

(55.) Of course against the whole theory of the pyramid as an observatory, and of the pyramids generally as observatories, may be urged the seemingly overwhelming objections: First, that each pyramid was covered over soon after the death of the king for whom it was built, and the value of the building as an observatory was thus entirely destroyed; and secondly, that if the Great Pyramid was an observatory, the second pyramid, nearly as large, was a mere waste of labour, and all the other pyramids were entirely useless. In reality, these objections, which involve very real difficulties, afford the strongest evidence in support of the theory: in this way, that they destroy every other theory, and are met by this one alone. If the Great Pyramid was a dynastic tomb or a temple, the second pyramid and the others were useless; for the members of the dynasty who ruled together might well be buried together, and might well worship together. But if the Great Pyramid, besides being a tomb and a temple, was also to be used as an observatory for watching the movements of the heavenly bodies, with direct reference to the fortunes of Cheops, it would be useless for all other men (even for his brother, Chephren, or his son, Mycerinus) born under other stars and needing other pyramid horoscopes. Regarding the Great Pyramid in its astrologically religious aspect, there would be something of profanity, to Egyptian eyes, in the employment of an edifice every stone of which had been laid with direct reference to the aspects of the stars influencing Cheops' fortunes, for the observation of the heavenly bodies in regard to the fortunes of Chephren or any other prince. During Cheops' life it was not only worth his while, but worth the nation's while, to go to any expense and employ any amount of labour in the construction of his horoscope temple, in which to conduct religious observances relating specially to him, with the whole amount of devotional energy which could be directed to the task. After his death, perhaps for many years, the observatory-temple might still be used, in solemn memory of Cheops, possibly in the hope that he might ere long be restored alive (if ceremonial observances were duly conducted) to reign with Chephren, Mycerinus, Asychis, and other princes, with their chiefs, ministers, and generals, restored to life amid that marvellous place of star-watched sun-lit tombs. But neither during his life, nor while his memory lasted, would his observatory-temple be of any use save for observances directly relating to his own fortunes. When, in due course,

moon also might have been well observed in this way, and possibly Jupiter and Mars might have been thus seen on the white screen when at their highest. The diameter of the moon, thus observed, supposing the screen a little over 100 feet down the gallery, would be one foot, and supposing the aperture to be half an inch in diameter, many of the details of the moon's surface would be recognisable, though the light would of course be faint. It would be in fact an image of the moon painted in moonlight (such as we see it on a white surface at night) as with a pencil half an inch broad at the point. Supposing all other light shut out, the image would be distinct enough. Indeed, the light-pencils might be somewhat reduced. Of course, in the case of the sun, a much smaller opening would suffice, especially if all extraneous light were carefully excluded. The diameter of Jupiter, estimated for a point-like aperture, would be under favourable conditions about one quarter of an inch at the distance which would make the sun's or the moon's diameter one foot; and it is hardly necessary to say that the circular form of Jupiter could not possibly be recognised. Barely could his light be seen, and that only by a very careful darkening of the gallery—even starlight being excluded.

his pyramid was completed, the sacred observatory, with all its instruments, was entombed as surely as the body of Egypt's erst powerful monarch.[1]

(56.) It may be noticed as agreeing with this interpretation, and this alone, of the Great Pyramid, that the other pyramids were severally designed to correspond with the importance of their future tenants during the life of Cheops himself. Had they been erected for tombs by the several princes who eventually occupied them, they would have been more nearly of the same size. For in actual life these princes, as they came to manhood, held positions much more nearly comparable with the position of Cheops himself than one would infer from their smaller pyramids. Doubtless the whole set of pyramids, as well as the massively constructed and carefully oriented tombs of the chief priests and ministers in the closely adjacent cemetery, were planned as a national work, in the hope that in due course the dead kings, princes, and leaders of the people would be restored to renewed life and power.[2]

(57.) I have been careful to indicate the character of the Great Pyramid as an observatory, not only or chiefly because of its interest so viewed, in the information it conveys as to the marvellous skill and accuracy of the astronomers of ancient Egypt, but because light is thrown on many problems of great importance in regard to the past—aye, and the present too--of the human race, so soon as we recognise that the observation of the heavenly bodies was a matter of religion with the nations of old. We fondly imagine an origin for the religious ideas of to-day absolutely free from all the superstitions of astrology and star-worship. But these superstitions were too wide-spread, they were too intimately associated with the whole life of each nation and each individual, and they lasted too long, to have been thus evanescent in their effects. To this day, the language and the symbols, the fasts and the festivals, nay, the very doctrines of the ancient star-worshippers are extant, in the midst of nations who would reject with horror all idea of substituting the worship of created things, even the most glorious, for the worship of the Creator.

(58.) Let two examples, not taken—as examples might be taken—from our own

[1] To this day the horoscope of the Parsee (the living representative of Sabaistic worship) is sacred, to be preserved religiously by the person for whom it has been calculated, to be burned to ashes on his death, and to be then thrown on the waters of the Sacred River.

[2] I would earnestly recommend to the attention of all who are interested in the Great Pyramid, that fine work, *The Pyramid and Temples of Gizeh*, by W. M. Flinders Petrie. Apart from its great value in other respects, it contains just such information about the pyramids, and especially about the pyramid of Cheops, as was required for the testing of those views which I have advanced in my book on the Great Pyramid. I had hoped to have been able myself to make such special study of the pyramid as I felt to be desirable; but could neither obtain the necessary time nor spare the expense which the work would have involved—much more, be it remarked, than the hundred pounds advanced to Mr. Petrie by the Royal Society. After delaying the publication of my work some time, in the hope that after all I might be able to study the pyramid as I wished, I decided that the evidence collected by others was presumably trustworthy, and that at any rate the astronomical evidence on which my theory was based must be regarded as ample. In the meantime, unknown to me, the evidence I so much desired was being collected by others. That it confirms my views in all respects, even in details and in ways scarcely expected, does not greatly surprise me; for my views had been very cautiously reasoned out. But it is natural that I should be gratified at the result. There are passages in Mr. Petrie's thoroughly excellent work which I might suppose to have been written specially to advocate my views, did I not know that Mr. Petrie could have heard nothing about these. I may remark that my theory explains several matters which Mr. Petrie found perplexing; and gives an interesting meaning to some relations which he deals with without recognising their importance. This I may show in full detail hereafter.

time or from among European nations, be cited to show how long the old astrological ideas remained, and how those who retained them were ready to erect structures, at great cost of labour and money, for the reverential observance of the orbs of heaven.

(59.) Fig. 9 represents an observatory at Benares, said to have been erected by the Emperor Ackbar. Sir Robert Barker, in the 'Phil. Trans.' vol. lxii. p. 598, mentions the following particulars of this curious collection of stone instruments.

(60.) 'I made inquiry when at Benares in the year 1772, among the principal Bramins, to endeavour to get some information relative to the manner in which they were acquainted of (*sic*) an approaching eclipse. The most intelligent that I could meet with, however, gave me but little satisfaction. I was told, that these matters were

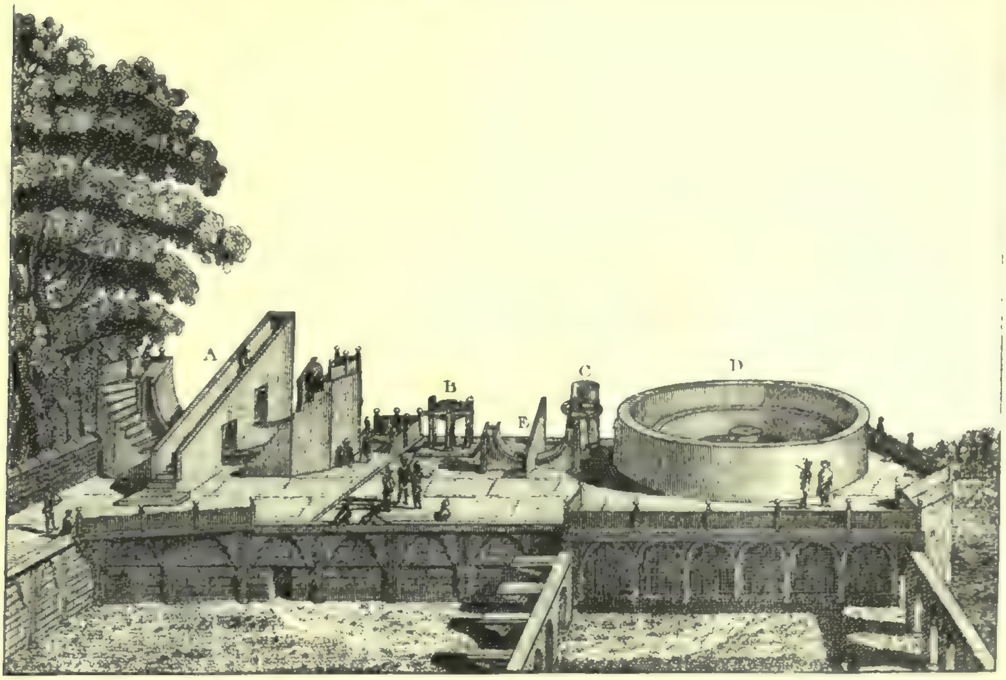


Fig. 9.—Ancient Observatory of the Brahmins at Benares.

confined to a few, who were in possession of certain books and records; some containing the mysteries of their religion, and others the tables of astronomical observations, written in the Sanskirrit language (*sic*), which few understood but themselves: that they would take me to a place which had been constructed for the purpose of making such observation as I was inquiring after, and from whence they supposed the learned Bramins made theirs. I was then conducted to an ancient building of stone, the lower part of which, in its present situation, was converted into a stable for horses, and a receptacle for lumber; but, by the number of court-yards and apartments, it appeared that it must once have been an edifice for the use of some public body of people. We entered this building, and went up a stair-case to the top of a part of it, near to the river Ganges, that led to a large terrace, where, to my surprise and satisfaction, I saw a number of instruments yet remaining, in the greatest preservation, stupendously large, immovable from the spot, and built of stone, some of them being upwards of twenty feet in height; and although they are said to have been erected

200 years ago, the graduations and divisions on the several arcs appeared as well cut and as accurately divided as if they had been the performance of a modern artist. The execution in the construction of these instruments exhibited a mathematical exactness in the fixing, bearing, and fitting of the several parts, in the necessary and sufficient supports to the very large stones that composed them, and in the joining and fastening each into the other by means of lead and iron.

(61.) 'The situation of the two large quadrants of the instrument marked A in the plate, whose radius is nine feet two inches, by their being at right angles with a gnomon at twenty-five degrees elevation, are thrown into such an oblique situation as to render them the most difficult, not only to construct of such a magnitude, but to secure in their position for so long a period, and affords a striking instance of the ability of the architect in their construction: for, by the shadow of the gnomon thrown on the quadrants, they do not appear to have altered in the least from their original position; and so true is the line of the gnomon, that, by applying the eye to a small iron ring of an inch diameter at one end, the sight is carried, through three others of the same dimensions, to the extremity at the other end, distant 38 feet 8 inches, without obstruction; such is the firmness and art with which this instrument has been executed.

(62.) 'Lieutenant-Colonel Archibald Campbell, at that time chief engineer in the East India Company's service at Bengal, made a perspective drawing of the whole of the apparatus that could be brought within his eye at one view; but I lament he could not represent some very large quadrants, whose radii were about twenty feet, they being on the side from whence he took his drawing. Their description, however, is, that they are exact quarters of circles of different radii, the largest of which I judged to be 20 feet, constructed very exactly on the sides of stone-walls built perpendicular, and situated, I suppose, in the meridian of the place; a brass pin is fixed at the centre or angle of the quadrant, from whence, the Bramin informed me, they stretched a wire to the circumference when an observation was to be made; from which, it occurred to me, the observer must have moved his eye up or down the circumference, by means of a ladder or some such contrivance, to raise and lower himself, until he had discovered the altitude of any of the heavenly bodies in their passage over the meridian, so expressed on the arcs of these quadrants: these arcs were very exactly divided into nine large sections; each of which again (*sic*) into ten, making ninety lesser divisions or degrees; and those also into twenty, expressing three minutes each, of about two-tenths of an inch asunder; so that it is probable they had some method of dividing even these into more minute divisions at the time of observation.

(63.) 'My time would only permit me to take down the particular dimensions of the most capital instrument, or the greater equinoctial sun-dial, represented by fig. A, which appears to be an instrument to express solar time by the shadow of a gnomon upon two quadrants, one situated to the east, and the other to the west of it; and, indeed, the chief part of their instruments at this place appear to be constructed for the same purpose, except the quadrants, and a brass instrument that will be described hereafter.

(64.) 'Figure B is another instrument for the purpose of determining the exact hour of the day by the shadow of a gnomon, which stands perpendicular to and in the centre of a flat circular stone, supported in an oblique situation by means of four upright stones and a cross-piece; so that the shadow of the gnomon, which is a perpendicular

iron rod, is thrown upon the division of the circle described on the face of the flat circular stone.

(65.) 'Figure C is a brass circle, about two feet diameter, moving vertically upon two pivots between two stone pillars, having an index or hand turning round horizontally on the centre of this circle, which is divided into 360 parts; but there are no counter divisions on the index to subdivide those on the circle. This instrument appears to be made for taking the angle of a star at setting or rising, or for taking the azimuth or amplitude of the sun at rising or setting.

(66). 'The use of the instrument, figure D, I was at a loss to account for. It consists of two circular walls; the outer of which is about forty feet diameter, and eight feet high; the wall within about half that height, and appears intended for a place to stand on to observe the divisions on the upper circle of the outer wall, rather than for any other purpose; and yet both circles are divided into 360 degrees, each degree being subdivided into twenty lesser divisions, the same as the quadrants. There is a doorway to pass into the inner circle, and a pillar in the centre, of the same height with the lower circle, having a hole in it, being the centre of both circles, and seems to be a socket for an iron rod to be placed perpendicular into it. The divisions on these, as well as all the other instruments, will bear a nice examination with a pair of compasses.

(67.) 'Figure E is a smaller equinoctial sun-dial, constructed upon the same principle as the large one, A.

(68.) 'I cannot quit this subject without observing that the Bramins, without the assistance of optical glasses, had nevertheless an advantage unexperienced by the observers of more northern climates. The serenity and clearness of the atmosphere in the night time in the East Indies, except at the seasons of changing the monsoons or periodical winds, is difficult to express to those who have not seen it, because we have nothing in comparison to form our ideas upon: it is clear to perfection, a total quietude subsists, and scarcely a cloud is to be seen.'

(69.) Two other observatories were erected in this favourable region of the earth, one at Agra, another at Delhi. The Gentur Muntur (or Royal Observatory) of Delhi was built by Rajah Jeysing, in the reign of Mohammed Shah, about the year 1710. His own account of the objects for which it was erected gives a good idea of the curious mixture of religious and superstitious ideas involved in ancient scientific researches.[1]

(70.) 'Sewai-Jeysing, from the first dawning of reason in his mind, and during its progress towards maturity, was entirely devoted to the study of mathematical science, and the bent of his mind was constantly directed to the solution of its most difficult problems; by the aid of the supreme artificer he obtained a thorough knowledge of its principles and rules. He found that the calculation of the places of the stars, as obtained from the tables in common use, gives them widely different from those determined by observation: especially the appearance of the new moons. Seeing that very important affairs both regarding religion and the administration of empire depend upon these; and that in the time of the rising and setting of the planets, and

[1] See 'Some Account of the Astronomical Labours of Jayascinha, Rajah of Ambhere, or Jayanaga,' by W. Hunter, Esq., in *Asiatic Researches*. Probably the Delhi Observatory was constructed before that at Benares, notwithstanding the antiquity assigned to the latter by the Brahmins. For Jeysing states that he caused other observatories to be erected at Benares, Jeypore, Matra, &c., to test the results he had obtained at Delhi.

the seasons of eclipses of the sun and moon, many considerable disagreements of a similar nature were found;' he represented the matter to Mohammed Shah, 'the majesty of dignity and power, the sun of the firmament of felicity and dominion, the splendour of the forehead of imperial magnificence, the unrivalled pearl of the sea of sovereignty, the incomparably brightest star of the heaven of empire; whose standard is the Sun, whose retinue the Moon; whose lance is Mars, and his pen like Mercury; with attendants like Venus; whose threshold is the sky, whose signet is Jupiter, whose sentinel Saturn; the emperor descended from a long line of kings; an Alexander in dignity; the shadow of God; the victorious king Mohammed Shah: may he ever be triumphant in battle!' A reply was given to his representation. 'Since you who are learned in the mysteries of science, have a perfect knowledge of this matter,

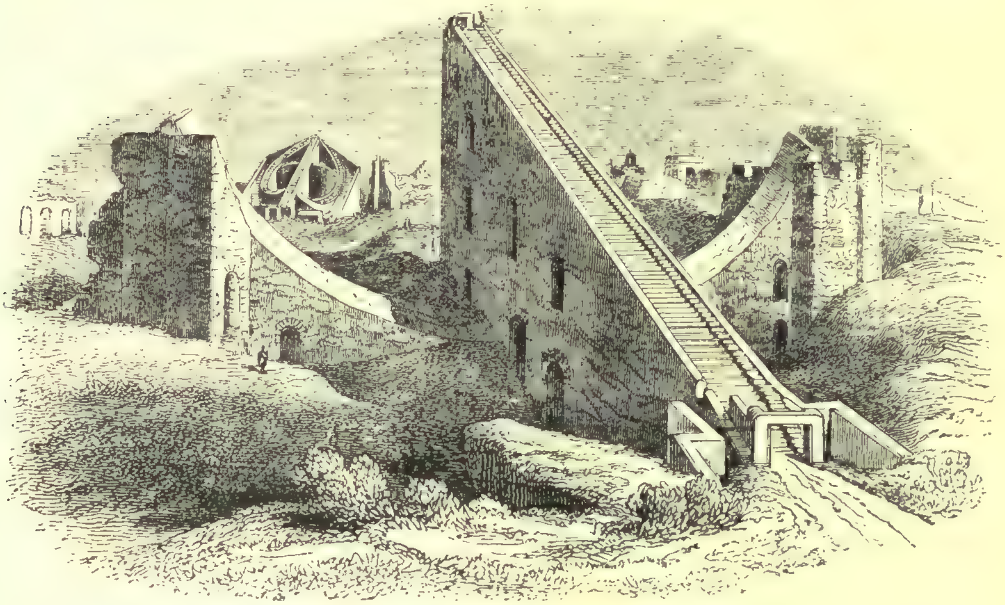


FIG. 10.—Ancient Royal Observatory of Delhi.

having assembled the astronomers and geometricians of the faith of Islam, and the Bramins and Pundits, and the astronomers of Europe, and having prepared all the apparatus of an observatory, do you so labour for the ascertaining of the point in question, that the disagreement between the calculated times of those phenomena and the times in which they are observed to happen may be rectified.' Jeysing justly remarks that this was a 'mighty task,' but having 'bound the girdle of resolution about the loins of his soul,' he constructed several of the instruments of an observatory in brass. 'But finding that brass instruments did not come up to the ideas which he had formed of accuracy, because of the smallness of the size, the want of division into minutes, the shaking and wearing of their axes, the displacement of the centre of the circles, and the shifting of the planes of the instruments,' he erected the existing great works 'of stone and lime, of perfect stability, with attention to the rules of geometry, and adjustments to the meridian, and to the latitude of the place.'

(71.) 'The observatory consists of several detached structures. The first is a large equatorial dial, tolerably entire in its outline, but the edges of the gnomon and of the

circle on which the degrees were marked are broken in several places. The gnomon measures above 118 feet; its base (each side) is computed at 104 feet, and its perpendicular height at nearly 57 feet. This instrument Jeysing speaks of as the 'prince of dials.' It is built of stone; but the edges of the gnomon and of the arches where the graduation was, were of white marble, little of which remains. Another equatorial dial, smaller and of different construction, stands at a little distance, in a very perfect state. The gnomon, which stands in the middle, contains a staircase leading up to its top, and on either side of the gnomon are concentric semicircles, having a certain inclination to the horizon. They represent meridians, removed by a certain angle from the meridian of the place. On each side of this part is another gnomon, of equal size to that last mentioned. The north wall of this structure connects the three gnomons at their highest end; and on this wall is described a graduated semicircle for taking the altitudes of bodies that lie due east or west from the observer. A double quadrant is described on a wall to the westward of the building. South of the great dial are two buildings, apparently exactly resembling each other, and adapted for the same purpose—the observation of the altitude and azimuth of the heavenly bodies. They are circular, with a pillar in the centre of each, rising to the top, which is open. From this pillar, at the height of about three feet, branch horizontal radii of stone to the circular wall. The radii are thirty in number: the spaces between them are equal to the radii, which increase in breadth as they recede from the pillar. In the wall, at the spaces between the radii, are recesses with holes, to enable a person to climb to the top, and containing each of them two windows. On the edges of the recesses are marked the degrees of the sun's altitude, as shown by the shadow of the pillar. The degrees are again subdivided into minutes. The spaces in the wall are divided into six equal parts or degrees, by lines drawn from the top to the bottom. By observing on which of these the shadow of the pillar falls, the sun's azimuth is determined. The altitude and azimuth of the moon and of a star may also be found by means of this erection. The circumference of the building is 172 feet 6 inches; of the pillar, 17 feet; length of each of the radii, 24 feet 6 inches. The height is not stated. Mr. Hunter remarks: 'I do not see how observations can be made when the shadow falls on the spaces between the stone radii or sectors; and from reflecting on them, I am inclined to think that the two instruments, instead of being duplicate, may be supplementary one to the other; the sectors in one corresponding to the vacant spaces in the other, so that in one or other an observation of any body visible above the horizon might at any time be made.' Between these two buildings and the great equatorial dial is an instrument, formed of mahogany, in the shape of a concave hemispherical surface, to represent the inferior hemisphere of the heavens. It is divided by six ribs of solid work and as many hollow spaces, the edges of which represent meridians, at the distance of 15 degrees from each other. The diameter of this work is 27 feet 5 inches.'

(72). We have no means of knowing in what degree astronomy was advanced by work done with such contrivances for observation as the pyramid passages and galleries, or by such great gnomons, shafts, walls, and the like, as were erected in India. We may be assured, however, that when building up these instruments (for such they were), men hoped to determine the positions and track the movements of the heavenly bodies, trusting in the size, weight, and rigidity of these structures to give accuracy to observations made by their means. Quite possibly the knowledge possessed by Egyptian

and Chaldæan astronomers long before the days of Hipparchus was chiefly due to observing appliances of this kind. More probably, however, the skilful and ingenious astronomers of Egypt and Chaldæa early recognised the advantage of using less massive and unmanageable instruments (so to call their great gnomons, tunnellings, and so forth), and did the best of their observing work with instruments akin to the astrolabes, quadrants, and parallactic rods employed by Hipparchus and Ptolemy. We may well believe, in fact, that as Hipparchus borrowed most of his astronomy from the Egyptians and Chaldæans, so also he borrowed from them the plans for the various instruments he employed in observing the heavenly bodies.

(73.) Many forms of astrolabe, or instrument (as the name implies) for 'taking the stars,' were invented by ancient astronomers. In some, the chief object aimed at was to determine the longitude and latitude of the celestial orbs—in other words, their positions in relation to the great circle in which the sun appears to travel round the celestial sphere. In others, observations more nearly akin to those made by modern astronomers were aimed at, the distances of the heavenly bodies from the poles and therefore from the great circle midway between them (the *celestial equator*) and their movements real and apparent around the polar axis, being the details which the observers sought to determine.[1] In yet others, the bearings of the celestial orbs with respect to the horizon and their altitudes above the horizon were determined. While lastly, observations akin to those made by means of our modern transit instruments were made by meridional circles, that is circles set in the plane of the meridian (their plane vertical and north-and-south) so that a pointer extending from rim to rim of one of these circles and passing athwart the centre would be directed upon the meridian, and would sweep the meridian if carried round a horizontal east-and-west axis passing through the circle's centre.

(74.) A chapter, and a tolerably long one, might be written here, on the ancient observing instruments; but it would occupy space which is required for other matters. The instruments formed by great masses of stonework such as I have dealt with at some length, not only have an interest outside their astronomical significance, as bearing on the history, the superstitions, and the customs of ancient races, but we know that they are the very instruments which ancient astronomers employed. The smaller instruments used by Chaldæan and Egyptian astronomers may have done more useful and more permanent work, little permanent though they were in themselves; but we have no evidence as to their exact nature. Their armillary (or braceleted) spheres with embracing circles situate so as to correspond with the meridian, the horizon, the equator, and the prime vertical (or east-and-west circle passing through the zenith) have long since perished. Of their astrolabes we can form no better idea than may be derived from Tycho Brahe's astrolabes, modelled on Ptolemy's descriptions of astrolabes such as Hipparchus almost certainly used, and such as he probably formed on methods derived from a much older astronomy than his own.

(75.) Let Figs. 11 and 12 from Tycho Brahe's *Astronomiæ Instauratæ Mecanica*

[1] Such instruments were called 'parallactic astrolabes'; but the term is a misnomer. Ptolemy's parallactic instrument, devised for determining the moon's parallax, was not akin to the equatorial astrolabe in any respect. Lalande (*Astronomie*, § 2278) points out that the proper word for an equatorial would be *parallatique* (not *parallactique*), because such an instrument follows stars along their parallels (of declination). We must suppose that the incorrect use of 'parallactic' has resulted from that strange fondness for long words of classical origin which persons who know little or nothing of the dead languages curiously affect.

suffice to illustrate the movable instruments employed by astronomers in pre-telescopic days.

(76.) Fig. 11 represents an instrument which could manifestly be used both for meridional and extra-meridional observations. The outer circle S Z N P′ represents the meridian, P P′ being the polar axis. (From the degree-divisions marked round the circle it will be seen that the instrument has been adjusted for about latitude 57°; for the latitudes of Egypt, Babylon, and Greece, P would have to be brought much nearer N.) To the polar axis PP′ the hour-circle A H B H′ is attached, and carries with it, when rotated on the polar pivots, the equatorial circle *e*H*e*′H′, which slides frictionally through grooves cut in the fixed meridian circle, at *e* and *e*′. On the hour-circle are the movable sights A and B; on the equator the movable sights F and G. At C, the centre of the axis, a small cylinder projects at right angles to the plane of the hour-circle. Lastly, there is a plumbline Z L to adjust the meridian circle exactly in the vertical; while four screws *s*, *s*′, and *s*″, and another not seen in the drawing, are provided to raise or lower either end or side of the base.

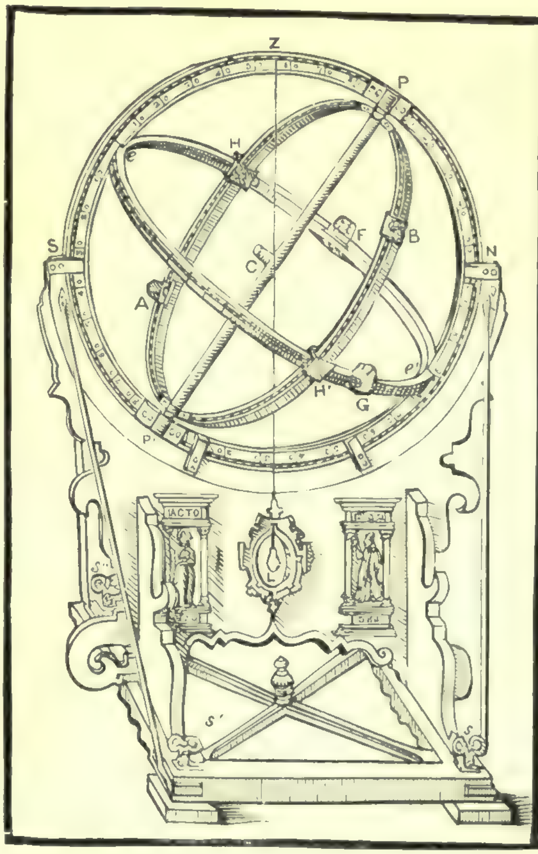


FIG. 11.—Ancient polar Astrolabe, akin to the modern universal Equatorial (having adjustment for latitude).

(77.) It will easily be seen that, by means of this instrument, the position of any heavenly body could be determined, alike as to its distance from the pole or equator, and as to its rotational direction with regard to the axis P P′. The sights on the hour-circle were not used however, as might be supposed, together, diametrally across C, but separately—A for objects north of the equator, and B for objects south of them. To measure the distance of a celestial object from the pole or equator, the hour-circle was first rotated till its plane passed through the star, and then the sight A or B was so moved round its quadrantal arc that the star could be seen both above and below the cylinder C, the orifice of the sight being made just large enough to allow this. Then, of course, the position of the sight on the graduated arc indicated the distance of the celestial object from the pole and from the equator. For instance, if A was the position of the sight in observing a star north of the equator, the arc AH measured the star's distance from the equator, while the arc P′A measured the star's distance from the north pole. If B was the position of the sight in observing a star south of the equator, the arc B H′ measured the star's distance from the equator, while the arc P′B

measured its north polar distance, greater in this case than 90 degrees. To determine the hour-angle or right ascension of a celestial object, the sights F and G were employed. Usually such observations took the form of differential work, the object being to determine the difference of right ascension between two celestial objects, one of them having a known position. Two observers worked simultaneously, one sighting through G, the other through F, each placing his sight so that the object observed was seen on the axis, both on one side and on the other—precisely as, in making polar-distance observations a celestial object was sighted on either side of the cylinder C. This being done, the arc distance between the sights F and G on the graduated equator-circle indicated the difference of right ascension between the two objects.

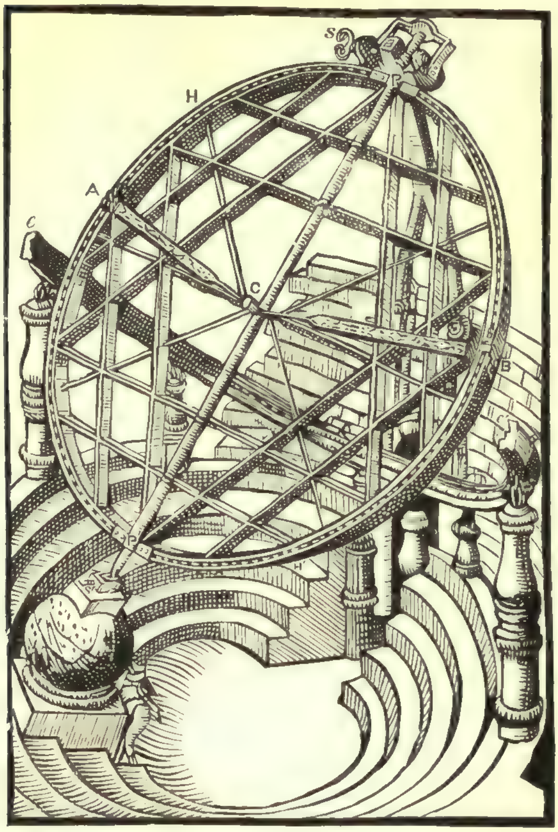


FIG. 12.—Ancient polar Astrolabe, akin in principle to the modern Equatorial.

(78.) It will be easily seen that by bringing the sights B or A to the meridional circle, transit observations, though of rather a rough kind, could be made with this instrument.

(79.) Fig. 12 represents an astrolabe akin in principle to the modern equatorial. An hour-circle H P B P′ rotates with the axis PP′ directed to the poles. At C, the centre of the axis, is a small cylinder as in the other form of the instrument. C A and C B are sights carried on the radial bars C A and C B round the centre C. The distance of a celestial object from the equator or poles, was determined with this astrolabe as with the other, by observations through A or B athwart the central cylinder C. Observations for the hour-angle or right ascension are made by noting where the hour-circle crosses the fixed equatorial circle *e e′*, when situate so that a celestial body is visible through either of the sights. It is evident that some trustworthy time-measurer must have been in use when this instrument was employed, as no provision was made for simultaneous observations, by which alone (where no good clock or other time-measurer is used) the right ascension of a celestial body can be accurately determined.

(80.) Hipparchus and Ptolemy also used, and indeed would seem chiefly to have depended upon, another astrolabe, called by Tycho the Ecliptical Astrolabe (*Armillæ Zodiacales*, the Zodiacal Rings) the one shown in Fig. 11 being the Equatorial Astrolabe (*Armillæ Æquatoriæ*, the Equatorial Rings). It may be described as resembling the equatorial astrolabe with the addition of a graduated circle inclined to the equatorial

circle at an angle equal to the observed inclination of the plane of the sun's path to the equator. It is evident that by suitably rotating the equator round the polar axis, the ecliptical circle could be brought to coincidence with the real ecliptic, that is, with the plane of the sun's path *at the moment.* Two movable longitude circles pivoting on the movable polar axis of the ecliptic, one outside the other inside, were employed to obtain differential observations by which the longitude of the moon could be determined, that of the sun being supposed known, for while the outer longitude circle was set by the graduations round the ecliptic circle to correspond with the longitude of the sun, the other was turned so that its sights bore on the moon, and thus the difference of the longitudes of the sun and moon was determined, as well as the moon's latitude, precisely as with the equatorial observations for determining polar distance and right ascension, already described. When the longitude of the moon had been determined, the longitude of any star could be obtained by differential observations, as well as its latitude directly. But manifestly the adjustment of the ecliptic plane being true only at the moment for which it was intended, observations with this instrument had to be made promptly, as every minute which passed impaired the ecliptical adjustment.

(81.) We have scant means of determining to what degree of perfection astronomers in ancient times carried their observations with instruments of this sort. Probably the chief difficulty they encountered arose from the want of accurate means of measuring time, without which observations directed to determine position lose a large part of their value. This applies, but not equally, to observations made upon the heavenly bodies when on the meridian and to such bodies observed when off the meridian. Yet the means of correcting time measurements, in some degree, by observing bodies on the meridian, must have led early to the construction of instruments as specially constructed for meridional observation as was the great gallery of Cheops' pyramid itself. We shall recognise further on, the principles on which such instruments are constructed; for the transit instruments and meridian circles of to-day are but developments of those used in days before the telescope was invented. In fact all our telescopic instruments for practical astronomy are but instruments on the old plans with added means for directing them more precisely and ascertaining their directions more correctly than of yore.

(82.) The invention of the transit instrument is usually attributed to Römer, in 1690; but the principle of meridional instruments was much earlier suggested by Tycho Brahé—among the moderns. His mural quadrant was, in fact, a sectional transit instrument.

(83.) The *Mural* or *Tychonic Quadrant* was a quarter-circle of copper, working on the face of a solid wall, in which its axis was fixed. The plane of the wall's face, and therefore of the quadrant, was that of the meridian (in other words, the wall was vertical and stood exactly north-and-south). The quadrant was nine feet in radius, and the *limb*, or curved edge of the instrument, was divided to read to ten seconds of arc, or the 360th part of a degree—about the 200th part of the average apparent diameter of the sun or moon. Tycho showed that by observing any celestial body through the pointers carried round his quadrant (that is, when such a body was on the meridian), and noting the time of the observation, the exact position of the body on

the star-sphere could be determined. But he himself was not content with the time determinations possible in his day, and preferred to use his mural quadrant in taking the midday height of the sun.[1]

(84.) Meridional observations of particular celestial bodies can only be taken at intervals averaging about a day (exactly a sidereal day for a star) and it may happen that the sky is cloudy when the body to be observed is on the meridian ; or the passage of the meridian may occur in the day-time, which for other bodies than the sun and moon renders observation impossible. Hence such instruments as the astrolabe and parallactic instrument were not displaced by meridional instruments even when good clocks made these more effective than they can be without the power of measuring time exactly. Accordingly we find that Tycho Brahé, whose instrumental arrangements represent about the highest development which observational astronomy attained before the telescope, employed extra-meridional instruments. Among these were quadrants which could be moved in azimuth—that is, could sweep the horizon. Of these Tycho possessed several, the largest being six feet in radius.

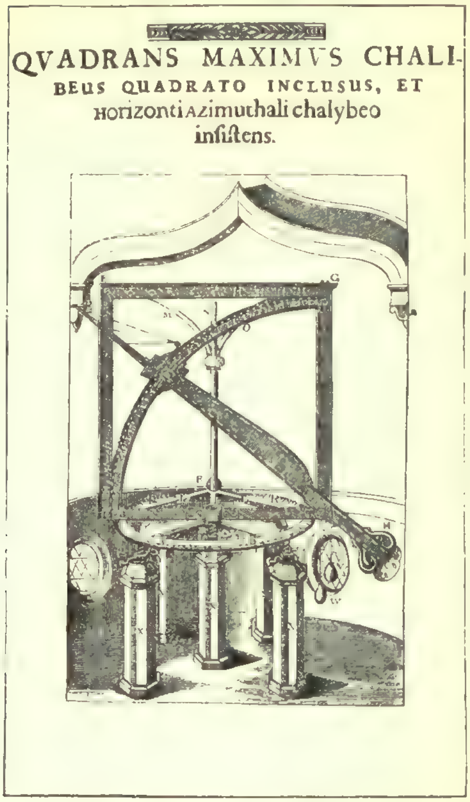


Fig. 13. Tycho Brahé's largest Alt-azimuthal Quadrant, six feet in radius. (Fac-simile.)

(85.) Fig. 13 represents Tycho's largest extra-meridional quadrant. It serves to give a good idea of the way in which observations were made before the telescope was applied to astronomical work :—

The instrument consisted of a steel quadrant, A B C D, having its centre at A, the edge A B horizontal, and the edge A D vertical. The adjustment was obtained by the plumb-lines G V and G W on either side of one edge of the carefully squared frame E F G A enclosing the quadrant, and by the level L L, the screws Y Y being used to secure the exact level. The frame and quadrant turned on the vertical axis N Z so that the vertical plane of the quadrant could be directed towards any point of the horizon. Observations were made by looking through the sight-holes which are shown in the figure near H and C. The part of H C beyond C had a fine edge whose position on the divisions upon the frame-edges E F, F G, indicated the altitude of the body sighted ; while the position of the frame-edge E A on the horizontal divided circle R S indicated the azimuth of the body, that is, its direction with regard to the compass-points. Tycho Brahé's largest quadrant of this class read to quarter-minutes of arc.[2]

(86.) For all such observations good means for time measurement were required,

[1] Astronomiæ Instauratæ Mecanica, No. 4.

[2] According to Professor Smyth, the correctness with which the Great Pyramid was oriented indicates an accuracy of observation tenfold greater than Tycho Brahé's best observations secured.

since the positions of celestial bodies alter with respect both to the horizon and the compass-points from moment to moment.

(87.) The instruments used by Hevelius represented the last attempts of pre-telescopic astronomy to secure accuracy of observation. He used the telescope to obtain views of the heavenly bodies, but he claimed that for determining positions, pin-holes were better than telescopes, and throughout his observing career, he refused the assistance which the magnifying power of the telescope would have given him.

(88.) It has been mentioned that modern observing instruments are but developments of the old instruments. The telescopic power added to them serves to magnify displacements, and so enables the observer to direct his instrument more exactly, and to detect smaller errors and differences of direction than he could otherwise deal with. It also enables him to see objects which could not be seen with the naked eye. But this power no more affects the principles on which the practical observation of the heavenly bodies is conducted, than do improvements in dividing the limbs (or edges) of the instruments, or the microscopical adjuncts employed for reading these divisions.

(89.) To introduce here an account of the invention of the telescope would be an ingenious and easy way of filling up a few pages, if that were needed; but since, as a matter of fact, all the space I have is wanted for matter properly belonging to my subject, I here note simply what the telescope, as used in astronomical observation, actually is and does.

(90.) The principle of the telescope used by Galileo is shown in fig. 14. A B represents a large glass called the *object-glass* because it is towards the object observed, *a b*

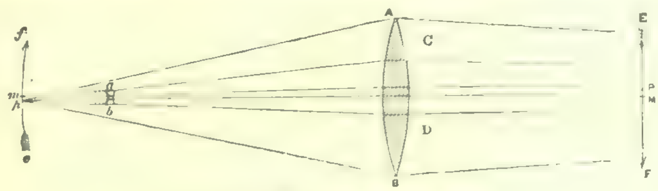


FIG. 14.—The Galilean Telescope.

a small concave glass called the *eye-glass*, because nearest the eye. The tube holding these glasses (set at suitable distance), is directed towards the distant object E M F, which must be supposed at such a distance on the right that the outside lines shown on the right of A B would converge on the point P. The line joining the centres of the eye-glass and object-glass tends to M. But the other lines are supposed to represent rays belonging to a pencil from P, falling on the full object-glass and converging towards *p*, a point in an image of E P F which would be formed at *e p f*, did not the eye-piece *a b* intervene. This eye-piece is so placed that it alters these convergent rays into parallel ones (suitable, therefore, for distinct vision with ordinary eyesight), by which the object is seen as if nearer. For, as Roger Bacon pointed out six centuries ago, the apparent nearness of an object depends on the angle which any part of it subtends to the eye. Now the small part *p m* of the image *f e* manifestly subtends the same angle from the middle of the object-glass A B, as would the corresponding part P M of the object itself. Fig. 15 shows this, in which O represents the centre of the object-glass: we see that the angle *m* O *p* is equal to the angle M O P; but the angle subtended by *mp* from *c* the middle of the eye-glass is greater than the angle *m* O *p* in the same degree that *m* O is greater than *m c*. Since the observer using

this Galilean telescope sees the point P in direction $c\,p'$ on $p\,c$ produced, and the point M in direction cm' on mc produced, and the angle $p'c\,m'$ is equal to the angle pcm, each such part of the object as P M subtends a greater angle than as viewed with the

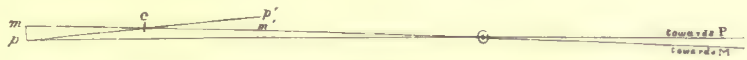


Fig. 15.—Explaining the apparent magnifying of an object or angle by a Galilean Telescope.

naked eye, in the same degree that c O is greater than $c\,p$, or that the focal length of the object-glass is greater than the (negative) focal length of the concave eye-glass.

(91.) But clearly this instrument is not suitable for determining the positions of heavenly bodies. It would not compare favourably even with the pinnules through which celestial objects were viewed before the telescope was invented. The observer could get an object into line with the pinnules through which he saw it, the pinnules and the object being visible together: but in looking through the Galilean telescope, as fig. 15 shows, or as anyone can see who looks through an opera-glass, you cannot at the same time see the object and any nearer defining points or lines. We should want our defining marks set beside $m\,p$, fig. 15, in order to estimate the displacement of P from the centre M, as indicated in the image beside $m\,p$. But the eye looks away from $m\,p$ when the Galilean telescope is employed.

For astronomical observation then, at least in determining position and measuring, it is necessary that the eye should be set so that the image $e\,p\,f$ (fig. 14) shall be viewed in the same direction as the object E P F.

To this end the astronomical telescope was devised, the principle of which is illustrated in fig. 16. Here A B represents the object-glass; $a\,b$, a small convex glass, is the eye-glass. The tube holding these two glasses is directed towards the distant object E M F—supposed at such a distance that the outside lines from A and B towards the right would meet at P. (The line joining the centres of the eye-glass and object-glass is directed along the line m M.) Under these circumstances an inverted image of E M F is formed at $e\,m\,f$. The figure represents the course of a beam or pencil of rays, which, originally diverging from P, has fallen on AB, and thence after refraction through this lens converges to a focus at p. Diverging from p the beam

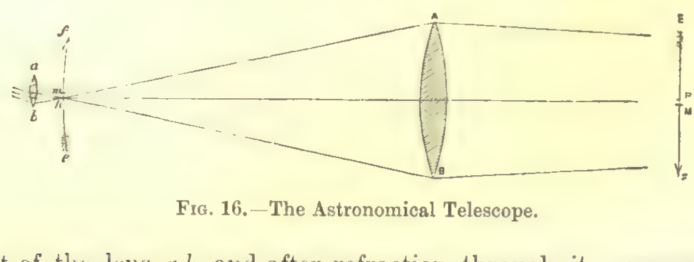


Fig. 16.—The Astronomical Telescope.

falls on a part of the lens $a\,b$, and after refraction through it emerges as a beam of parallel rays (suitable, therefore, for distinct vision by ordinary eyesight) to the eye here placed. For this purpose the lens $a\,b$ must be set so that m is in its principal focus. For distant objects m is in the principal focus of the object-glass AB. The object is magnified as with the Galilean telescope. For the small part $p\,m$, of the image $f\,e$, subtends the same angle from the middle of the object-glass AB, as would the corresponding part PM of the object itself. Fig. 17 shows this, in which O represents the centre of the object-glass: we see that the angle $m\,\mathrm{O}\,p$ is equal to the

angle M O P; but the angle subtended by $m\,p$ as viewed by the eye E through the convex eye-glass, whose centre is supposed to be at c, is equal to the angle $m\,c\,p$,—by the well-known optical property that after a pencil diverging from p has fallen at b on a lens, the axis b E of the emergent pencil is parallel to $p\,c$. And the angle subtended by $m\,p$ from c, the centre of the eye-glass, is greater than the angle m O p,

FIG. 17.—Explaining the apparent magnifying of an object or angle by an Astronomical Telescope.

in the same degree that m O the focal length of the object-glass is greater than $m\,c$ the focal length of the eye-glass.

(92.) This instrument is obviously suitable for determining the positions of the heavenly bodies. For m, (figs. 16 and 17), the middle of the field of the eye-glass can be marked, either by the intersection of cross lines, or in some other way, and when the optical axis of the telescope is directed exactly towards a star, the star will be seen to coincide with the point thus marked. Any divergence of direction will be magnified and so recognised more easily. For example if a star lies towards P, fig. 17, and the telescope is directed towards M, the divergence, which is really measured by the angle P O M or its equal p O m, will appear to the observer's eye to amount to the much greater divergence $p\,c\,m$. The inversion of the image, and therefore of any apparent displacement (considering the astronomical telescope as an instrument for determining direction) is obviously a matter of no moment whatever in astronomical work.

(93.) Regarding the telescope, or rather the combination of an object-glass forming an image at its focus with an eye-glass for examining this image, as simply an aid to the exact determination of direction, we see that all the various methods of observation considered above can be applied advantageously with this special optical contrivance to make them more effective. Without for the moment considering what particular nature an object-glass should have to form the most perfect image, or what kind of eye-glass (or combination, perhaps, of glasses) will best enable the observer to examine that image, we see that if, instead of a small hole at the object end of a rod or pointer, we have an object-glass, while instead of a small hole at the eye end of such a rod we have a convex glass or other magnifier by which to examine the image formed by the object-glass, our means of directing the line of sight and so determining the direction of an object must be improved at least as much as our glasses increase the apparent dimensions of objects observed.

(94.) Of course the telescope was applied, even from its first invention, to magnify remote objects with the idea of recognising their nature more clearly. Galileo, Scheiner, Hevelius, Huyghens, and a host of other astronomers directed telescopes of gradually increasing power towards the sun, the moon, the planets, and the fixed stars, to see what these orbs were like, not to determine their exact directions or movements. Physical astronomy, or the study of the heavenly bodies, regarded as other worlds and other suns, had its beginning at this time—for we can hardly consider the vague inquiries started by some of the old astronomers respecting the nature of the sun and moon as belonging to this the most interesting department of modern astronomical research. It does not appear that the first to use the telescope paid any attention to the use of this instrument as a means of making exact observations of the positions of the heavenly bodies. The Galilean telescope, which has no value for such

work, was not replaced by the astronomical telescope until after 1630, though Kepler had pointed out its advantages as early as 1611, or two years after Galileo had made his first telescopic observations. Scheiner, in his 'Rosa Ursina,' published in 1630, states that he had tried a Keplerian instrument thirteen years before; but the Galilean telescope continued in general use until the middle of the seventeenth century.

(95.) The use of the telescope as an instrument for determining and measuring directions began somewhat earlier than this, probably about the year 1638. The honour of inventing the telescopic method of observation, which is as it were the foundation of modern exact astronomy, must be assigned to Gascoigne, who certainly as early as 1640 had used the telescope as an adjunct to his quadrants and sextants, 'with threads in the common focus of the glasses,' even extending the improvement so far as to use artificial light for illuminating the threads, a method which he found 'very needful when the moon appeareth not, or it is not otherwise light enough.' But it was not until 1667 that this method, revived by Picard and Auzout, came into general use.[1] It should perhaps be mentioned that, as early as 1635 Morin pointed out the advantages of the astronomical telescope for determining positions; but he does not seem to have recognised the advantage of the use of cross-threads at the focus of the object-glass. It is the use of such threads and the power of magnifying and measuring the field of view across which they are stretched which constitutes the most essential feature of the modern astronomical method.

(96.) Although reflecting telescopes are much less used for determining positions than telescopes in which images are formed by refraction through an object-glass (which are therefore called *refracting telescopes*, or simply *refractors*), this seems the right place to mention that the properties of reflection as well as refraction have been enlisted into the service of the astronomical observer. The formation of an image by means of a concave mirror is exhibited in fig. 18, where the points of the real object,

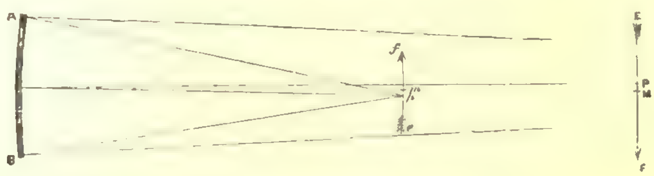


FIG. 18.—The Reflecting Telescope.

and the corresponding points of the image, are lettered as in figs. 14 and 16. Since the observer's head would be placed between the object and the mirror, if the image, formed as in fig. 18, were to be examined through an eye-glass, as in fig. 16, various devices are employed in the construction of reflecting telescopes to avoid the loss of light which would result—a loss which would be important even with the largest mirrors yet constructed. In Gregory's telescope, a small mirror, having its concavity towards the great one, is placed in the axis of the tube and forms an image which is viewed through an aperture in the middle of the great mirror. A similar plan is adopted in Cassegrain's telescope, a small convex mirror replacing the concave one. In Newton's telescope a small inclined-plane reflector is used, which sends the pencil

[1] In 1640 Gascoigne was but nineteen years old. He gave promise of a noble life of scientific and philosophic research. But unfortunately he was killed in the battle of Marston Moor (February 1644). In this one life the human race lost more than the whole race was worth over whose 'divine' rights Englishmen squandered so much blood and property in the seventeenth century.

of light off at right angles to the axis of the tube. In Herschel's largest telescope the great mirror was inclined so that the image was formed at a slight distance from the axis of the telescope. In the first two cases the object is viewed in the usual or direct way, the image being erect in Gregory's and inverted in Cassegrain's. In the third the observer looks through the side of the telescope, seeing an inverted image of the object. In the last the observer saw the object inverted, but not altered as respects right and left. The last-mentioned method of viewing objects was called the front view — apparently because it is the only one in which the observer's back is turned towards the object!

(97.) In all astronomical telescopes, reflecting or refracting, a *real image* of an object is submitted to microscopical examination. The image is affected, however, by certain optical defects; to wit: *curvature*, *indistinctness*, and *false colouring* of the image.

(98.) The curvature of the image is the least important of the three defects named —a fortunate circumstance, since this defect admits neither of remedy nor modification. The image of a distant object, instead of lying in a plane, that is, forming what is technically called a *flat field*, forms part of a spherical surface whose centre is at the centre of the object-glass. This is shown in the curvature of the image *e m f* in figs. 14, 16, and 18; E M F being straight. It follows that the centre of the field of view is somewhat nearer to the eye than are the outer parts of the field. The amount of curvature clearly depends on the extent of the field of view. Thus, if we suppose that the angular extent of the field is about half a degree (a large or low-power field), the centre is nearer than the boundary of the field by about 1-320th part only of the field's diameter.

(99.) The indistinctness of the image is partly due to the obliquity of the pencils which form parts of the image, and partly to what is termed *spherical aberration*. The first cause cannot be modified by the optician's skill, and is not important when the field of view is small. Spherical aberration causes those parts of a pencil which fall near the boundary of a convex lens to converge to a nearer (*i.e.* shorter) focus than those which fall near the centre. This may be corrected by a proper selection of the forms of the two lenses which replace, in all modern telescopes, the single lens hitherto considered.

(100.) The false colouring of the image is due to *chromatic aberration*. The pencil of light proceeding from a point such as P, figs. 14, 16, and 18, consists of rays of different refrangibility, and therefore not converging to a focal point such as *p*, but to a short line on the axis of the pencil,—one end of this focal line (the end nearest the object-glass) being violet, the other red, and the intermediate points being orange (next the red), yellow, green, blue, and indigo. Thus a series of coloured images is formed at different distances from the object-glass; and if a screen were placed to receive the mean image *in focus*, a coloured fringe due to the other images (*out of focus, and therefore too large*) would surround the mean image.

(101.) Newton supposed that it was impossible to get rid of this defect, and therefore turned his attention to the construction of reflectors. But the discovery that the *dispersive* powers of different glasses are not proportional to their refractive powers, supplied opticians with the means of remedying the defect. Let us carefully consider the nature of this discovery. If with a glass prism of a certain form we produce a spectrum of the sun, this spectrum will be thrown a certain distance away from

the point on which the sun's rays would fall if not interfered with. This distance depends on the *refractive* power of the glass. The spectrum will have a certain length, depending on the *dispersive* power of the glass. Now, if we change our prism for another of exactly the same shape, but made of a different kind of glass, we shall find the spectrum thrown to a different spot. If it appeared that the length of the new spectrum was increased or diminished in exactly the same proportion as its distance from the line of the sun's direct light, it would have been hopeless to attempt to remedy chromatic aberration. Newton took it for granted that this would happen. But the experiments of Hall and Dollond showed that there is no such strict proportionality between the dispersive and refractive powers of different kinds of glass. For instance, the refractive power of flint glass is greater than that of crown glass; but its dispersive power is greater in greater degree. Hence if, instead of a single lens of either crown glass or flint glass, we have a double lens such as is shown in fig. 19—a convex lens of *crown* glass with a concave lens of *flint* glass—we may so combine their refractive and dispersive powers as to obtain an image in which though a little colour still remains, there is not enough to interfere seriously with the distinctness of the image. The convex crown glass lens, which is towards the object, would, alone, cause the rays to converge towards a focus, as shown in fig. 16, with a considerable dispersion and therefore much colour. The concave flint glass undoes a *certain portion of the convergence*, sending the rays to a remoter focus; but it undoes *the whole of the dispersion between certain tints of red and violet.* It would undo the whole chromatic dispersion, were the dispersions by flint glass and crown glass proportional throughout the whole spectrum. But as this is not the case, the telescopist has to be content with an arrangement by which the most troublesome part of the chromatic dispersion is corrected, the part which remains being, in good double object-glasses, very slight.

Fig. 19.

(102.) But even if the image formed by the object-glass were perfect, yet this image, viewed through a single convex lens of short focus placed as in fig. 16, would appear curved, indistinct, coloured, and also *distorted*, because viewed by pencils of light which do not pass through the centre of the eye-glass. These effects can be diminished (but not entirely removed *together*) by using an *eye-piece* consisting of two lenses instead of a single eye-glass. The two forms of eye-piece most commonly employed are shown in figs. 20 and 21. Fig. 20 is Huyghens's eye-piece, called also

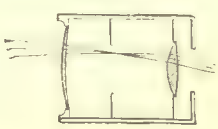


Fig. 20.—Huyghens's Eye-piece.

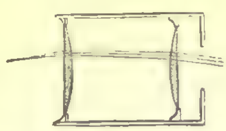


Fig. 21.—Ramsden's Eye-piece.

the *negative* eye-piece, because a real image is formed *behind* the *field-glass* (the lens which lies nearest to the object-glass). Fig. 21 represents Ramsden's eye-piece called also the *positive* eye-piece, because the real image formed by the object-glass lies *in front of* the field-glass.

(103). The course of a slightly oblique pencil through either eye-piece is exhibited in the figures. The lenses are usually plano-convex, the convexities being turned

towards the object-glass in the negative eye-piece and towards each other in the positive eye-piece. Coddington has shown, however, that the best forms for the lenses of the negative eye-piece are those shown in fig. 20.

(104.) The negative eye-piece has the advantage of being achromatic, and is commonly employed in all observations requiring distinct vision only. But as it is clearly unfit for observations requiring micrometrical measurement, or reference to fixed lines at the focus of the object-glass, the positive eye-piece is used for these purposes.

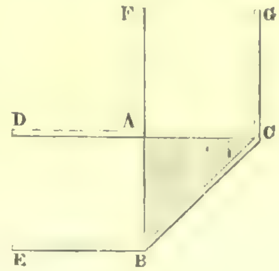


FIG. 22.
The diagonal Eye-tube.

(105.) For observing objects at great elevations the diagonal eye-tube is often convenient. Its construction is shown in fig. 22. A B C is a totally reflecting prism of glass. The rays from the object-glass fall on the face A B, are totally reflected on the face B C, and emerge through the face A C. In using this eye-piece, it must be remembered that it lengthens the sliding eye-tube, which must therefore be thrust further in, or the object will not be seen in focus. There is an arrangement by which the change of direction is made to take place between the two glasses of the eye-piece; for instance, one lens of the eye-piece would lie between D E and A B, the other between F G and A C. With this arrangement (known as the *diagonal eye-piece*) no adjustment of the eye-tube is required. However, for amateurs' telescopes the more convenient arrangement is the diagonal eye-tube, since it enables the observer to apply any eye-piece he chooses, just as with the simple sliding eye-tube.

FIG. 23.—Huyghens's method of mounting a long-focus Telescope.

(106.) Not with such convenient aids, however, did the first telescopic observers pursue their work, when as yet the single object-glass, with all its optical defects uncorrected, was employed; only the use of object-glasses having great focal length would give images which would bear much magnifying. Fig. 23 will serve to give an idea of the inconveniences which resulted. It represents Huyghens's method of mounting a long-focus telescope. The object-glass, enclosed in the short tube k,

and set on the frame *e f c d*, was carried up and down the tall pole *a g*, by means of the cord *h a c* passing over the pulley *a*. The weight of the frame and object-glass was counterpoised by the weight *h*. The weight *n* brought the centre of gravity of the apparatus attached to the object-glass into the ball and socket above *f*, so that it could be easily managed by the wire *l m*, and its axis brought into line with the eye-glass at *o*. When it was very dark Huyghens was obliged to make his object-glass visible by means of a lantern L, so constructed as to throw the rays of light upon it in a parallel beam. The picture is a fac-simile of an engraving which appeared in the first edition of the *Encyclopedia Britannica* (art. 'Optics'). The perspective might have been improved upon.

(107.) Campani at Bologna made object-glasses having focal lengths of 86, 100, and 136 feet. Divini at Rome and Borelli in France also made object-glasses of great focal length. But, unless the stories related of Hartsocker are true, Auzout surpassed all others in the focal length he gave to object-glasses, one of his having the preposterous focal length of 600 feet; naturally he could never use it. He succeeded in looking occasionally at the heavenly bodies through another, 300 feet in focal length, but not to any useful purpose.

(108.) We are at present concerned, however, only with the telescope as a means of increasing the accuracy of astronomical observations for determining the positions of the heavenly bodies. The method invented by Gascoigne was brought into general use after its successful adoption by Picard and Auzout in 1667. It was then that the series of exact determinations of position began which has been the basis of modern exact astronomy.

(109). From this time two principal forms of instrument for determining positions came into vogue :—

First, instruments mounted to turn on an east-and-west *horizontal axis*, to which the optical axis of the instrument is *fixed at right angles*, so that as the instrument is turned round on its horizontal axis, the centre of its field of view sweeps the meridian. Such an instrument is said to be mounted *meridionally*, and is called a *meridional instrument*: according to details of construction it is named a *transit instrument*, a *transit circle*, or a *mural circle*,—the last being now little used.

Secondly, instruments mounted to turn on a *polar axis* (that is, an axis directed to the pole of the heavens), to which the optical axis of the instrument *may be set at any angle*, so that as the instrument is turned round on its fixed polar axis the centre of its field of view sweeps a *declination parallel*,—such an instrument is said to be mounted *equatorially* and is called an *equatorial instrument*, or sometimes simply an *equatorial*, under which general name several special forms of instrument are included.

(110.) To these may be added a class of instruments used only for special purposes in large observatories, but commonly the form of telescope with which the young student of astronomy is most familiar, viz :—

Thirdly, instruments mounted to turn on a *vertical axis* to which the optical axis of the instrument may be *set at any angle*, so that as the instrument is turned round its vertical axis, the centre of the field of view sweeps a *circle parallel to the horizon*. Such an instrument is called an *alt-azimuth*; because one movement alters the direction of its optical axis in *altitude* or angular elevation above the horizon, while the other alters that direction in *azimuth* or direction with regard to the compass points :—

Of telescopes of the first kind, a general idea may be obtained from fig. 23A, borrowed from Herschel's 'Outlines of Astronomy.' It will be easily seen that if the axis of such an instrument is exactly horizontal, and lies also exactly east-and-west, the optical axis of the telescope must lie always in the meridional plane, and the centre of the telescope's field of view will always be a point on the meridian. Thus if we turn the telescope on its axis the centre of the field of view will sweep the meridian.

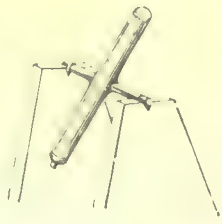


FIG. 23A.—Illustrating the principle of Transit Instruments.

(111.) But it need hardly be said that for astronomical surveying, instruments of a less simple construction are required. To ensure the accurate horizontality of the axis requires careful levelling. The east-and-west position of this axis is determined in the first place by assigning due positions to the notches in which it turns. But this is a work requiring great care; moreover, after the right position of the axis has been obtained, changes are apt to take place from a number of causes which cannot be prevented nor in all cases foreseen. The notches wear unequally; the ends of the axes are equally liable to unequal wear; the axis itself yields more or less, and more or less unequally; changes of temperature affect the side supports, the axis, the telescopic tube. Even the ground and the pier on which the transit instrument stands undergo slight changes of position from time to time changes which would not have been worth considering with the old astrolabes and quadrants, but which may measurably affect the exceedingly delicate observations which the telescope enables astronomers to make. Thus the transit instrument must be constantly under careful supervision and adjustment. Fortunately the very qualities which cause such instruments to require adjustments so delicate, enable the observer to effect the adjustments with the necessary accuracy.

(112.) The general principles on which the transit instrument in ordinary use in observatories—the so-called *portable* transit instrument—is constructed, are illustrated in fig. 24—from the 'Penny Cyclopædia.'[1] The principal parts here pictured are:—

1st.—The metal stand, A B, carrying the grooves (technically called Y's) in which the axis of the telescope works, and the adjustment by which these are brought into position.

2nd.—The telescope C D, and its axis, E F, with a vertical circle, G, at one end of the latter, so that the inclination of the telescope to the horizon may be known, and that stars to be observed in transit may be brought into the field and identified.

3rd.—The 'striding level' H K, for adjusting the axis to horizontality. Part of the milled head of a screw under the left-hand Y can be seen in fig. 24. By this the Y is raised or depressed till horizontality is secured. The

[1] The student of astronomy must not be led by the apparent complexity of astronomical instruments as pictured in figs. 24, 26, and 31, and the like, to imagine that working the instruments is a complicated affair. Many of the appendages which have the most complicated aspect, have little more to do with the observer's actual work than the interior details of a pianoforte with the execution of a performer. The chief difference is that the astronomer has to keep all the details of his instruments under constant supervision. But he cannot be many days at work with an instrument, even though of novel construction, without recognising the mutual relations and interactions of its parts; after which he can have no difficulty in attending to them, adjusting, lubricating, easing, tightening, or the like, as occasion may require.

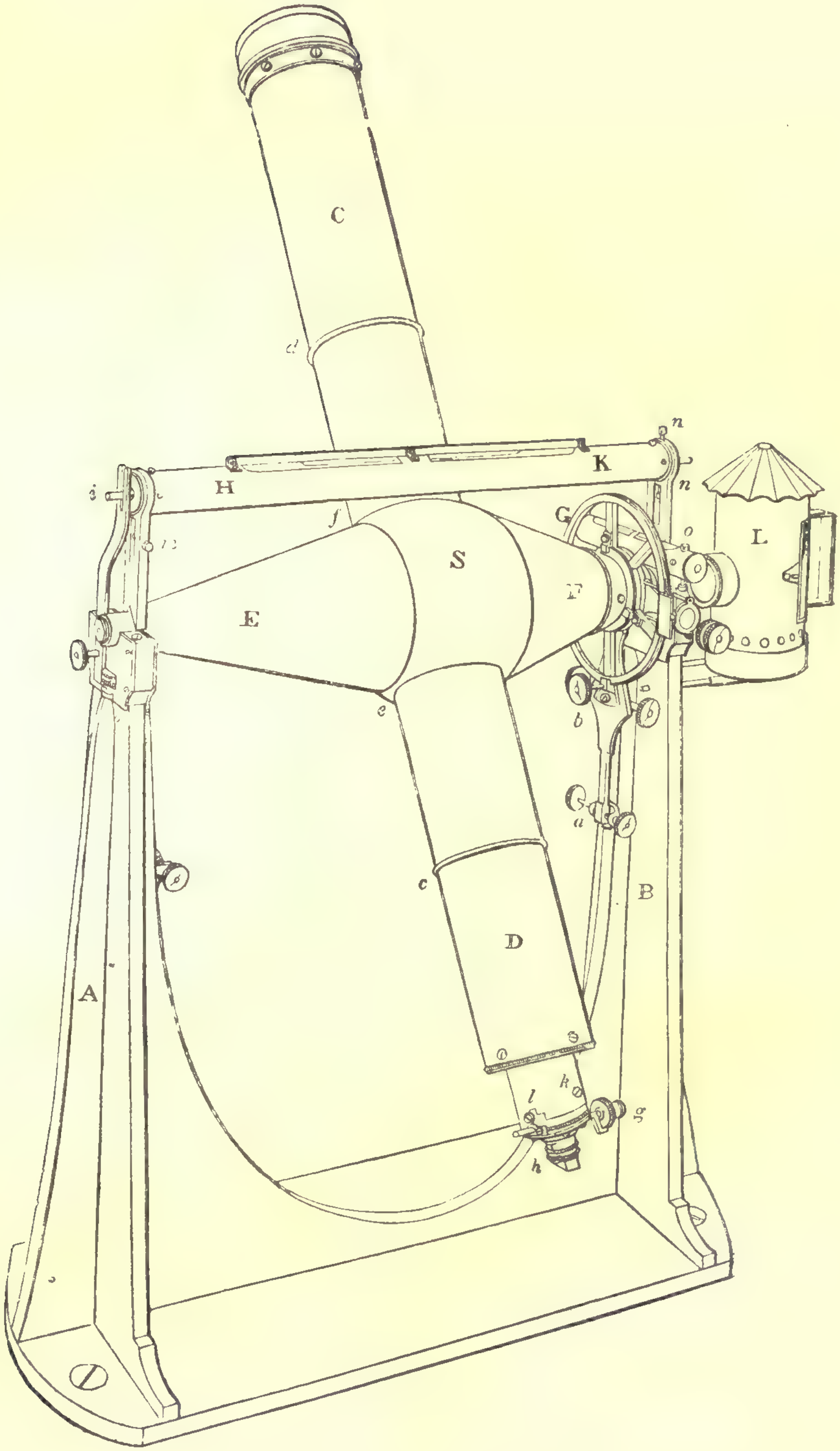


Fig. 24.—The portable Transit Instrument.

right-hand Y is moved horizontally—that is, in azimuth—by a screw, the milled head of which is seen under the glass of the lantern in fig. 24.

(113.) The axis of the telescope is made of two strong brass cones, soldered on a central sphere S, through which the tube of the telescope passes; pivots are soldered to the ends of the cones. The right-hand pivot (in the figure) is pierced to admit light from the lantern L. This light falls on an annular plate in the middle of the central sphere, inclined at an angle of 45° to the axis and to the length of the telescope. The light from the object observed passes through this annulus, but the light from the lantern, falling on the surface of the ring, is reflected to the eye-end, illuminating the field of view. (This light can be regulated.)

(114.) The level rides on the pivots as seen. There is a pin at each end falling into a fork, *i*, for safety. The level is adjusted to parallelism with the axis, and the left-hand pivot is then lowered or raised until the level indicates horizontality.

(115.) The instrument, it will be seen, can be lifted out of its Y's (after the striding-level has been removed) and reversed, that end of the axis which had been to the east being set to the west, and *vice versâ*. This is an important aid to adjustment, enabling the observer to ascertain the corrections for several of the causes of error, though not for all. I do not enter here into an account of the various adjustments by which either the instrument is brought into a position of practical exactitude, or the corrections to be applied to its indications are ascertained, before observations begin; all such matters are fully treated of in books on practical astronomy, and if dealt with fully here would occupy an inappropriate amount of space, with matter, too, which would be by no means original or suitable to my special purpose in this work.

(116.) In large observatories the transit instrument rests on stone piers instead of a movable metal stand, and as the instrument itself is larger and heavier, arrangements have to be adopted for supporting its weight in such sort as to diminish flexures, friction, and so forth. But into such details we need not here enter.

(117.) In the focus of the eye-piece of the transit instrument is set a system of five or seven equidistant vertical threads, (vertical, that is, when the instrument is directed horizontally) and one central horizontal cross-thread (or preferably two horizontal cross-threads close together). These are called 'wires,' but in reality the finest wires would be too coarse for this work, and the filaments of the spider web are used. A special kind of spider is, indeed, chosen to supply transit wires; and some attention is given to the selection and breeding of this particular arachnoid.

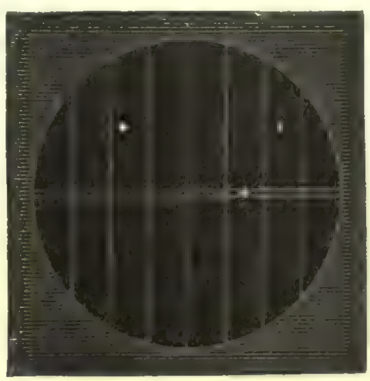


FIG. 25.—The Field of View of a large Transit Instrument. (In smaller instruments five vertical wires and one horizontal wire would be used.)

(118.) The field of view of the transit instrument thus presents the appearance shown in fig. 25. Three stars are in the field, passing across from right to left (the telescope being astronomical or inverting). The one under observation, between the horizontal lines, has already passed two wires. The time of passing each wire is noted, either by mentally recording the indications of the clock (beating sidereal seconds), or by tapping a break-circuit key so that a record will be left on a registering apparatus, in company with records of the clock-beats. The time of passing the central wire, if correctly taken,

would be the true time of the star's transit of the meridian. The mean of the times of passing the middle three wires, or the middle five, or all the seven, would also, if correctly taken, give the true time of transit. But as a matter of fact no observer can be trusted, or trust himself, to make any such observations exactly. The use of the seven wires is to give several observations, the mean of which may better be trusted than any single observation, or than the mean of a smaller number. It is found accordingly that the recorded time of transit across the middle wire is seldom the same as the mean of the recorded times of crossing the seven wires. When it is the same it is only by chance. It is found also that different observers make errors varying in amount, while the error for each observer is apt to be of a kind special to himself. One observer will always or generally record the transit of a wire a little ahead of its true time, or always or generally be a little behindhand; the average amount of such error being greater with some observers than with others. Such peculiarities are called the observer's 'personal equation,' which must be carefully taken into account in considering each observer's work.

(119.) The old Egyptian astronomers watching transits from inside the Grand Gallery of the Great Pyramid would have been amazed to hear of the developments of observing accuracy which have thus been obtained by modern science—and that, too, without any such reliance on mere mass and solidity as made their own constructions for observation so expensive, alike in regard to time, to labour, and even to life.

(120.) The transit instrument gives the true time of meridional passage, but not the precise altitude at which the transit is made. This, of course, it must roughly indicate, or the identification of a star would often be difficult; nay, the observer could not be sure of the star passing through his field of view unless he had the means of determining with considerable accuracy the inclination of the tube to the horizon. But to determine altitudes with such accuracy as modern astronomical work requires, a transit instrument of a different sort must be employed. The vertical circle, or circles, for determining the inclination of the instrument, must be larger and must be more finely divided, while methods for precisely ascertaining the indications of these circles must also be provided.

(121.) The *mural* circle was formerly much used for this purpose. It may be described as a transit telescope whose axis passed through a vertical north and south wall on whose face the flat circle turned, with suitable devices for indicating its position, and therefore the inclination of the telescope to the horizon.

(122.) But the *transit circle*, (pictured in fig. 26,[1]) has now to a great degree replaced mural instruments in large observatories. The nature of the instrument will be readily understood. The axis is supported by two piers on the east and west—the instrument being reversible, though, owing to its size and weight, reversal is not often effected. The telescope is set between two large circles, each graduated into 5 spaces, from 0° to 360°. By means of eight micrometers (four of which, on the nearer side of the piers, are shown at A, B, C, and D) very precise readings of the great circle divisions are obtained, and thus the altitudes of a heavenly body can be ascertained with great accuracy. But obviously the transit circle can be used also to determine the time of meridional transit. The meridional altitude (or its comple-

[1] Fig. 26 is borrowed from Loomis's *Practical Astronomy*, in which it appears in company with a number of other illustrations of instruments, derived from various sources. It represents the transit circle of the Harvard observatory at Cambridge, Mass.

ment, the zenith distance) can at once, with suitable corrections for refraction, be reduced to polar distance (or its complement, the declination, or distance north or south of the equator); while the time of meridional transit gives the right ascension (or the distance of the hour circle through the object from the hour circle through the place where

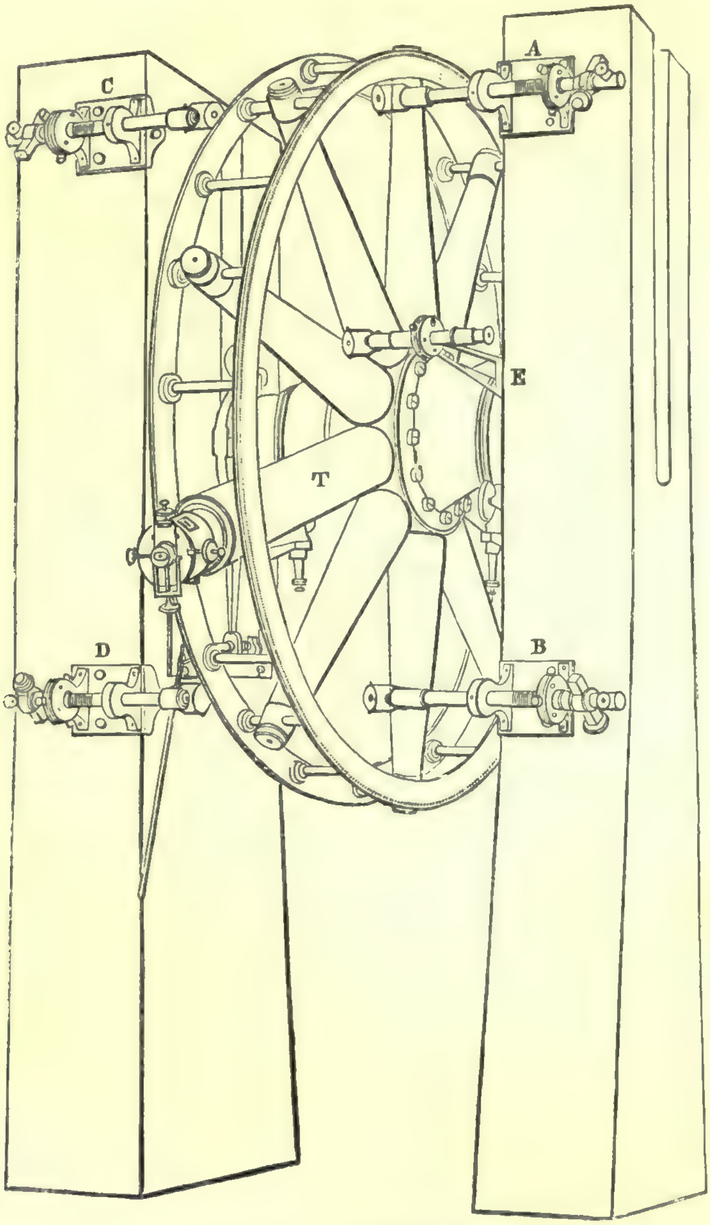


FIG. 26.—The Transit Circle.

the sun's path crosses the equator ascendingly). Hence the transit circle can be used to determine the position of an object on the celestial sphere with extreme accuracy. It may be regarded, in fact, as the most important of all astronomical surveying instruments.

(123.) The second class of telescopes, the equatorial instruments, are of greater

importance than transit instruments in the study of the heavenly bodies, because they can be directed to all parts of the celestial sphere. They are used also for determining positions. Indeed, it often happens that unless the position of a moving body could be determined when it is off the meridian, the astronomer would have little chance, or none, of determining its orbit.

(124.) The equatorial telescope is represented, almost in its simplest form, in fig. 27. Here *a* is the polar axis, round which the telescope is carried; *b* is the axis which bears the telescope; *c* and *d* are graduated circles on these axes, by means of which the amount of turning around either can be indicated and measured; and *w* is a counterpoise to the weight of the telescope, so arranged that the centre of gravity of the whole mass moved round the axis *a* may fall on this axis, the centre of gravity of the telescope itself being in the prolongation of the polar axis *b*. The screws *s*, *s*, *s*, *s* are used to bring the polar axis to its right inclination, and other screws not shown in the figure give to the vertical plane through *a* a motion in azimuth, by which this plane is made to coincide exactly with the meridian. It will be seen that the optical axis of the telescope may be inclined at any angle to the axis *a*, from parallelism pointing towards the pole to parallelism pointing from the pole; but in the last-named directions, useful positions are limited by the position of horizontality (for astronomers do not study objects below the horizon). In rotating the telescope on the polar axis, after its inclination to the declination axis has been fixed, it will be found that there are limits to the motion, owing to the interposition of the stand. For instance, supposing the telescope clamped in declination in the position shown in fig. 27, it could be swung round the polar axis to horizontality by bringing up the eye-end, and from that position, by lowering the eye-end, it can be swung back to the position shown in the figure, and thence onwards in the same direction, till the eye-end comes into contact with the stand. Yet the range of the instrument is not limited on this account. If any direction such as the telescope cannot assume by rotation round the axis *a* on this side is required, all that is necessary is to carry the telescope over, round *a*, moving the object-end towards the left in the figure till the object-end is lower than the eye-end, and then, having previously unclamped the declination axis, bringing the telescope to the right inclination with the declination axis on the other side of the stand. It can then be swayed round the polar axis on that side of the stand till the object to be observed is in the field of view. Or the student may find it more convenient to first shift the telescope round the polar axis (both axes being unclamped), drawing the eye-end up from the position shown in the figure until the axis of the telescope is parallel to the polar axis, passing beyond that position until the telescope's axis makes the same angle as before with the polar axis, but with its eye-end above insteaa of below, or *vice versâ*,

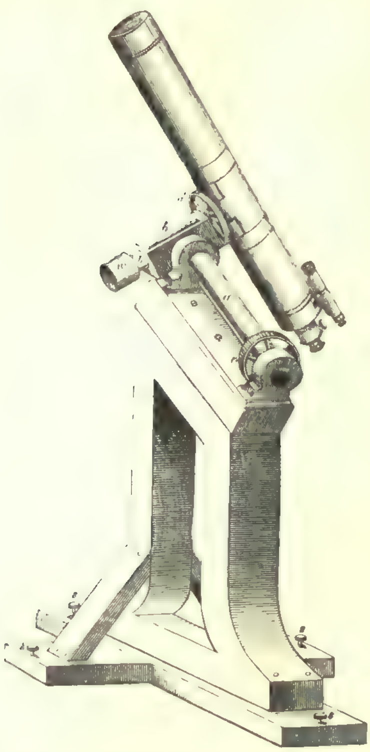


FIG. 27. - The Equatorial Telescope.

as the case may be: then clamp the declination axis,[1] and sway the telescope round to the other side of this stand from that which it had before occupied.[2]

(125.) The small telescope attached near the eye-end of the telescope, in fig. 27, is called the *finder*. Its optical axis is parallel to that of the large telescope, and it has a large field of view; so that by bringing an object to the centre of the field of this small telescope, marked by cross wires, the object is brought to or near the centre of the field of view of the large telescope.

(126.) The equatorial telescope does not appear, however, in this simple form in the observatories of our time. Many complicated appendages are attached to it to make its work more complete. The principle of the instrument remains unchanged however.

Plate II. represents one of the large equatorial instruments[3] of our day, and the interior of the domed observing-room within which it is mounted. The instrument is provided with no less than three finders, one of which is a finder on a finder. To the eye-end spectroscopic adjuncts are attached, the instrument being supposed to be at the moment employed in spectroscopic research. The clockwork for driving the telescope at due rate round the polar axis will be noticed. By its means a star can be kept in an unchanging position in the field of view for a long time. Any object also whose movement in declination is slow can be kept by the driving apparatus, suitably regulated, a long time in the field; but of course, as declination changes, the inclination of the telescope to the polar axis must be changed by movement round the declination axis.

(127.) The domed interior of the modern observatory, as shown in Plate II., is worth noticing. The roof revolves either on spheres, or, as illustrated in Plate II., on wheels; and though a roof may be very heavy it can be so well poised that a very slight force will set it revolving. The curved shutter, shown in Plate II., can also be easily closed and opened.

(128.) One such illustration as Plate II. will tell all that the student of astronomy requires to learn about equatorial telescopes from a work like the present. It would be *en règle* here to introduce illustrations of ten or twelve well-known instruments, including some which have been repeated in our books of astronomy a score of times at least. But this, though it would suit me well if I wished to find matter to occupy my space, would be exceedingly inconvenient where my great difficulty is to find space for the matter I wish to deal with.

(129.) Instruments of the third or alt-azimuth class are less commonly used by astronomers than either meridional or equatorial instruments. They are not

[1] By clamping an axis I mean using such clamp as the optician has provided to stop motion round that axis.

[2] It may be interesting to the young mathematical student to notice that in one of the above considered directions of the telescopic axis with respect to the declination axis, motion around the polar axis causes the optical axis to assume successive positions of one set of generating lines of an 'hyperboloid of one sheet'; while in the other direction of the telescopic axis, the same motion causes the optical axis to assume successive positions of the other set of generating lines of the same hyperboloid.

[3] The telescope here illustrated is that which has been mounted in the Halsted Observatory at Princeton, New Jersey. Its object-glass is 23 inches in diameter; focal length 30 feet; diameter of the domed roof, 39 feet. The hour-circle (on the polar axis) and the declination circle (on the other axis) are each 30 inches in diameter. The performances of this telescope, hitherto, have not been remarkable; but it has only been six or seven years at work, and its powers have hitherto been devoted to matters of greater difficulty than usefulness—so difficult some of them (and so useless) that one could wish they were impossible.

susceptible of the same degree of precision. Transit instruments turn round one axis constantly fixed in position, and determine with extreme precision the time of crossing a particular plane, and also the direction of crossing in the case of the transit circle. The equatorial instrument derives its chief advantage from the fact that, after being fixed at a certain inclination to the polar axis, it can be kept directed upon the same celestial object for a long time by uniform motion around the other axis alone. The alt-azimuth has no such advantages. To keep a celestial object in the field of view, motion must be maintained about both axes, and at varying rates to follow the varying rates at which a celestial body changes its height above the horizon and its direction with respect to the cardinal points. This does not greatly matter with small telescopes, but with large ones and for observations of precision it is a fatal objection for all save such very special uses of the alt-azimuth as will presently be considered.

(130.) The ordinary alt-azimuth instrument for astronomical amateurs is shown in fig. 28, and needs no description. In my own amateur days I devised an alt-azimuth mounting which I found very convenient in use. It is illustrated in fig. 29. The handles h, h', are conveniently situated near the observer. By turning the upper handle, so that the rod $h\,e$ rotates on its own axis, the endless screw near e works the pinion b and thus moves the quadrant a so as slowly to raise or depress the telescope as may be wished. Rotating similarly the lower handle h', turns a pinion working in a crown wheel, by which another pinion, working against

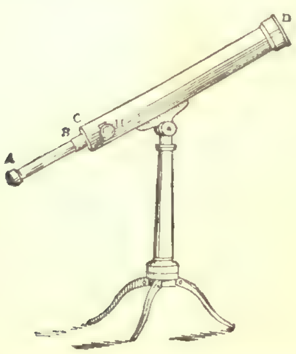


Fig. 28. A small Alt-azimuth for amateur observation.

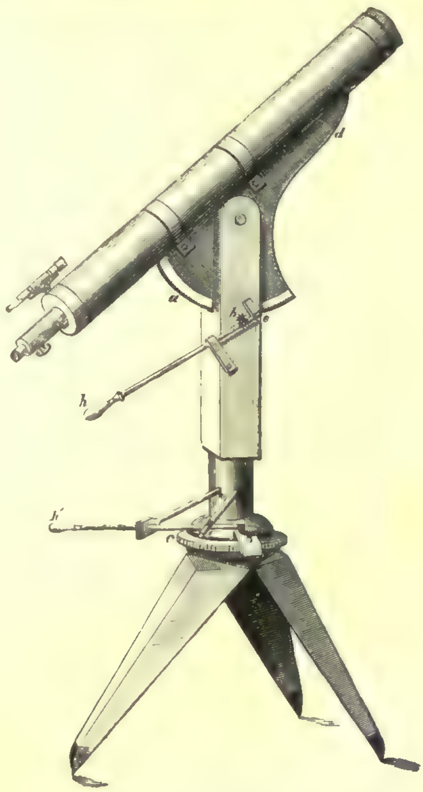


Fig. 29.—Proctor's Alt-azimuth Mounting.

the teeth of the circle c, rotates the telescope in azimuth. For large movements in altitude, the handle h is drawn slightly upwards, an elastic fastening at e allowing the endless screw to be thus drawn away from the pinion at b; then, the quadrant a being released from the slow movement, the telescope can be moved freely. In like manner, on slightly depressing the lower handle h', the elastic fastening at e' allows the pinion at the end of this handle to be drawn free of the crown wheel, when the telescope can be moved freely in azimuth. With a little practice it is as easy to

keep a star in the field of view by gently turning the handles h, h', as if the telescope were an equatorial;[1] while yet the telescope is freed both in altitude and azimuth by an instantaneous movement.

(131). The alt-azimuth has its value in exact astronomical observations. Its principle, regarded as an instrument for indicating positions, is the same, of course, as that of the old non-polar astrolabe, Tycho Brahe's quadrant, and similar instruments. Such instruments had their special value, and although the transit circle, mural circle, and transit instruments of our time do all the meridional work which instruments having a movement in altitude could effect, there are cases in which extra-meridional observations are essential. (For example, the moon when near the sun cannot be observed on the meridian.) Now such observations can be made with the equatorial; but with the equatorial mounting we are manifestly more apt to be troubled by mechanical displacements than with a mounting in which one axis is vertical, as with the alt-azimuth. (In transit instruments there is but one moveable axis.) For instance, if we set an alt-azimuth so that the telescope when horizontal is directed due north or south, and clamp the vertical axis, the instrument is manifestly for the time a transit instrument, and may be made very trustworthy if the mechanical constructions are well devised and well executed. Now the equatorial becomes a transit instrument if the declination axis is brought to a horizontal position; but the telescope is then on one side or on the other of the polar axis, and however satisfactorily the weight of the telescope may be counterpoised, yet as it is not balanced by a counterpoise of the same shape and material, there must be changes of balance arising from differences of temperature, position, atmospheric currents (and resulting pressures), which, however trifling they may seem to others than

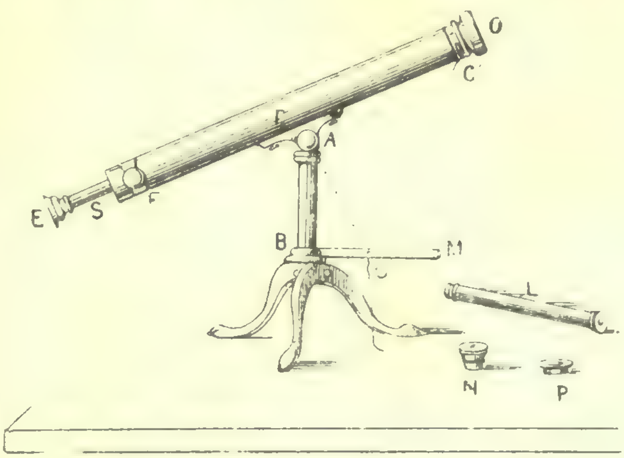


FIG. 30. Method of mounting an Alt-azimuth to work equatorially.

[1] An ingenious way of altering an ordinary alt-azimuth into an instrument almost as handy as an equatorial, is illustrated in fig. 30, borrowed from that excellent little treatise on amateur work in astronomy, my friend Captain Noble's *Hours with a 3-inch Telescope.* The method was invented by Earl Crawford, better known in scientific circles as Lord Lindsay. B M is a horizontal bar attached firmly to the pillar A B, and directed towards the south. At C a hole is bored such that C A is directed towards the pole of the heavens. It is evident that if after the telescope had been directed towards a star in the southern skies, a cord, as C C′, is tautened between C and C′ (a convenient fastening-place on the tube) and fastened, then if the telescope is moved in azimuth, the cord C C′ remaining taut, the telescope will move precisely as if swayed round A C as an axis. For throughout the movement the shape of the triangle C A C′ remains unchanged—or this triangle moves precisely as though it were rigid. Hence since C′ A is directed to the pole of the heavens, the telescope, when moved according to the prescribed conditions, is carried round a polar axis, precisely as an equatorial. For observation of the northern heavens A B M must remain unchanged in position, but the tube must be directed towards the north, and the cord, stretched from C to some suitable fastening-place near the eye-end.

astronomers, must affect such precise observations as modern exact astronomy requires. Of course the best meridional instruments will do better work than an alt-azimuth even of the best construction; and no astronomer would care to use an alt-azimuth in meridional work except for comparison with the work of some thoroughly trustworthy meridional instrument, whereby some of the corrections which the alt-azimuth requires may be ascertained. But for extra-meridional work of the surveying kind, the alt-azimuth properly mounted is the best instrument available—at least, where great accuracy is required. Of course its use is more laborious. For every observation special computations must be made, both beforehand that the instrument may be duly directed to receive the orb observed, and afterwards for the exact interpretation of the observations. But for such work there are plenty of paid assistants in large observatories.

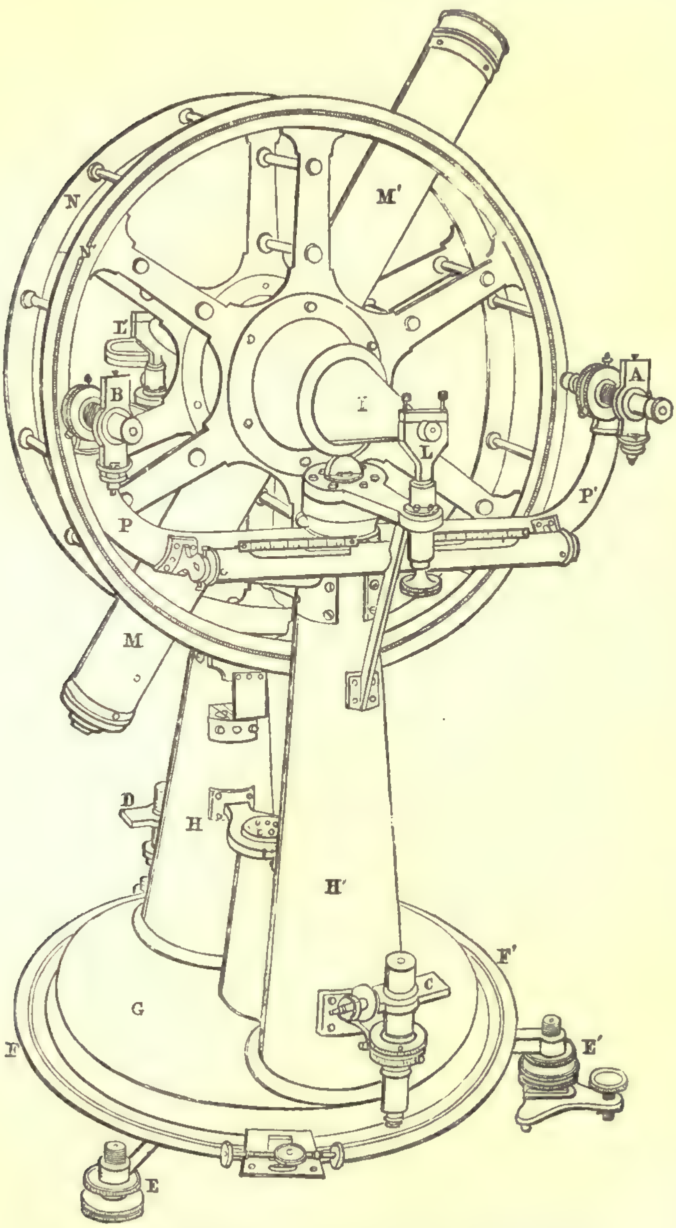


Fig. 31.—The Alt-azimuth as used in large observatories.

(132.) Fig. 31 represents an alt-azimuth as employed in exact astronomical work.[1] The telescope is mounted between two pillars H and H′ so as to be poised over the centre of the azimuth circle F F′. The strong circular plate G bearing these pillars and the telescope turns smoothly and evenly on a vertical axis. Two of the screws for adjusting the azimuth circle to perfect horizontality are seen at E and E′. A contrivance will also be noticed in connection with the foot E′, by which the position of this foot can be slowly shifted. (Instead of resting

[1] The figure is taken from Loomis's *Practical Astronomy*; the original drawing, from an instrument made by Troughton and Simms in 1836, will be found in the *Penny Cyclopædia* (Art. 'Circle, Astronomical').

on the fixed base, this foot rests in a groove on the small triangular top of a tripod, one foot of which is the end of a screw while the others are sharp points: turning the screw lifts or depresses the outer angle of the small tripod, and thus slowly raises or depresses the free foot of the alt-azimuth.) D and C are microscopes for reading the indications of the graduated azimuth circle. The telescope is fixed between the circles N and N′, which are fastened together for greater rigidity by a number of small brass pillars. Circles, telescope, and double conical axis (shown on one side at D), are carried round the horizontal axis on the pivots L and L′. The microscopes A and B for reading the indications of the graduated altitude circle N′ are carried by two strong arms, P and P′, attached near the top of the pillar H′.

(133.) The alt-azimuth has been much used for extra-meridional observations of the moon. But there are other objects which may conveniently be observed by means of this instrument, which should find a place in all large observatories.

(134.) Before passing from this preliminary discussion of telescopes as the instruments used in modern astronomy for the exact survey of the heavens (the use of the telescope for physical research in astronomy will be considered farther on) I must make a few remarks on the devices employed for reading the indications of graduated circles.

(135.) Of these the vernier, though practically replaced in precise work by the micrometer, is still constantly used for first adjustments and where very precise readings are not required. Its principle is very simple:—

In fig. 32, C C′ is supposed to represent part of the graduated circle which is to be read; in this case it is a circle divided into degrees (71°, 72°, and 73° are shown) and tenths. V V′ is the vernier carried round with the motion we have to measure by the reading indicated, the edge *p a* being the indicating or pointing edge. (For instance, in the mercurial barometer the pointer *p* is made to coincide with the top of the mercury.) In such work as we are considering now, an instrument is directed to some heavenly body, and when that body is brought centrally into the field, the edge *p a* takes a certain position, which the graduated circle and the vernier V V′ enable us to determine in the manner now to be explained.

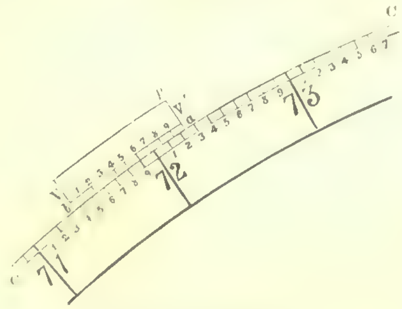


Fig. 32.—The Vernier.

The arc V V′ measures exactly nine of the divisions of the arc C C′, and is divided into ten equal parts, each of which therefore measures exactly 9-tenths of a division of the graduated circle. Now it will be noticed that the edge *p a* reads off on the graduated circle 72°, 2-tenths, and a fraction which we might roughly estimate as three or four fifths of one of the tenth divisions. The vernier, by showing its sixth division in close if not exact agreement with one of the tenth divisions on the arc C C′, tells us that this fraction is nearly if not exactly 6-tenths, or gives the reading 72° 2-tenths 6-hundredths as approximately correct. The reason is not far to seek. If division 6 on the vernier agrees with a division on the graduated circle, division 7 is *one*-tenth behind a division on the circle; division 8 is *two*-tenths behind; division 9 is *three*-tenths behind; and finally the edge *p a* is 4-tenths behind a division on the graduated circle, or 6-tenths in advance of a division, which we see at once is division 72°·2; so that our reading is 72°·26. It is easy to perceive, by similar reasoning, that

whatever may be the number on the vernier in nearest agreement with a division on the circle, that number gives the number of lengths of the divisions on the graduated circle, or, in the case illustrated, the number of hundredths of a degree which we are to add on account of the fraction of a division between the pointing-edge *p a* of the vernier, and the graduation (72°·2) next behind it on the circle.

(136.) Suppose now, that instead of reading the indications of the vernier when the pointing-edge is in a particular position, we wish to *set* the pointing-edge in such a position. Suppose for example we wish to adjust an equatorial telescope so that it will point, when suitably rotated on the polar axis, to a star having north declination 72° 15′ 36″ (which we reduce at once to 72°·26 because our graduated circle is supposed to show degrees and tenths of a degree). We revolve the telescope on its declination axis until the pointing-edge of the vernier falls between 72°·2 and 72°·3. That already brings the telescope into rough adjustment for declination. We then bring the 6 mark on the vernier into agreement with one of the neighbouring division marks on the graduated circle, selecting that division with which the 6 mark on the vernier can be brought into agreement without the pointing-edge *p a* passing outside the space 2–3 on the graduated circle.[1] Then our adjustment is as nearly exact as the vernier can make it.

(137.) It is hardly necessary to note that where exact tenths of the divisions on the graduated circle are not indicated, or given (as the case may be), it is easy, if necessary, to approximate to the true intermediate reading. Supposing, for example, the vernier 6 mark, in fig. 31, were slightly behind the 9 mark on the graduated circle: then we should know that the reading was slightly less than 72·26, while the reading would be slightly more than 72·26 if the vernier 6 mark were in front of 9 on the circle. Were the 6 mark as far behind the 9 as the 5 mark was in front of the 8, the true reading would be 72·255: and it is easy, by noticing the proportion between the displacements of one mark behind and another mark in front of neighbouring divisions, to get a near approach—all that can be needed in vernier work—to the hundredths of the circle-divisions, or in this case to the third decimal place of degrees.

(138.) But in modern astronomical work verniers are only used for comparatively rough adjustments. For exact work the microscope is constantly brought into play, in company with screw adjustments by which the minutest displacements may be determined. Instruments of this kind are called micrometers, and are of three principal kinds: instruments for the exact reading of graduations, called 'reading micrometers'; instruments for measuring small celestial arcs, called 'measuring micrometers,' and usually found in the form known as the 'filar micrometer'; and instruments for determining celestial directions, called 'position micrometers.' At this point we need only consider the Reading Micrometer.

(139.) Fig. 33, from Herschel's 'Outlines of Astronomy' indicates roughly the way

[1] It is necessary to notice this point: unless the observer, as is always desirable, has already brought the edge *p a* to its approximately correct position, in which case the particular division on the vernier which is to be dealt with will be already quite close to the right division on the graduated circle. Otherwise, a little carelessness might set the vernier a tenth division wrong. Suppose, for example, that fig. 32 shows the rough adjustment of the vernier when the required reading is 72°·22. If now the 2 division in the vernier, which falls about halfway between 5 and 6 on the graduated circle—really nearer 5, but a careless eye would not notice this—is brought to agreement with 6, the instrument, instead of being adjusted to 72°·22, will be adjusted to 72°·32.

in which a fixed microscope commands a view of the graduated circle, moving as the telescope is swayed round some particular axis. The interior of the microscope with its cross-wires at the focus of the eye-piece is shown in fig. 34. Fig. 35, from Loomis's 'Practical Astronomy,' is a more exact view of the exterior of the microscope, showing the eye-piece (positive) G I, and the position of the object-lens L. K is a rectangular frame, across which are placed two spider lines, at the common focus of the eye-piece and the object-lens. M H, divided round its circumference into 60 equal parts, is the milled head of a screw shown at S C in fig. 36; and *a b* is a section of the rigid bar,

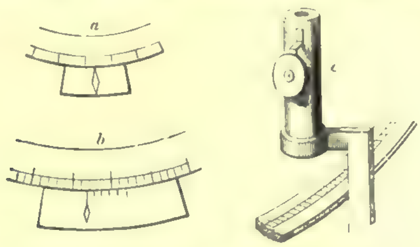


Fig. 33.—Simple index, *a*; Vernier index, *b*; the Reading Micrometer, *c*, and the Graduated Circle.

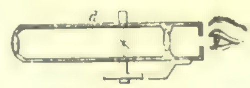


Fig. 34.—Interior of the Reading Micrometer.

or other fixed support to which the micrometer is firmly attached. Fig. 36 shows a part of the graduated circle C *d d d*, in the field of view of the microscope, with the position of the frame K (fig. 35) and the milled head M H. In the field of view is shown also the cross-wires of the micrometer; and it can easily be seen that by turning the screw the inside frame bearing the cross-threads will be carried across the field of view so that the intersection will travel along the graduated circle in either direction as may be desired, within certain limits of distance sufficient for the observer's purpose. Matters are so arranged, by suitable adjustment of the eye-piece and object-lens (often requiring repeated trials) that five revolutions of the screw will just carry the inter-

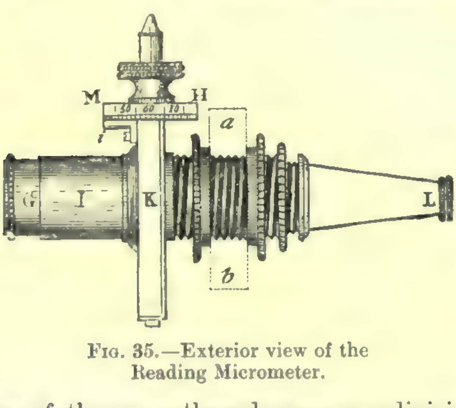


Fig. 35.—Exterior view of the Reading Micrometer.

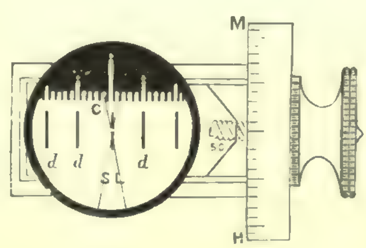


Fig. 36. Field of view of the Reading Micrometer.

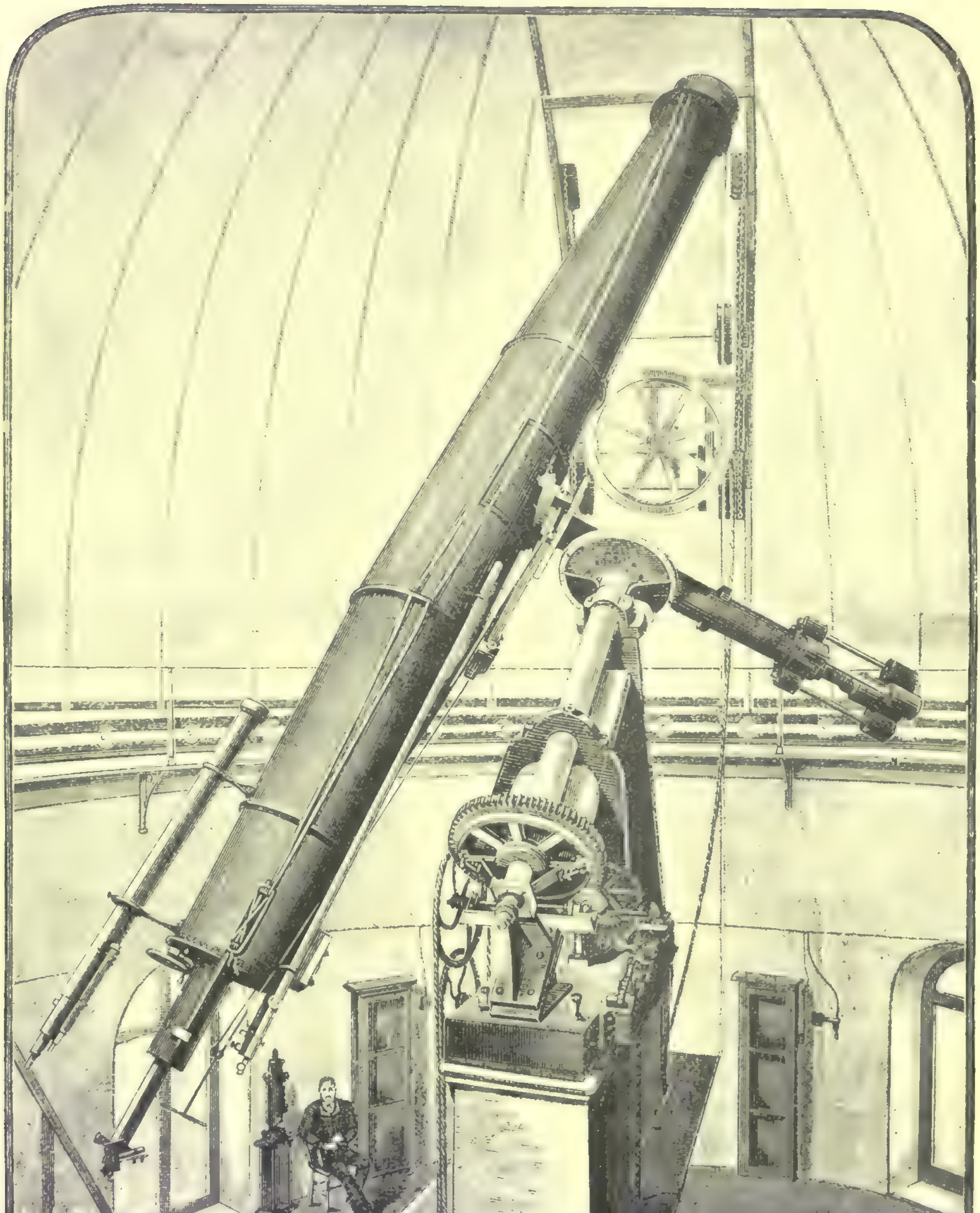
section of the cross-threads over one division of the graduated circle, representing 5′. One revolution, then, corresponds to 1′, and as the circumference of the head is divided into 60 equal parts, it follows that a movement of the screw-head through one of these divisions, (measured by an index-mark at *i*, fig. 35) gives the cross-threads a movement of 1″ along the graduated circle. Thus it will be easily seen that by noting first the reading of the graduated circle, as seen through the microscope, for the 5′ division next beyond which the cross-threads lie, and then turning the screw-head (supposed set originally, as it should be, with the zero-division exactly opposite the index) until the intersection of the cross-threads is carried back to that division, we can read the

circle with great accuracy. Suppose, for example, the reading is 72° 10′ *plus* the distance at which the intersection of the cross-threads lies beyond the division marking 72° 10′, and that to carry the cross back to that division, the head of the screw must be turned round 4 times *plus* an amount shown by the index to be 33 divisions and 3-10ths. Then we read the circle as indicating 72° 14′ 33·3″.

(140.) A vernier may be used with advantage in reading the indications of the screw-head; that is the index *i*, fig. 35, may be provided with a vernier. And a magnifying glass of small power may be used in reading the vernier. Theoretically, indeed, one might read the divisions of the screw-head with a microscope, or use micrometer on micrometer, or adopt methods of even more minute measurement. But we gain nothing by carrying such measures beyond a certain point. For the graduations themselves which we have to read cannot be made with more than a certain degree of accuracy; and it is idle to attempt to read to within thousandths of a second graduations which cannot be trusted within hundredths or perhaps tenths.

(141.) Such are the leading features of that amazingly exact system of measurement which modern astronomy applies to determine the positions and movements of the heavenly bodies. The work of ancient astronomers seems rough and even coarse by comparison; yet it is not less worthy of respect, seeing that they used the methods available to them patiently and resolutely and thus prepared the foundation and the scaffolding of modern astronomy. Even more remarkable, however, than the development of instrumental methods and devices in modern astronomy, has been the system of cross-questioning to which the results obtained by telescopic observation and micrometric reading are exposed before their evidence is accepted. The modern astronomer does not accept the evidence given by a celestial body respecting its position till he has sifted from it the false evidence given by the air (correcting this very sifting, for the varying effects of heat, moisture, elevation, and so forth), till he has taken into account the influence of our earth's motion in modifying the apparent direction of light-rays reaching her from outside, till he has considered the effect of our earth's reeling and nodding (precession and nutation), besides a multitude of minute corrections and movements depending on the movements of the earth's crust either because of subterranean action or local displacements. Even when all such corrections have been made to the best of his ability and knowledge, he retains a mental attitude of fitting scientific doubt, as recognising the possibility, nay the probability, that other corrections still remain to be made of which the astronomy of to-day has as yet no clearer conception than the astronomy of ancient times had in regard to the corrections for the aberration of light, or for terrestrial nutation, or for the proper motions of the stars.

PLATE II.



THE INTERIOR OF A MODERN OBSERVATORY.

CHAPTER II.

ANCIENT AND MODERN STUDIES OF THE EARTH'S SHAPE.

(142.) Astronomers in the course of their study of the heavenly bodies from different parts of the earth's surface, accumulated observations by which the various facts of astronomy as understood now were disclosed gradually, but not in any systematic or orderly sequence. The rotundity of the earth began to be recognised simultaneously with the laws of the apparent motions of the sun, the moon, the planets, and the stars. Measurements of the earth regarded as a globe were in progress simultaneously with exact investigations of the details of the movements of the heavenly bodies. The range of observation and inquiry over which astronomers had to work was so vast, even in those earlier days, the different departments were so intermixed, and each department was so extensive and so difficult, that it was not possible to undertake separately the inquiries necessary for each and to carry them to a final issue. It is necessary to study these matters separately, however, if we would have clear ideas not only of each individually but of their mutual interdependence. A certain order, which must necessarily be artificial—since it cannot correspond with the actual order of discovery—must be selected at the outset. The order followed here has been adopted as that which on the whole seems to correspond best with the requirements of the student of the astronomy of to-day.

(143.) First, the astronomer has to determine the figure of the earth on which he lives and from which his observations are made. He is supposed, however, to have already ascertained that, from whatever place he observes the heavens, the stars are carried around in the manner already described, as if they were fixed points inside a great enclosing sphere turning on an axis through the observer's station, and inclined at a certain angle to the horizon, so that one pole or unmoving point of the spherical surface is above the observer's horizon, the other being below.

(144.) The first point to be determined is the apparent position of the pole of the heavenly sphere, when the astronomer occupies different stations

on the earth. We can imagine an astronomer of Babylon visiting Egypt, and inquiring there whether the polar axis of the heavens around which all the diurnal movements take place, and whose inclination in the north-and-south plane (or meridional plane) he had carefully ascertained at his Babylonian station, is the same at the chief observing station of his Egyptian fellow-workers. We know from the Great Pyramid that the position of the pole was very carefully determined in Egypt; and we have good reason to believe that the astronomers who did the work were either from Mesopotamia, or were well acquainted with the astronomy of Mesopotamia. Hence we may fairly suppose that among the indications men obtained of the earth's figure, the difference between the star sphere as seen in the latitudes of Babylon and of the Great Pyramid were the earliest and most decisive.

(145). The latitude of the Babel Observatory near Babylon being about 32° 20′, and that of the Great Pyramid about 29° 59′, it follows that the direction of the pole of the heavens at the former station is inclined to the horizon at an angle exceeding by rather more than $2\frac{1}{3}$ degrees the corresponding angle at the latter. This, to observers so skilful as the builders of the Great Pyramid, would be a very obvious and indeed measurable difference, corresponding to more than four times the apparent diameter of the sun or moon. Moreover, as it would be found that at both stations the stars were carried with the same uniformity of motion round the polar axis, it would be certain from such observations alone, that the horizon plane at Babylon was not the same as the horizon plane at the Great Pyramid. *But for this*, it might have been possible to suppose that while B P (fig. 37) was the position of the

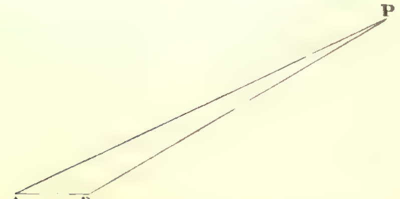


Fig. 37.—Illustrating the different directions of the Pole of the Heavens at different stations.

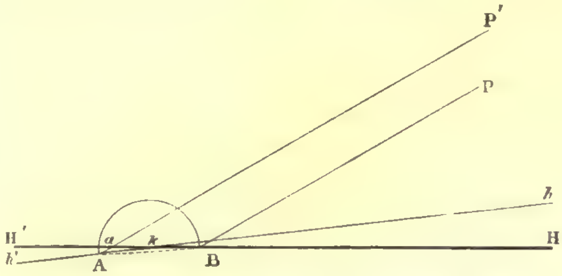
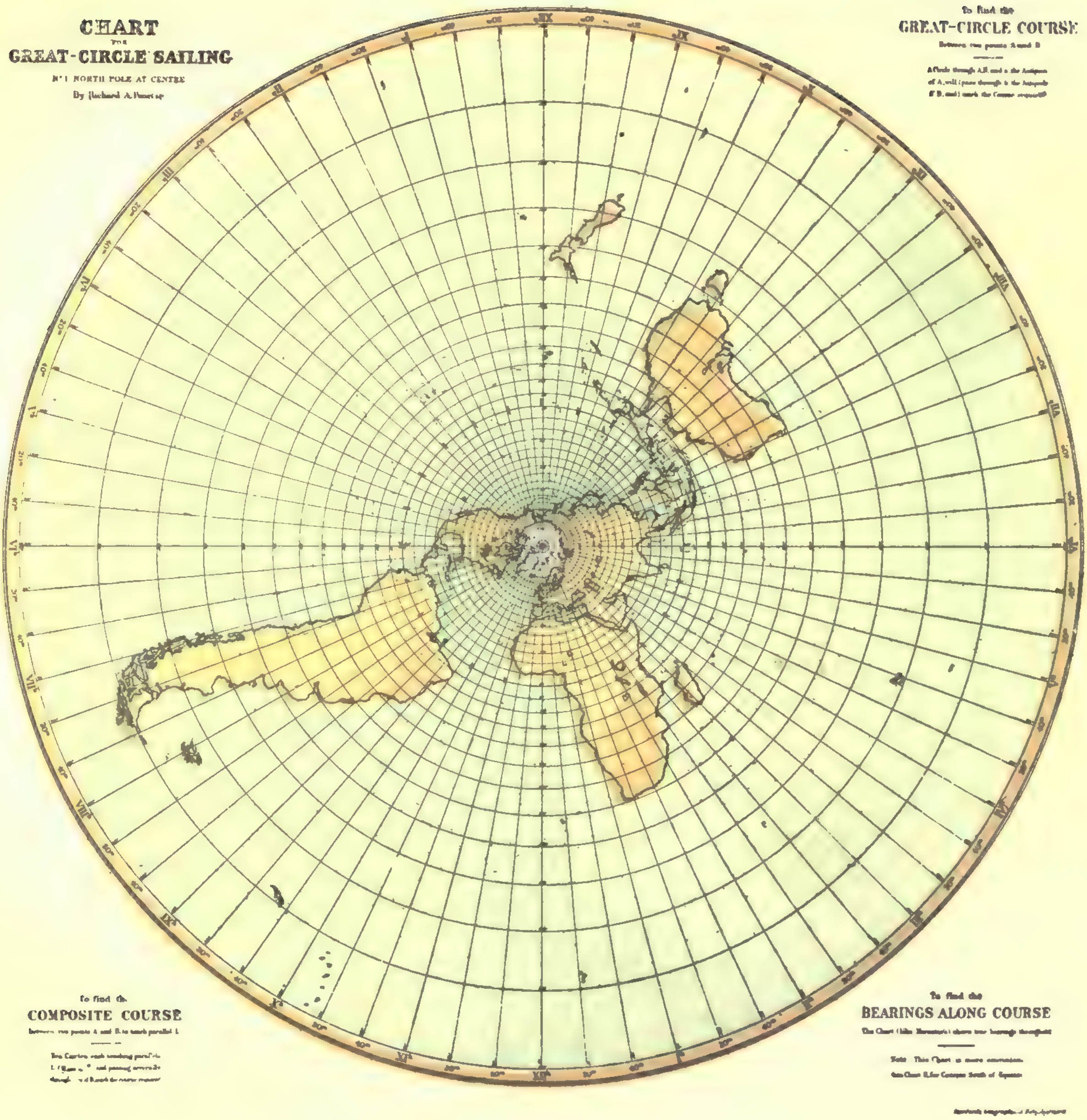


Fig. 38.—Interpretation of the difference of Polar Altitude at different stations.

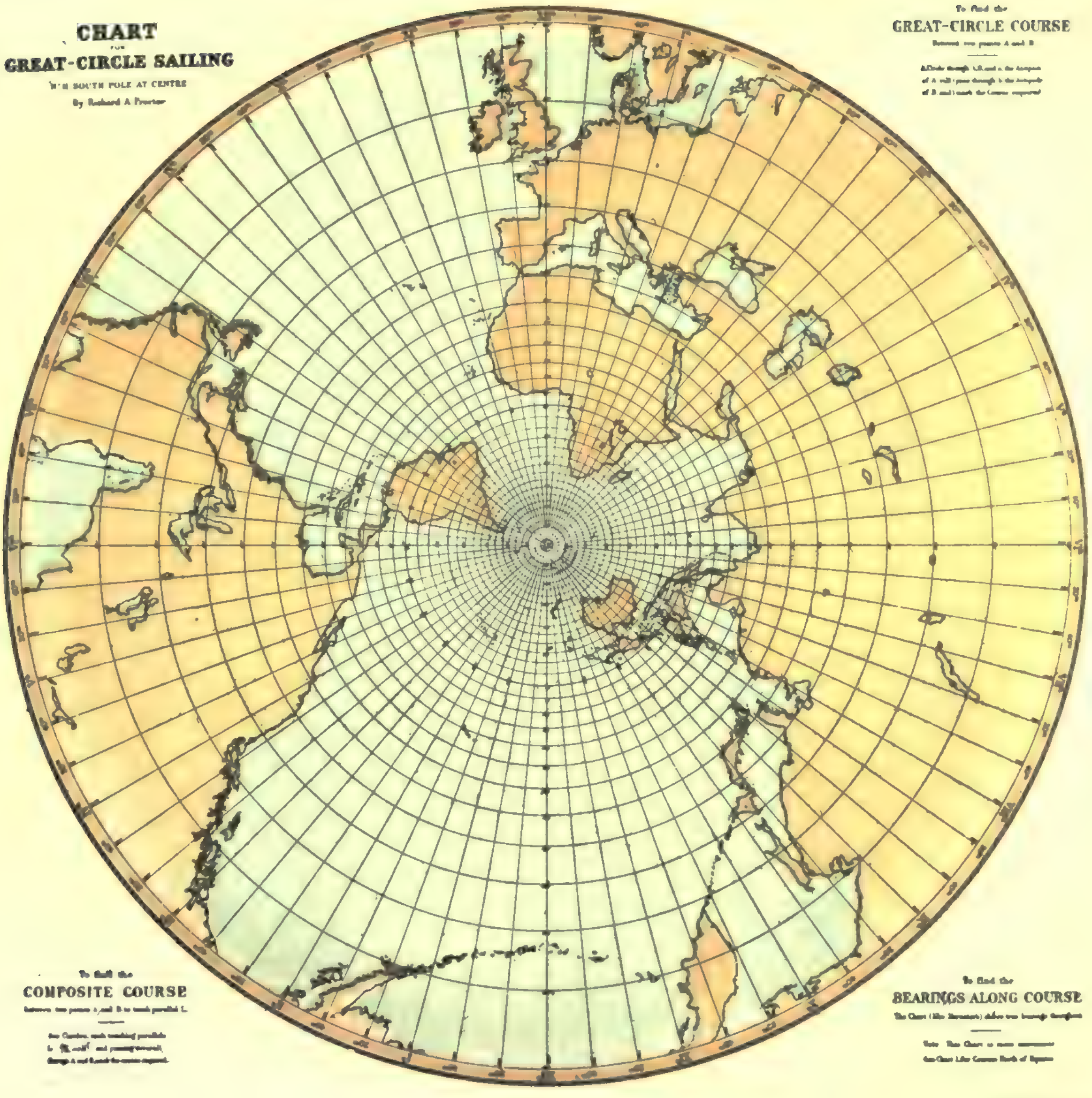
polar axis of the heavens at Babylon, B, A P was the position of the axis at the Great Pyramid A. But skilful mathematicians, as the astronomers of Babylon and Egypt undoubtedly were in those days (probably some 3,400 years before the Christian era), were well aware that the star-sphere could not rotate, or seem to rotate, about two axes inclined to each other, like B P and A P, at a measurable angle. They would see, in fact, that the difference

PLATE III.



STEREOGRAPHIC CHART OF EARTH (NORTHERN).

PLATE IV.



STEREOGRAPHIC CHART OF EARTH (SOUTHERN).

in the apparent direction of the polar axis of the star-sphere could not possibly indicate a real difference of direction, and therefore could not be explained as in fig. 37. Nor would they have long failed to perceive that the real explanation must be such as is suggested in fig. 38. Here H B H′ is supposed to represent a north-and-south horizon line through the Babel Observatory, B P the position of the polar axis of the star-sphere as observed *there.* The Great Pyramid cannot be anywhere in H B H′, as for instance at *a*, because wherever we suppose the Great Pyramid, the polar axis there must be in such a position as *a* P′ parallel to (or—which is the same thing—in the same direction as) the axis B P; and if the pyramid were at *a* the inclination of the polar axis would be P′*a* H, or the same as the inclination observed at B, which astronomers had found not to be the case. Evidently we must suppose the pyramid somewhere, as at A, below H B H′, its horizon plane being as *h* A *h*′, so that the inclination of the polar axis is P′A *h*, an angle smaller than P B H, as observation requires.

(146.) Already, then, the astronomers of Egypt and Babylon would have ascertained that the earth cannot be a plane, but that in passing from one station to another the astronomer travels below the horizon plane of the point from which he set out. For we note that if the Egyptian had set out from A, to compare the conditions existing at Babylon, B, with those at the Great Pyramid, he would have found B below his horizon plane *h* A *h*′, just as his Babylonian brother would have found A below the horizon plane H B H′ of Babylon.

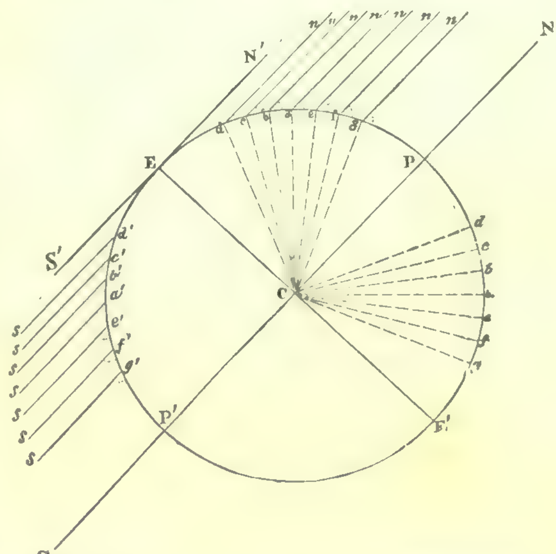


FIG. 39.—Illustrating the effect of north-and-south journeys on the Earth.

(147.) The idea would at once be suggested that at any rate in travelling northwards or southwards — the only part of the travelling between Babylon and the Great Pyramid which affects the apparent position of the pole—the voyager is pursuing a *curved course*—not a course such as B *k* A, fig. 38. And this would be confirmed when, in traversing a southerly course as a b c d from a, fig. 39, the pole was found steadily passing lower and lower, as shown by the diminishing angle between the polar directions and the curved arc d c b a; and when, returning to a and travelling

northwards along a e f g, the pole was found to be as steadily and uniformly rising. The idea conveyed would be that, passing further south, as to E, the pole would be brought actually to the northern horizon, as in direction E N′, the opposite pole appearing now on the southern horizon, as in direction E S′; and that, on travelling still further south, the northern pole of the star-sphere would pass below the horizon, the southern steadily rising. It would further be suggested that by travelling north beyond g the north pole would rise higher and higher above the horizon, till at last it would be overhead when the observer had reached such a position as P.

(148.) It would not thus be proved that the earth is a globe; for other solid figures besides the globe have circular sections such as P a E in fig. 39. For instance, the earth might be cylindrical (or drum-shaped) or cone-shaped, so far as such observations as have hitherto been considered were concerned; and indeed we find that some among the ancient astronomers considered that the earth might be drum-shaped. Still such ideas could never have commended themselves to mathematicians, as the ancient Babylonian and Egyptian astronomers undoubtedly were. For, after even only such observations as I have described already, it would be manifest that, if the earth were cylindrical, the axis of the cylinder must be at right angles to the polar axis of the enclosing star-sphere. We see this from fig. 39, where if we suppose P E P′ E′ a circular section of a cylinder, the axis would be as seen endwise at C, as a point, or at right angles to P P′. Clearly there would be something altogether improbable in the conception of the earth as having an unsymmetrical relation, such as this, to the enclosing sphere. We need not be surprised therefore to find that the ancient Egyptian astronomers, and therefore probably the Chaldaeans, regarded the earth as a globe, whose centre was the centre of the star-sphere.[1]

(149.) But without some means of determining time very exactly, the observers of those days could not prove that the earth is a globe. For journeys east and west do not alter the aspect of the star-sphere like journeys north and south. The elevation of the pole of the heavens remains unchanged by east-and-west journeys, and when any given star is rising at

[1] Diogenes Laertius, *In Proœmio.* Manilius, in the first book of his *Astronomicon*, ver. 179, shows clearly that in his day the position of the earth as a globe (though he does not mention the shape) in mid-space, was clearly recognised:—

Nunc quia non imo tellus dejecta profundo,
Sed medio suspensa manet, sunt pervia cuncta,
Qua cadat, et subeat cælum, rursusque resurgat.

The unchanging forms of the constellations—apart from the slight effect of refraction—as they approach and pass below the western horizon, and as they rise above the horizon in the east, show observers at even a single station that the surrounding star-strewn regions are carried round the comparatively limited region occupied by the earth, unless (which equally explains the observed appearances) the earth itself is turning on an axis inside the much vaster region within which the stars are strewn. On either view, the idea seems natural that the earth's figure is globular.

one station, the aspect of the star-strewn heavens is precisely the same there as it is when the same star is rising at some other station due east or due west. The only difference is in the time of the star's rising: and to recognise this an observer at one station must have some means of determining exactly what time it is at the other station; and this can only be done, at least when astronomy is young, by the use of some such time-measuring instruments as chronometers, which can convey the time of one place to another place. The ancients had no such instruments.

(150.) But assuming the earth to be a globe, as the ablest astronomers and thinkers naturally would and did, the north-and-south observations already described would suggest a ready way for determining the size of that globe:—

The method adopted by Eratosthenes (though doubtless it had been employed centuries before by Chaldean astronomers) was based on observations of the sun, which, however, in this inquiry might be regarded simply as a convenient index on the star-sphere. At the summer solstice the sun is moving nearly parallel to the equator, at his greatest distance (about 24° in the time of Eratosthenes) from that circle; and therefore at noon when due south, the midsummer sun indicated the exact direction of a celestial body at a known distance from the pole. Now Eratosthenes found that at Syene, the modern Assouan (latitude 24° 5′ 30″ S), the midsummer sun was vertical at noon, or in direction *s* Z, fig. 40, C *s* Z being drawn from the earth's centre C); whereas at Alexandria (latitude 31° 11′) the midsummer sun at noon was below the zenith towards the south by one-fiftieth part of a circumference, or by 7° 12′—which, considering the comparatively rough nature of his observations, was very fair work.[1] This angle is represented by *z* A S in fig. 40, where C A *z* is drawn from the earth's centre towards the zenith *z*, and A S is supposed to represent a line towards the distant sun, a line appreciably parallel to S Z. Thus since the angle *s* C A is equal to the angle S A *z*, it follows that the arc S A between Syene and

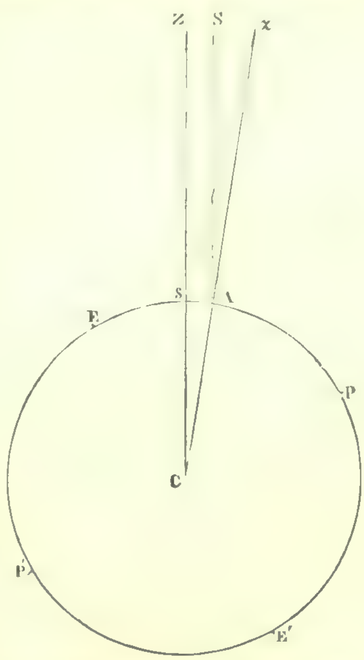


Fig. 40.—Illustrating the measurement of the Earth by Eratosthenes.

[1] He set Syene on what is now called the tropic of Capricorn, which, in his day, according to his own measurements, was in latitude 23° 51⅓′. Thus he set Alexandria in latitude 31° 8⅓′. Such results from mere observations of a noonday shadow were very fair.

Alexandria is one-fiftieth of the circumference of the earth. Eratosthenes estimated the distance between Syene and Alexandria (which, nearly enough for such observations, are in the same longitude) at 5,000 stadia, and therefore concluded the circumference of the earth to be 250,000 stadia. The best estimate we have of the Greek stadium assigns to that measure a length of $606\frac{3}{4}$ English feet, whence it would follow that the circumference of the earth as determined by Eratosthenes has a length of 28,728·7 miles, which for him was a fair measurement, though considerably in excess of the truth.

(151.) We may regard such observations and measurements as these, with the experience derived from long journeys, as giving such sufficient proof of the earth's rotundity as the best informed astronomers of old had obtained, and as we need alone consider here. The voyages beyond the equator which Herodotus mentions, when he tells of travellers who saw the sun move from right to left across the sky, instead of from left to right, must have satisfied astronomers, at any rate, that the earth is globular in form, and surrounded on all sides by star-strewn space. For in such journeys men became acquainted with the whole surface (so to call it) of the star-sphere. Indeed at the equator, in the course of a single night of twelve hours, the whole of the stellar sphere could be seen, were it not for the star-concealing light of the morning and evening twilight. During a month, in any part of the year, at the equator, all the star-groupings, over the whole celestial sphere, would become visible ; and no doubt would be left in the mind of any reasoning observer that the earth is enclosed on every side by stellar regions presenting the appearance of the inside of a hollow sphere strewn with stars, and turning once round the earth in four minutes less than an ordinary day.

(152.) At this stage, many parts of the earth which had been surveyed, would have shown the globular form recognised when such surveys began. And in those days, when men were possessed, naturally enough, with the idea that throughout nature regular forms exist wherever possible, no doubt would remain that the earth is a perfect sphere.

(153.) We need not be prevented from recognising that this must have been the opinion of astronomers and men of science, by the consideration that a number of other ideas were entertained then and till much later times. Various ideas were suggested by those who were not astronomers or mathematicians ; just as similar fancies would be suggested now, but for the general spread of scientific knowledge among those who have no time, or perhaps capacity, for independent scientific research. We learn from Cleomedes ('De Mundo,' lib. 1), what we might have guessed without his testimony, that some considered the earth a plane; others thought its figure that of a cube, or a pyramid or a cylinder. The heavens were regarded generally as a

vast hemispherical vault, with its base resting on a circular continent beyond the ocean encircling the earth.[1] In the ancient poems called the Chaldæan Oracles (but not known to represent Chaldæan philosophy) we are told that 'The All-Father made seven orbs, enclosing each in a globular form' (not the Sun, Moon, Mercury, Venus, Mars, Jupiter, and Saturn, but the orbs or spheres carrying these seven planets of the ancients); 'he made the great host of the fixed stars; he also placed the earth in the middle, the waters within its bosom, and the air above it.'

(154.) The exact figure of the earth will be considered further on, as will also those phenomena and observations on which its determination depends. But it will be well to consider here those phenomena which show, even at a single station on the earth, the rotundity of the earth's figure.

(155.) Since irregularities like mountains, hills, valleys, ravines, and so forth, necessarily prevent an observer from recognising the uniform rotundity which belongs to the earth's figure regarded as a whole, we must seek some place where either there are broad tracts of level ground such as we find in prairies, or better (since we never can be sure that even the most seemingly level prairie has not had its surface rounded independently of the general rotundity of the earth) we must take for our observing station some place near a widely extending water surface.

(156.) The dweller near the sea has evidence in the darkened outline often presented by the horizon that the surface of the water is not plane. Were it so, it is obvious that he would see the sky and the earth mingling by indefinite gradations, and, so to speak, lost in one another. The outline of the sea, which would be his true horizon-line, would be hidden by the extended atmospheric medium through or rather into which the eye, directed horizontally, would be looking. But in clear weather the sea-line is very strongly and sometimes even darkly projected against the sky.

(157.) The sea affords other evidence. Ships and rocks which, if the sea had a plane surface, would never disappear except through the effects of interposed air, are lost to sight at sea, in a different way, as our distance from them increases. We lose sight of the hull of a ship while the upper part is still clearly visible. As its distance increases we lose sight of more and more of the ship, but always from below upwards; and we finally lose sight of every part, not through the effect of distance (for, were it so, a telescope would bring the ship into view again) but by the obvious interposition of the rounded or domed surface of the sea between us and the ship.

(158.) As commonly presented in popular treatises on astronomy, however, the proof of the earth's rotundity afforded by the appearance of ships as they pass over the horizon-limit and beyond, is apt to perplex, though sound enough in itself. A rounded hill of water is shown, over the top of which a line of sight is carried from an observer

[1] In the Eleventh Orphic Hymn (10) ver. 15, we find the encircling ocean referred to in the line:

'Ωκεανός τε πέριξ ἐνὶ ὕδασι γαῖαν ἑλίσσων;

and in the Eighty-second Orphic Hymn (ver. 7), the engirdling continent is referred to in the line:

τέρμα φίλον γαίης, ἀρχὴ πόλον ὑγροκέλευθε.

There are reasons for supposing that in the account of the Shield of Achilles given in the Iliad, and of what is called the Shield of Hercules (evidently belonging to the same original poem), the ancient poet described a Zodiac temple in which this arrangement was presented.

as at A, fig. 41, a departing ship being shown in several positions as at 1, 2, 3, 4, fully seen at 1 and 2, hull down at 3, and showing only the upper part of her mast at 4, to the spectator at A. In such pictures the depression of the line of sight A *a* touching the convexity of the water, or the angle *a* A *t* which it makes with the horizontal line A *t*, is so considerable as to be obvious. The student is thus taught two things—one true, the other untrue. He learns correctly enough how and why a

FIG. 41.—Much exaggerated effects attributed to the Sea's curvature.

ship disappears beyond the convexity of the sea; but he is also taught what is not correct, viz. that the sea-horizon dips observably below the true horizon, and that the depression of the sea-horizon becomes obviously greater as the observer's height above the sea-level increases. For in fig. 41, the angle between the true horizon-line A *t*, and the depressed line A *a* to the apparent horizon, is one of several degrees. At the seaside there is no such visible depression; indeed, when the observer is a good deal above the sea-level the sea-horizon appears higher (by an optical illusion, but still very strikingly) than when he stood on the sea-shore. Thus the student is perplexed by the contrast between what he has been taught and what he sees.

(159.) In like manner an illustration such as that in fig. 42 is misleading unless it be clearly stated that the vertical dimensions are greatly exaggerated. So understood it illustrates fairly the evidence of the earth's rotundity given by the varying appearance of the seascape as an observer ascends a cliff such as A *a a'*, viewing from various heights a ship such as *s*, or a distant shore such as B *b b'*. Or the observer may be supposed to climb the cliff B *b b'*, and thence to view the ship *s* and the cliff *a' a* A.

(160.) The evidence of the earth's rotundity obtained in such cases as are illustrated in fig. 42, are striking and convincing, because of the differences of distance involved.

FIG. 42.—Effects of the Earth's curvature.

Thus if *a* (fig. 42) be the place of an observer, A the sea-level beneath him, and *a* P *b* the line of sight touching the sea-horizon at P, and extending onwards to the cliffs at *b*, we are not merely convinced but perceive that P is much nearer than *b*; for we find that the sea-horizon at P is seen much more clearly than the cliffs at *b*. We know then that the surface must be rounded above the straight line A B in order to have this relative nearness at P.

(161.) But an even more effective proof of the rotundity of the water-surface may

be obtained by using a powerful telescope at a station such as *a*, fig. 42, and directing it upon the horizon-line at P toward a ship as at *s*. It will be found that when the telescope is focussed so as to show the sea-horizon distinctly, the masts and sails of the ship *s* are seen indistinctly. Fig. 43 gives an idea of what is seen. To bring the ship sharply into view, the focussing rackwork must be used so as to draw in the eye-tube, as for a more distant object; and then presently the parts of the rigging in view are seen sharply defined as in fig. 44, while the sea-horizon has become hazy and indistinct. Where a low magnifying power is employed on a large telescope, so that the focal range for different distances is relatively great, this observation is singularly effective. I have never known anyone who has ever tried it, under good observing conditions, without finding that, strong though his faith might already have been in the rotundity of the earth, it was much confirmed and strengthened by this particular observation. As the focussing rackwork is shifted to and fro, bringing the ship

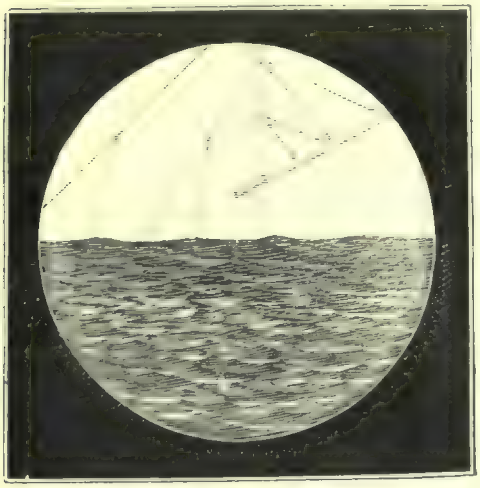


Fig. 43.—Telescopic illustration of the Earth's rotundity. A 'hull down' ship seen indistinctly beyond the sharply-defined sea-line.

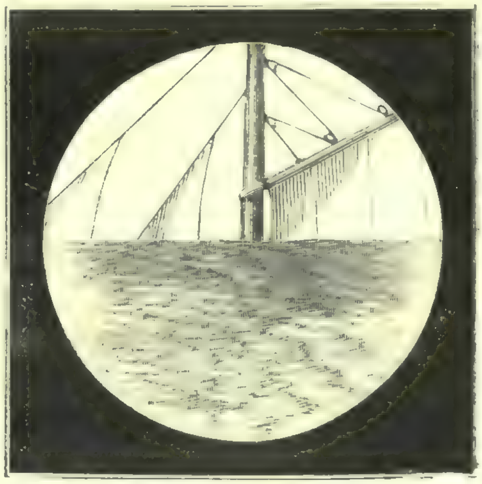


Fig. 44.—The same view with change of focus; the 'hull down' ship seen distinctly beyond an indistinct horizon.

into focus and out of focus while the horizon passes out of focus and into focus, the observer seems to *feel* that the sea-surface rises in a bold sweep between him and the more distant object seen in the same field of view.

(162.) But this way of recognising the sea's rotundity may be improved upon :-

Let us suppose that our observer starts from A, fig. 42, to climb up the cliff (along the roadway will do very well) till, after passing the level *a*, he reaches the summit of the cliff at *a′*. When he is at A the line of sight to the sea-horizon meets the sea-curve close by as at *p*, and the clearness of the sea-line, supposing the observation made in good observing weather, is very striking; as is also the contrast between the sharp definition of the sea-surface at *p* and the haziness of the cliff at *b′* if that is visible at all, and (in less degree) that of a ship at *s*. If the telescope is used as in the experiment just described, at such a station as A, the amount of focussing required to correct from a clear sea-horizon into well-defined ship-rigging, or *vice versâ*, is much greater than when the telescope is set higher above the sea-level.

(163.) When the observer has reached *a*, the sea-horizon has retreated to P, and is less distinctly seen, though it is very marked against the distant cliff *b b′*. But when

passing onwards to a', he brings the sea-horizon to B or even farther from him, so that he sees to the very foot of the cliff, then the sea-horizon on either side of the cliff B b b' (which we suppose to be a cape projecting towards him) is no longer seen to be freer from the effects of haze than the cliff's face at b. If the observer has noted the sea-horizon from time to time during his ascent he will have seen (presuming the weather remains tolerably constant) that the sea-horizon gets lighter and lighter in tone as he passes higher and higher, showing that it passes farther and farther away and is thus more and more affected by the presence of any haze that may be present in the air. For, even in the clearest weather, there is always enough haze near the sea-level to affect the distinctness of the sea-horizon, when, owing to the observer's ascent, it is thrown twenty or thirty miles away.

(164.) The actual amount of depression of the sea-horizon for any given distance may be readily determined by geometrical considerations based on the known dimensions of the earth; or a reverse process might be employed, and the dimensions of the earth determined, somewhat roughly, by the observed actual depression for given distances.

(165.) Thus, suppose t, fig. 45, a telescope (or other observing instrument by which a direction line may be given, as by looking through a tube or rings) directed towards an

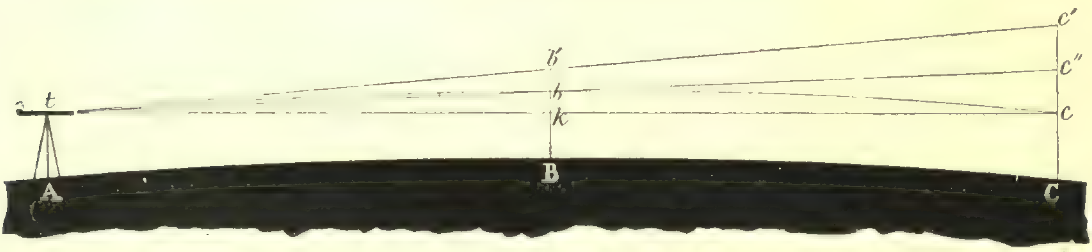


FIG. 45.—Measuring the Earth's globe from its observed curvature.

object c, so situate that its height C c above the surface of water is exactly equal to the height A t of the telescope above the same surface; and let B k b b' be an upright carefully divided into inches and parts of an inch, set up at B, exactly midway between A and C, or one mile from each. Suppose B b exactly equal to A t or C c; and let k be the point where the line of sight $t\,c$ crosses the upright B b'.

Then $t\,b\,c$ is part of a circle about 7,920 miles in diameter, $t\,c$ is a chord 2 miles long, divided into equal parts in k. Hence by a familiar property of the circle the square on $t\,k$, is equal to the rectangle under $b\,k$ and the earth's diameter (less $b\,k$, but $b\,k$ by comparison may be neglected). That is to say, using miles for the unit of length,

$$7920 \times b\,k = 1 \times 1 = 1$$
$$b\,k = 8 \text{ inches almost exactly.}$$

(166.) The actual depression of the arc $t\,b\,c$ below the true horizon-line $t\,b'c'$ is $b'b$, which is obviously equal to $b\,k$, in so flat an arc—where $t\,b'$ and $t\,k$ are practically equal. (Or we might have begun by saying the square on $t\,b'$ is equal to the rectangle under $b\,b'$ and the earth's diameter). The depression for one mile, determined geometrically, amounts to 8 in.; and obviously the depression $c\,c'$ for two miles is four times 8 inches, or 2 ft. 8 in.; for 3 miles the depression is 9 times 8 inches, or 6 feet; and so on—within such greater distances as have to be considered in ordinary observations. Thus, up to

100 miles, it may be said that the depression, estimated geometrically, is equal to 8 inches multiplied by the square of the number of miles.

(167.) But as actually observed the depression is considerably less than this. The layers of air above the earth's surface, being curved like that surface, the lines of sight, like rays of light, are slightly curved in their passage through the air. Thus, if P (fig. 46) be a point on the earth's surface, from which a line touching the surface would pass along the true horizon plane to a, b, and c, then a line of sight directed towards c, from P, would in reality pass along such a curve as P a' b' c', the point a' being thus raised to the horizon, or to the apparent position a; b' would be raised to the apparent position b; and c' to the apparent position c. The depression, then, of A, B, and C below the line of sight P $a'b'c'$, will be A a', B' b', and C c' respectively, instead of A a, B b, C c. Moreover it is obvious that refraction correspondingly diminishes the depression of the horizon as seen from any point raised above it.

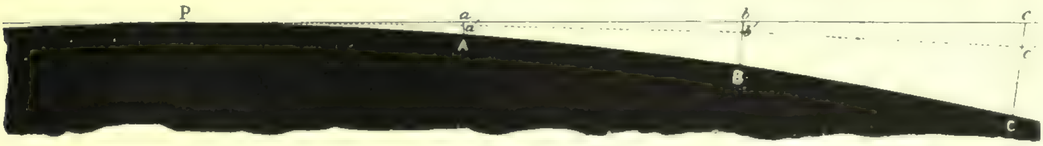


FIG. 46.—The effects of refraction in diminishing the apparent curvature of the Earth.

For the line of sight P $a'b'c'$ from P to c' is also the line of sight from c' to P; and it is clear that $c'b'$, the direction in which an observer at c' sees the horizon at P, is more inclined to the vertical C c', and therefore less inclined to the horizon, than a tangent from c' to P A B C would be. It is also to be noticed, as a point which has to be taken into account in many astronomical inquiries relating to the earth, moon, planets, and even the sun (as my discussion of sun-spots will show), that, owing to refraction, an observer at c' or any point further away (on the right) sees more of the arc C B A P than he otherwise would.

(168.) This effect of refraction varies measurably according to the density of the air. It is greater when a high barometer shows increased atmospheric density, and *vice versâ*. At average atmospheric pressure, and supposing P A = A B = A C = 1 mile, A a' $= 6\frac{2}{5}$ in. (instead of 8 in); B $b' = 4$A $a' = 2$ ft. $1\frac{3}{5}$ in.; C $c' = 9$A $a' = 4$ ft. $9\frac{3}{5}$ in.; and so forth. But sometimes atmospheric refraction diminishes the geometrical depression by as much as a fourth or even more, sometimes by barely a sixth or even less. For ordinary rough calculation it suffices to put 6 in. for the optical depression due to one mile.

(169.) It is easy to deal with any problem requiring us either to find the distance of the sea-horizon for any given height of the eye above the sea-level; or, *vice versâ*, to determine what is the height of the eye when the sea-horizon lies at a given distance.[1]

[1] Thus, if d is the optical depression for one mile, D the depression at a distance of n miles, we have $D = n^2 d$; $n = \sqrt{\frac{D}{d}}$. Since d may usually be taken equal to half a foot, we have a very simple numerical relation to deal with when—as usual—the depression is given in feet.

Suppose, for instance, the problem—*If the eye is 20 feet above the sea-level, how far off is the sea-horizon?*

Here we have, taking a foot as the unit of length, and $d = \frac{1}{2}$ foot –

$$n = \sqrt{20 \div 2} = \sqrt{40}$$
$$= 6\tfrac{1}{3} \text{ approximately.}$$

If we had put $d = 6\frac{2}{5}$ inches, we should have had, taking the inch as the unit of length—

$$n = \sqrt{240 \div \tfrac{32}{5}}$$
$$= \sqrt{\tfrac{1200}{32}} = \sqrt{37 \cdot 5}$$
$$\text{or } n = 6\tfrac{1}{9} \text{ approximately.}$$

The varying effects of atmospheric refraction appreciably affect the results of such calculations. (See preceding note.) As, however, the calculations are never required for *exact* measurements of heights and distances, this is a matter of little importance. In geodetical surveying, where exact levelling is required, levels are never taken over distances which can be appreciably affected by changes in atmospheric refraction. In rough surveying, indeed, over short distances, not only may the effects of such changes be neglected, but the difference between the optical and geometrical depression may be overlooked, or even the depression itself disregarded. For instance, in a distance of 88 yards or $\frac{1}{20}$th of a mile, the depression (even considered geometrically) is only $\frac{1}{400}$th part of 8 inches, or the $\frac{1}{50}$th part of an inch. Taking this twenty times we get only $\frac{2}{5}$ths of an inch of error in taking levels over a mile thus subdivided. Thus in ordinary levelling, unless great exactness is required, the effects of the earth's curvature may be neglected. In back-and-fore-sight levelling, where the observer takes levels from a station midway between two others, and compares *their* level, no correction at all is required; for, whatever depression there may be for the station on one side of the observer, the same depression is there for the station on the other side; hence, in comparing the two, no difference whatever arises from the earth's curvature.

(170.) For instance, if B, fig. 48, be a station midway between two others, A and C, we see that, observing along the $t\,k\,c$, the horizontal line through k, we determine

Again, suppose the problem *If the sea-horizon lies 20 miles away, how high is the eye above the sea-level?*

Here, taking the foot as the unit of length, and $d = \frac{1}{2}$ foot, we have

$$D = (20)^2 \div 2$$
$$= 200 \text{ feet.}$$

If we put $d = 6\frac{2}{5}$ inches, we get, instead,

$$D = (20)^2 \frac{32}{5} \text{ inches}$$
$$= 80 \times 32 \text{ inches}$$
$$= 2560 \text{ in.} = 213 \text{ ft. } 4 \text{ in.}$$

Let us next take a class of problem scarcely less simple, but of more general use.

In a letter recently addressed to a daily paper, in 1883, Prof. Tyndall remarked that, going out in the steam-yacht the 'Princess Alexandra' to a distance of 21 miles from the Galley Head Lighthouse, the earth's rotundity coming between them and the shore, the light 'dipped' beneath the horizon. Supposing the eye of the observer to have been 16 feet above the sea-level, what is the height of the Galley Head Light above the same sea-surface?

A common way of getting confused over such a problem is to deal with it as follows:—For 21 miles the depression of the sea-surface below a tangent-line is 8 inches multiplied by the square of 21, or 441 *i.e.* 294 feet. Hence it seems as though the beacon light must have been 294 feet above the sea-level, less only the height of the observer's eye above the sea. Putting this height at 16 feet, we should obtain 278 feet for the height of the Galley Head Light, or about twice the real height.

The problem should be dealt with as follows:

Let L P O (fig. 47) be the earth's surface, l the light, o the place of the observer's eye, $l\,P\,o$ touching L P O at P; let L l be the height of the lighthouse, O o the height of the observer's eye

FIG. 47.—Sea-range of a Signal Light in a lighthouse of given height.

above the sea, or 16 feet. Then putting $d = 6$ in. we have

$$P\,o = \sqrt{16 \div \tfrac{1}{2}} = \sqrt{32} = 5\tfrac{2}{3} \text{ nearly enough;}$$
$$\therefore l\,P = 21 - 5\tfrac{2}{3} = 15\tfrac{1}{3} \text{ miles}$$

and

$$L\,l = 6 \times (15\tfrac{1}{3})^2 \text{ inches}$$
$$= 1{,}411 \text{ inches} = 117 \text{ ft. } 7 \text{ in.}$$

If we had given d its average value, $6\frac{2}{5}$ inches, we should have obtained for the height of the light above the sea-level, at the time of observation, about $120\frac{1}{2}$ feet.

by uncorrected levelling, two points, *t* and *c*, neither of which is at the same height above the mean level A B C as *k* is; yet, as they are both at the same height, no correction is required. On the contrary, were observations made from *t*, we should not determine, by uncorrected levelling, a point *b* at the same height as *t* above A B C;

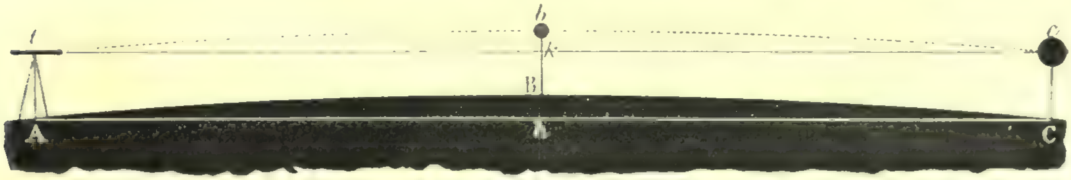


Fig. 48.—Illustrating back-and-fore-sight Levelling and a method of measuring the Earth's curvature.

for a horizontal line at *t*, being tangent to the arc *tbc*, passes above *b*. Our error would be still greater (twice as great measured angularly, and four times as great measured in linear height) if we were levelling from A to C; for the line *tkc*, from *t* to a point *c* at the same height above the earth's surface A B C, is inclined at twice as great an angle as *t b* to the true horizontal line at *t* (tangent to the arc *t b c*).

Fig. 49. Discs of equal height observed from different distances.

(171.) Wherever there is a sufficient stretch of water surface, the student who has leisure can make observations on the earth's curvature in the way illustrated in fig. 48. A telescope may be set up at A either in a boat or at a measured height A *t* above the water's surface; at B may be a boat carrying an upright B *k b* with a disc at *b*, at the same height as *t* above the water-level; and at C another may be set, either in a boat or on the ground at the other side of the water, C *c* being made equal to B *b* or A *t*. It is convenient also to have the disc *c* twice as great in diameter as the disc *b*, because then it appears of the same size as seen from *t*. Looking then from *t* towards *c*, the appearance seen is that shown in fig. 49, *b* being seen measurably above *c*.

(172.) It is clear that, if the surface of level water were plane, the telescopes and discs set up as described would be arranged as in fig. 50, where *t b c* is a straight line; so that the disc *b* would just hide the disc *c* from view.

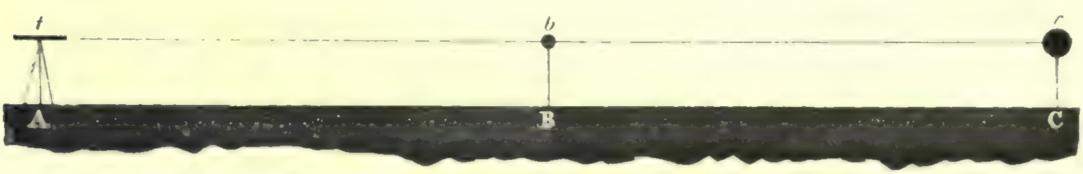


Fig. 50.—Observations on a plane surface.

(173.) One can by means of such observations as these form a rough estimate of the earth's size. Suppose, for example, A B C, fig. 48, is a distance of 6 miles, A B = B C = 3 miles; and let the disc *b* have a diameter of 1 foot. Suppose the rod, *b k* B, divided into feet and inches. Then the same observation which shows *c* below *b*, as in fig. 49, indicates also, by suitable division marks along B *b*, how much *c* lies below *b*, when thus seen—or, in other words, the length *b k*. Suppose we find *b k* thus to be 5½ feet, then our estimate of the earth's diameter would be obtained from the consideration that

$$b\,k \times \text{earth's diameter} = (t\,k)^2$$

or, with one foot for the unit of length,

$$5\tfrac{1}{2} \times \text{No. of miles in earth's diam.} \times 1760 \times 3 = (3 \times 1760 \times 3)^2$$

$$\therefore \text{No. of miles in earth's diameter} = 1760 \times 27 \times \tfrac{2}{11}$$

$$= 160 \times 54$$

$$= 8640$$

which, though considerably too large, would be a very good estimate for so rough a method, applied upon so short an arc.[1]

(174.) Among the effects of the earth's rotundity which are most easily observable, I may notice the two following:

If an observer, situate as at C (fig. 51), looking over the sea-horizon towards a ship as at P or P′, alternately raises and lowers the eye over such a range as $a\,b$, he will find the horizon-line shifting most obviously downwards and upwards over such a range as $p\,q$ or $p'q'$ on the mast and sails of the distant ship. The further off the ship the greater is the effect. So also the effect is greater, the nearer the observer's eye is to the sea-level.

(175.) Again, in the railroads across the prairie regions in America, the track is sometimes perfectly direct and runs on an unchanging level for several miles. Along such parts of the road one can look back from the rear carriage on two parallel lines

Fig. 51.—Illustrating the effect of changes of Level in shifting the Horizontal Line.

of rails appearing as in figs. 52 and 53. Suppose now that, standing up on the rear platform, the eye is as at a, fig. 51, so that the horizon is at A. Then the rails will be seen as at B A, D C, fig. 52, meeting the distant horizon H H′ at A and C. But if now, by stooping, the eye is brought to a considerably lower position, as at b, fig. 51, so that the horizon is at B, then the rails will be seen as $b\,a$, $c\,d$, fig. 53, meeting the less distant horizon $h\,h'$ at a and c—$a\,c$ appearing considerably greater than A C, for the simple reason that it is really the same distance (viz. the breadth between the rails) seen much nearer. (In fig. 52, $a\,c$ indicates the change of distance as compared with A C.)[2]

[1] Observations of this kind in principle were once made at the Bedford Level, on a range of water six miles long, to settle a wager made by a person who, not being able to understand the evidence showing the rotundity of the earth, had been persuaded by a charlatan that the earth's surface is plane. The results were of course such as are indicated above. They did not satisfy the doubter; at which we need not be surprised, since it may fairly be assumed that one who is unable to understand the astronomical evidence of the earth's rotundity must be beyond the influence of evidence in such matters, nor could any explanation avail to make the case clear to him. The power of self-bewilderment shown by the believers in a flat earth, when discussing a result which (comparing figs. 48 and 50) should have been not only convincing, but striking, would be amazing did we not remember that the initial ineptitude they had shown renders their subsequent bewilderment altogether natural.

[2] I have never so thoroughly *felt* the rotundity of the earth as I did on the day (in the spring of 1880) when first this illustration occurred to me. I was on the rear platform of a train running smoothly and swiftly over the prairie region of Kansas. In the clear air of that region the two rails on which we were running seemed to meet the horizon in two sharply defined points like the ends of two slightly slanted rods. Lowering the body the ends drew apart; raising it,

(176.) On the other hand, when we might expect to recognise the effect of change of elevation very markedly, as in climbing great heights, or from the car of a balloon, we are apt to be more than disappointed. Not only does the horizon not appear as much depressed as we expected : it often appears actually raised.

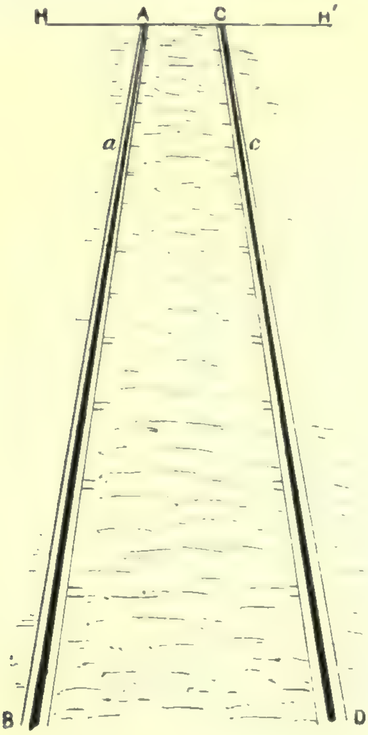


FIG. 52.—Direct Rails on a prairie viewed from above.

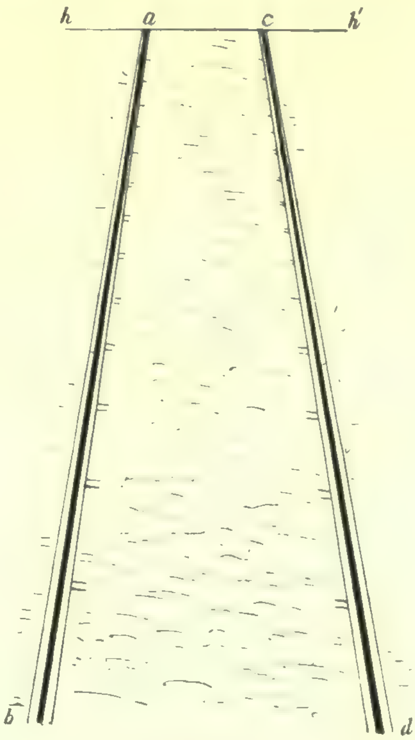


FIG. 53.—The same Rails viewed from a slightly lower level.

(177.) The appearance which the earth presents, for instance, when seen from a balloon, is peculiar, and at first view suggests anything but the idea of a convex surface, such as a globe like the earth might be expected to present. The earth beneath the balloonist appears, in fact, like a gigantic basin, the rim of which is the horizon all round him, while its deepest part lies below him. Mr. Glaisher, the well-known meteorologist and aëronaut, has spoken of this illusion, though of course he was in no sense deceived by it :—

Fig. 54 illustrates the peculiar effect in question ; but I have added to the illusion affecting the aspect of the earth's surface another equally marked, but not noticed because much more familiar, which affects the aspect of the clouds.

(178). The explanation of both peculiarities is the same.

I consider farther on how we are deceived into the idea that the clouds form a sort of dome over our heads, whereas the under surface of a layer of clouds, though slightly arched, is in reality very nearly flat within the range of view commanded by the eye. The eye is not sensible of the much greater distance separating us from the clouds

they approached again ; and alternately raising and lowering the body, keeping the eyes steadily fixed on the horizon, the ends seemed to approach and draw apart as if they were really moved to and fro.

near the horizon than from those overhead; and losing the effect of distance we picture the cloud surface near the horizon as springing almost if not quite vertically from the earth's surface, to arch over, gradually at first and more rapidly afterwards, towards the point overhead. When we view the under surface of clouds from a balloon situated as shown in fig. 54, a similar effect is produced.

(179.) But also, and for precisely similar reasons, a similar effect is produced on the appearance of the earth's surface below us. When we look directly down we see that the earth lies far below us, the greatness of the distance being very obvious and striking. On the other hand, when we look towards the horizon, although the line of

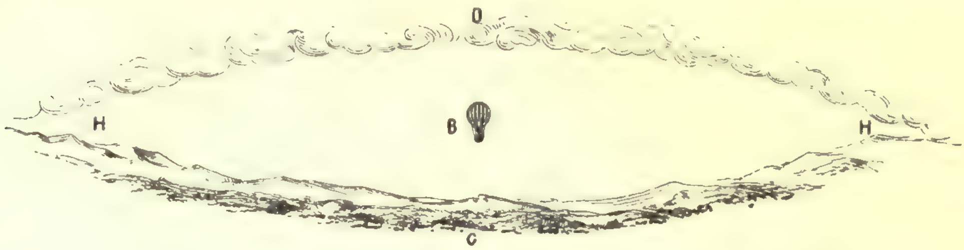


Fig. 54. Illusion affecting the Earth's appearance as seen from a balloon.

sight is really depressed slightly below the horizontal direction, the depression is not at all obvious, even when we are a mile or two above the sea-level.

Say, for instance, we are even so much as two miles above the sea-level. Then the depression of the visual horizon (supposed to be a sea-horizon) below the true horizontal direction, corresponds to the angle subtended by four miles at the distance where the line of sight from a height of two miles touches the sea-level. This distance, neglecting refraction, which increases it, is about 127 miles, and four miles at a distance of 127 miles subtends less than two degrees, which to the eye appears but an insignificant angle. Comparing unconsciously the slight depression of the horizon, with the obvious and startling depression of the earth's surface underneath his car, the aëronaut is deceived into the impression that the surface underneath rises up all around him to his own level or nearly so,—in other words, the illusion of a basin-shaped depression such as is shown in fig. 54 is produced.

(180.) The real state of things is represented, only the earth's curvature is of course monstrously exaggerated, in fig. 55, where b is the position of the balloon, $a\,c\,a'$

Fig. 55.—The illusion explained.

the earth's surface, c immediately below the balloon, $q\,d\,q'$ a layer of clouds parallel to the earth's surface. The lines $q\,p\,b$ and $b\,p'\,q'$ are tangent lines to the earth's surface, which they meet in the points p and p', while they meet the layer of clouds in q and q'.

(181.) There is a way of recognising, and indeed of measuring the depression of the sea-horizon (the only horizon to be trusted in such observations), which can easily be put in practice. If the sea-horizon is really depressed below the real horizon, then

it is obvious that the sea-horizon imaged in a vertical mirror ought to appear below the centres of the imaged eye-pupils of the observer. For in the mirror the horizon is shown with its depression truly reflected, while the eye-pupils must be in the same horizontal plane as their images reflected in a truly vertical mirror. When the experiment is tried, there usually seems to be no such depression of the imaged sea-horizon. But it is easy to understand why this is, and to devise a way by which a vertical mirror may be made to demonstrate and even roughly measure the rotundity of the earth.

(182.) Let us first see what is the actual depression of the sea-horizon, viewed from some sufficient but easily attained height, such as 200 feet :—

Let a (fig. 56) be the place of the observer's eye, A a being 200 ft. ; A B the sea surface ; a B the direction of the line of sight from a to the curved sea surface ; B b' b parallel to A a, through B, and $a\,b$ the direction of the real horizon at A. Draw A b' parallel to $a\,b$ to meet B b in b'. Then obviously B b' = A a = $b\,b'$ (since B b is parallel to A a). Therefore B b = 2 A a = 400 ft. Hence the dip of the horizon, or the angle B a b is that subtended by 400 ft. at the distance a B or A m B (appreciably the

FIG. 56. Illustrating the Earth's curvature.

same). Now (Euclid, Bk. III.) the square on a B is equal to the rectangle under A a and the earth's diameter. Hence with feet for our unit of length

$$a\,\mathrm{B} = \sqrt{7920 \times 1760 \times 3 \times 200}$$
$$= 91450 \text{ ft. approximately } (= 17\tfrac{1}{3} \text{ miles}).$$

(183.) Now a length of 400 ft. at a distance of 91,450 ft. subtends the same angle as 1 ft. at a distance of about 228 ft., or about one-fourth of a degree. This is very different, as anyone can see by looking at a circular protractor and noticing how small are the half-degrees usually marked in, from the enormous depression usually indicated in pictures supposed to illustrate the globular shape of the earth. The real angular dip of the horizon is little more than three-fourths even of this, atmospheric refraction diminishing the true or geometrical dip by nearly one-fourth. The real angle of dip is that subtended by 1 inch at a distance of about 320 inches or less than 27 feet.

(184.) In our experiment, supposing the eye one foot from the mirror or two feet virtually from the imaged eye, the imaged horizon (if the station were 200 feet above the sea-level, would be $\frac{1}{150}$th of a foot or about 4-hundredths of an inch above the imaged eye-level. This would not be discernible by ordinary eyesight, the line of the imaged sea-horizon being intercepted by the imaged head. The nearer the head was brought to the mirror the less the imaged depression of the horizon would be. But this difficulty can be removed, and a pretty illustration of the earth's rotundity obtained by the mirror method.

(185.) Let A B C D (fig. 57) be a rectangular mirror, broad enough to include the imaged face (the breadth of which is always exactly half the breadth of the real face), and to show an inch or two clear on either side, as shown. Let a line $a\,b$ be drawn on

the glass exactly parallel to the sides A B, D C. Let the mirror be supported at E and F by rods E G and F H, which can be fixed firmly into the ground. Let the observer so fix them into a turf-sward some 200 feet (say) above the sea-level, on a spot commanding a fine sea-horizon, putting the face of the mirror seawards and being careful to leave ample room for safe walking in front of the mirror. Suppose that at K there is a fine thread K *k* bearing a plumb-bob P, by means of which the face of the mirror may be made perfectly vertical by suitably turning it on the pivots E, F. But as the glass of the mirror may not be of perfectly equal thickness throughout, let the plumb-line admit of being fastened at L, the side D C being made uppermost either by changing the supports or by shifting the mirror in the pivot-holes E and F.

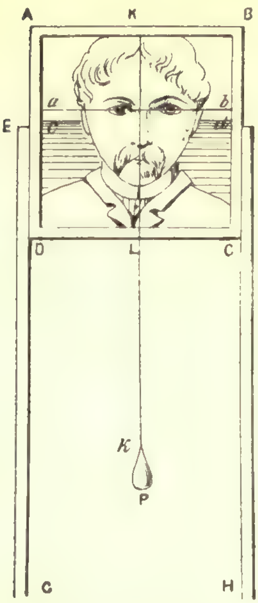


Fig. 57.—Experiment for measuring the Earth's curvature.

After setting the mirror exactly vertical, K uppermost, let the observer retire from it to such a distance—say two yards—that he can see with perfect distinctness not only the pupils of his eyes in the glass, but the diamond-cut line. Let him bring the pupils of his eyes centrally on the line *a b*. He will then see that the water surface *c d* is about a quarter of an inch below *a b*. On the other hand if, by slightly raising his head, he brings the water surface to exact coincidence with the diamond line, he will see that the pupils of his eyes are about a quarter of an inch above that line. Inverting the mirror and letting the plumb-line hang from L, he will get the same result precisely if the mirror is a good one, and very nearly the same result if the mirror is a bad one. In the latter case the mean of his two results will correspond to the single result obtained when the mirror is a good one.

(186.) By using a brightly polished plane of steel, which need not be more than four or five inches square, a better result still will be obtained.

(187.) But a polished plane of steel four or five inches long and only an inch broad or even less, may be very effectively used without a plumb-line, as follows :—

Let A B C D (fig. 58) be the polished steel plane, set on the edge of a saucer E F, containing a little mercury (or ink). Suppose the cup set on a little platform, admitting of levelling adjustment by a slow screw movement; and having set A B across the cup, and levelled the cup's rim in the direction A B (nothing very exact is needed), slowly shift the rim in a direction square to A B by means of the screw movement, until, looking down the face A C, its image on the mercury is reduced to a straight line. Then the face A B is perfectly vertical. Now, as before, look into the steel mirror A C, bringing the centres of the pupils to the edge A B, as at *a* and *b*. The sea-horizon will then be seen as at *c d*, about a quarter of an inch below A B, if the eyes be about two yards from the mirror.

Fig. 58.—A similar experiment.

(188.) If the edges A D and B C are finely divided, and the observer's eyes are set at a measured distance from the mirror, the rotundity of the earth (and therefore the

earth's size) can be measured by this observation, at a single station and with exceedingly simple apparatus.

For different heights, different depressions of the sea-horizon will be noted in this experiment. For places near the sea-level there is no observable displacement of $e\,f$ below $a\,b$; for places much higher than the 200 feet of our experiment the displacement is much greater.[1]

(189.) In this experiment the sea-horizon, as seen in the mirror, shows no curvature; nor can any curvature be seen when the sea-horizon is viewed directly. This sometimes perplexes the observant student. It is, however, easy to explain it.

Let us take a case where it really seems that some curvature ought to be recognised.

(190.) Suppose an observer whose eye is 200 feet above the sea-level looks at the long horizontal roof-ridge of a house, beyond which lies a sea-horizon, and that he brings the middle of the ridge just below the sea-horizon exactly in front of him—ought he not as his eye ranges to right and left along the ridge to lose the sea-horizon through its curving down below the ridge? Or, if he brought the ends of the ridge exactly level with the sea-horizon, ought not the sea-line to stand visibly above the middle of the ridge?

(191.) Theoretically it ought and it does; practically the question is one of degree, and our inquiry must be, how *much* does it curve?

Suppose the ridge to be 50 feet long, and its middle point 25 feet from the eye, so that in sweeping along the ridge the eye ranges over a right angle. Suppose also a fixed point set near the eye to guide it, for otherwise the observation would be altogether inexact. Every line of sight must be taken athwart this fixed point, 25 feet from the centre of the perfectly horizontal ridge-line, to different parts of this line; and what we want to find is how much the lines of sight to either end of the

[1] The law connecting h, the height of the observer, with the angular depression δ of the sea horizon may be thus obtained:—

Call the radius of the earth r. The angle δ is the angle $b\,a\,\mathrm{B}$ of fig. 56, or (appreciably it is the angle subtended by $\mathrm{B}\,b$, or twice h, at a distance $a\,\mathrm{B}$). Now

$$(a\,\mathrm{B})^2 = 2\,r\,h \text{ (appreciably)}$$

$$\therefore\ \delta = \frac{\mathrm{B}\,b}{a\,\mathrm{B}} = \frac{2\,h}{\sqrt{2\,r\,h}} = \sqrt{\frac{2\,h}{r}},$$

where a right angle $= \frac{\pi}{2} = 1{\cdot}5708$.

Thus the angle of depression varies as the square root of the height of the observer's eye. In the above, δ is the geometrical depression-angle. The apparent depression that we are dealing with (which may, indeed, be called the real depression, since it is what is *observed*) is about four-fifths the geometrical depression.

As an example, consider what height is required to give an observed depression of one degree, which would be very slight—far too slight to be noticed without the aid of instruments. The above equation gives us—since δ, the geometrical depression in this case, is five-fourths of a degree—

$$\tfrac{5}{4} \text{ circ. measure of 1 deg.} = \sqrt{\frac{2\,h}{3960}},$$ taking a mile as our unit of length. Now

$$\text{Circ. measure of 1 deg.} = \frac{1}{90}\,(1{\cdot}5708) = {\cdot}01745.$$

$$\therefore \text{ squaring, } \quad \frac{25}{16}\,({\cdot}01745)^2 = \frac{2\,h}{3960}$$

$$\text{say roughly) } 25\,({\cdot}0003045) = \frac{4\,h}{495}$$

$$\begin{aligned} \text{or } h &= 3094 \times {\cdot}0003045 \text{ nearly} \\ &= 0{\cdot}9421 \text{ of a mile} \\ &= 4974 \text{ feet} \end{aligned}$$

One may say that by ascending to the height of 5,000 feet a depression of one degree (apparent) may be observed, if the sea-horizon is visible. To attain a depression of two degrees a height of about four miles must be attained. This angle would seem very small, scarcely to be recognised by the unaided eye, even if the sea-horizon were visible. An ordinary landscape-horizon would not seem lowered at all recognisably. Thus we can readily understand the optical illusion by which the region visible below a balloon at a great height seems shaped like a vast basin.

ridge-line pass above a line of sight to the sea-horizon there, when a line of sight to the middle of the ridge-line just touches the sea-horizon.

Let a, fig 59, be the fixed point athwart which the lines of sight are taken, E B F the roof-ridge 50 feet long, B its middle point; a B square to E F; and a B = E B = B F = 25 feet. Let a vertical plane through E B F cut the true horizontal plane through a in $e\,b\,f$, and let E′ B F′ represent the sea-horizon as supposed to be seen on this plane; e E E′, b B, and f F F′ being vertical lines. We want to find the length of E E′ and F F′.

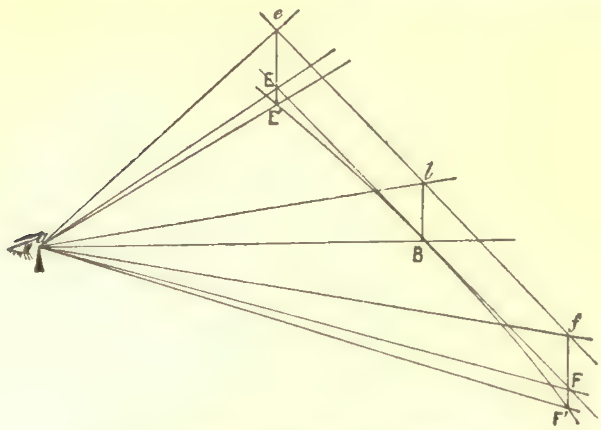


Fig. 59.—Measuring the curve of the Sea-horizon.

Now obviously B $a\,b$ in fig. 59 represents the same angle as B $a\,b$ in fig. 56, as dealt with in the preceding note. For, produced far enough—really to some $17\frac{1}{3}$ miles—a B would meet the sea surface and be the a B of fig. 56; $a\,b$ of fig. 59 would then be the $a\,b$ of fig. 56; and b B would therefore be 400 feet. The proportions of the triangle B $a\,b$ would be precisely the same in both cases (because we are dealing with a point a in each case 200 feet above the sea-level). In each case B b is 1-228th part of $a\,b$. But $a\,b$ in fig. 59 represents 25 feet. Therefore B b represents 25 feet ÷ 228, or about $1\frac{1}{3}$ inch.

But it is clear that the triangles $e\,a$ E′ and $f\,a$ F′ are also similarly proportioned to B $a\,b$ of fig. 56. For $a\,e$, like $a\,b$, is truly horizontal, and a E′ like a B is directed to the sea-horizon. It matters not in what direction we look seawards from a fixed point above the sea; the sea-horizon has always from such a point the same depression. The only difference is that $a\,e$ and $a\,f$ are longer than $a\,b$, while e E′ and f F′ are longer than b B in the same proportion.

Whereas then B b is about $1\frac{1}{3}$ inch in length, e E′ and f F′ exceed $1\frac{1}{3}$ inch in the same degree that $a\,e$ or $a\,f$ exceeds $a\,b$; which is (appreciably) the same degree in which a E or a F exceeds a B, that is as the diagonal exceeds the side of a square (for a B and B E are equal and at right angles to each other). Thus since the diagonal of a square is about 1·414 when the side is 1, we have

$$e\,E' = f\,F' = 1\tfrac{1}{3} \text{ inch} \times 1{\cdot}414$$

$$\text{and } e\,E = f\,F = 1\tfrac{1}{3} \text{ inch (each being equal to B } b\text{);}$$

$$\therefore\ E\,E' = F\,F' = 1\tfrac{1}{3} \text{ inch} \times 0{\cdot}414 = {\cdot}55 \text{ inch approximately}$$

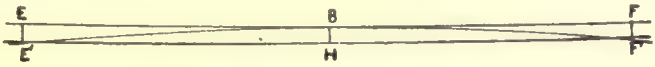


Fig. 60.—Curve of the Sea-horizon.

(192.) In reality, taking refraction into account, the angles of depression are all reduced by about one-fifth, leaving E E′ and F F′ optically equal to only ·44 inch, or $\frac{4}{9}$ths of an inch. The actual curvature of E′ B F′ would be fairly shown if in such a diagram as fig. 60, E B and B F were each 25 feet long, E E′, B H, and F F′ each

$\frac{4}{9}$ths of an inch long; and if then the curve E′ B F′ were swept out in the narrow rectangle E F′, whose length *would be more than thirteen hundred times its breadth.*

(193.) So that even from so great a height as 200 feet, a ridge-roof so long as 50 feet, seen from a distance of 25 feet, commanding therefore a range of a full right angle along the roof, and brought at the two extremities to exact apparent coincidence with the sea-horizon, would be less than half an inch below the sea-horizon at its middle point, even if use were made of such a point as *a* (fig. 59) to guide the eye.

(194.) Observations such as these suffice to show that around any place where they are made, the earth's surface is curved, and equally curved towards all directions, north, south, east, and west. Comparing also the curvature found at one place with that found at any other, the observer, by such methods, comparatively rough though they are, sees no reason to suppose that the curvature of the earth varies in amount from place to place. In the Old and New World, in the northern and southern hemispheres, anyone who cares to repeat such observations as are described above will obtain practically the same result—an apparent depression of about six and a half inches below the horizon-plane at a distance of one mile measured in any direction from the observer's station, the depression increasing outwards as the square of the distance from that station increases. And although the range of the earth's surface which can be dealt with in this way from a single station is small, yet when observations of the kind are repeated in many parts of the earth, they in reality demonstrate the earth's rotundity in a very effective way.

(195.) The actual portion of the earth's surface which can be dealt with under suitable conditions by this method is, however, much smaller than many imagine. For instance, even from a height of six miles, the greatest ever attained by man, the amount of sea surface (more favourably seen, of course, than the irregular surface of land) within visual range would not be more than $\frac{1}{1320}$ of the entire surface of the earth.[1] The position of such an observer may be represented by the point C in fig. 61, where the tangent lines C A T, C B T′ meet at a height of six miles above the surface of the earth D C E—a height so small that on the scale of fig. 61 it can only be recognised by a very slight thickening of the circular outline. The inclination of the lines C T and C T′ to the tangent-line at the earth's surface below C is also small.

[1] For such heights as can be reached above the earth's surface very small compared with the earth's diameter—the range of surface commanded geometrically bears to the whole surface the same ratio which the observer's height bears to the earth's diameter. Thus, at a height of 1 mile, the surface included within tangent-lines to the sphere would be $\frac{1}{7920}$ of the earth's surface (which is about 196,800,000 miles), or about 25,000 square miles, the area increasing with increased height, in direct proportion to the height attained. Taking refraction into account, we must increase the estimate, precisely as though the earth's sphere were increased in diameter, in the same degree that the depression below the horizontal or tangent-line is diminished (for any given distance) by refraction. For instance, if the depression at a distance of 1 mile is only $6\frac{1}{2}$ inches instead of 8, which but for refraction it would be, we must increase our estimate of the visible area as though the earth's sphere had a diameter of 7920 miles increased as 8 to $6\frac{1}{2}$ or as 16 to 13. Hence the visible area is increased as the square of 16 to the square of 13, or as 256 to 169, roughly as 26 to 17. And other cases, where the refraction is greater or less, are similarly dealt with: we increase the geometrically commanded area as the square of the geometrical depression per mile (8 inches) exceeds the square of the optical depression.

How nearly this method of determining the geometrical range of view approximates in all real ascents above the earth to the true value will be seen if we consider that it only differs from exactness even for a 6-mile ascent, in the degree in which the distance of C, fig. 61, above the arc A B exceeds the distance of the bisection of the arc A B from the bisection of the chord A B, which is the same as the degree in which *c* C exceeds *c* A.

Twice this small angle is represented by the angle A c B between lines drawn from A and B to c, the centre of the circle C D E. The small circle $a\ b\ e$ round c as centre represents the area commanded from such a position as C, supposed to be brought centrally under the eye. This circle is drawn double, the inner circle showing the geometrical range from such a point as C, while the outer shows the somewhat larger range which the effects of refraction enable the observer to command optically. It will be seen that the surface commanded is very small compared with the earth's whole surface, which is equal to four times the circle C D E. Yet though relatively small, the area $a\ b\ e$ is in reality large, being in fact no less than 150,000 square miles, or 30,000 square miles more than the whole extent of Great Britain and Ireland,—this, too, without taking into account the effect of refraction, which considerably enlarges the area within the range of vision, making it indeed almost half as large again.[1]

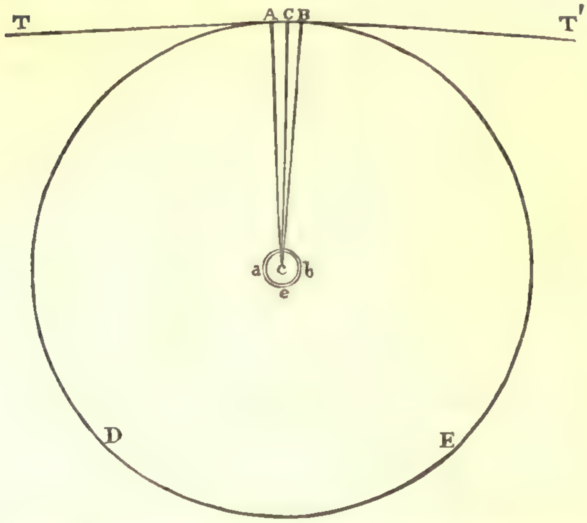


Fig. 61. Illustrating the small area of the Earth which can be seen at one view, even from the loftiest station.

(196.) When science enters on the inquiry into the earth's shape, extended surveys must be made and more trustworthy methods than those hitherto considered must be employed. Such journeys in a north-and-south direction as we considered at the beginning of this chapter, carried out from different parts of the earth, show always the same general result—the north pole of the heavens rising as the observer travels north and sinking as he travels south; while after the north pole has been brought to the horizon, the south pole rises as the observer continues his journey south, and sinks again as he returns northwards. These apparent movements of the pole take place so nearly in exact proportion to the distance traversed north or south as to suggest at first the impression that the earth must be a perfect sphere. Journeys east and west could not be so readily interpreted by the astronomers of old times. A journey either towards the east or towards the west (guided throughout, let us suppose, by careful observations of the pole-star) leaves the aspect of the stellar heavens entirely unchanged. This, indeed, of itself sufficed to convince ancient astronomers that the region traversed must be curved convexly east-and-west as well as north-and-south, since a journey in a straight line would necessarily alter the position of the observer within the heavenly sphere, supposed in old times to lie at a measurable distance. But an ancient astronomer had no means of recognising what the astronomer, the

[1] It is unnecessary to explain why the enlargement due to refraction has the value indicated. The recognition of $(\delta - x)$ instead of δ inches as the depression for a mile, with correspondingly reduced depressions for greater distances, corresponds precisely with what we should find if we lived on a globe whose diameter was 7,920 miles $\times \delta \div (\delta - x)$, the atmosphere on this enlarged globe having no refractive power.

It is an interesting consequence that, seen from such short distances, the earth is *magnified* by refraction in the degree just indicated.

geographer, and the voyager of to-day recognise so readily—the change in the absolute time of the occurrence of such events as the rising, southing, and setting of particular orbs. The appliances he used for measuring time were inexact; and, such as they were, they could not be conveniently carried from place to place while still indicating time. A clepsydra, or instrument for measuring time by the running of water from a cistern, would not be a handy instrument, even when not at work, for a traveller to take with him on a long journey; but it would be simply impossible to measure time with such a clumsy chronometer during actual travel. Thus the ancients were practically unable to determine the dimensions of the *small circles* of the earth crossing the meridians at right angles—the *latitude-parallels* of modern maps. They could, however, calculate them from their known position with respect to the poles and equator.

(197.) Fig. 62 represents the earth as thus known to the astronomers of days preceding the invention of the chronometer and the telescope—instruments equally essential to the determination of the earth's proportions with the precision which modern astronomy requires. P and P′ are the north and south poles; P O P′ is the polar axis, either of the earth's rotation as some of the ancient astronomers already suspected or of the celestial sphere as the greater number supposed. The *great circle* E E′ midway between the poles is the *terrestrial equator*; P E P′, P F P′, P G P′, P O P′, P G′ P′, P F P′ and P E′ P′ the visible halves of *great circles* through the poles P, P′ are the *meridians* or *longitude-circles*. (Those here shown are two hours of longitude apart, since they are separated by intervals along the equator each equal to one-twelfth of its circuit.) The small circles (seen foreshortened as straight lines) B B′, C C′, *c c*′, and *b b*′, are *latitude-parallels*, so called because their planes are parallel to the plane of the equator.[1] A A′ and *a a*′ are latitude-parallels, called the *Arctic* and *Antarctic circles* respectively (or simply the *Arctic circles*), and bound those parts of the earth called the *Frigid Zones* A P A′ and *a* P′ *a*′, within which at midwinter the sun does not rise, while at midsummer he does not set. T T′ and *t t*′ are latitude-parallels called the *Tropics of Cancer* and *Capricorn* respectively, or simply the *tropics*, and bound those parts of the earth called the *tropical zone*, within which at some part of the year the

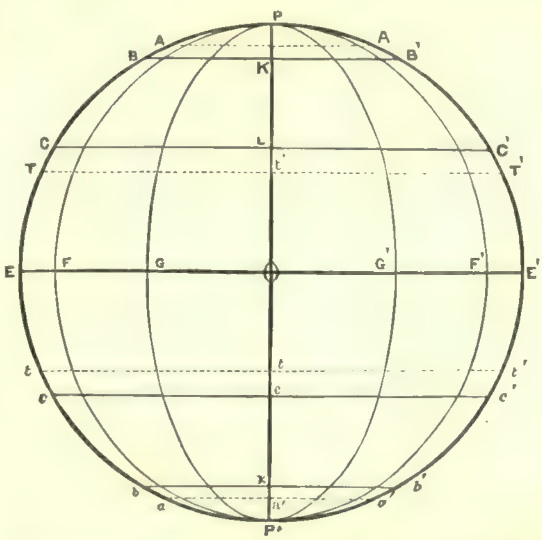


Fig. 62.—The Earth's Meridians, Latitude-parallels, and Tropics and Arctic Circles.

[1] The terms *longitude* and *latitude*—both as applied to the earth's globe and to the celestial sphere—were derived from considerations arising in making maps. They mean simply length and breadth. In mapping a sphere, whether celestial or terrestrial, we have usually the length of the map from top to bottom, representing distances from the pole (in the case of ancient celestial maps the pole of the ecliptic) and the breadth representing thwart distances—in other words, the length and breadth of ancient maps, both celestial and terrestrial, as of modern terrestrial maps, are measured along longitude-circles and latitude-parallels, respectively.

mid-day sun is vertical. The zones A T′ and *a t′*, which lie between the frigid zones and the tropics, are called the *temperate zones*.

(198.) To prove that the earth is an exact sphere, it would be necessary to prove that the relations discovered by the earlier astronomers are presented constantly on every part of the earth's surface. Modern astronomy might have put this matter to the test by direct observation. The result would have been to show that the pole of the heavens does not rise or sink through the same arc on the star-sphere for equal journeys towards or from the pole. As a matter of fact, theory had already suggested that this would be the case, before observations were made by which it could be proved to be so. But we may conveniently consider here the direct evidence.

(199.) The variation, though small, is measurable. It is found that the distance to be travelled to produce a given change in the elevation of the celestial pole becomes greater as we approach the earth's pole, and is least near the equator. It follows, of course, that the curvature of a meridian is variable, being greatest at the equator and least at the poles. A meridian, therefore, is not a perfect circle. Its figure is that of an ellipse whose major axis is the line joining the points where the meridian crosses the equator.

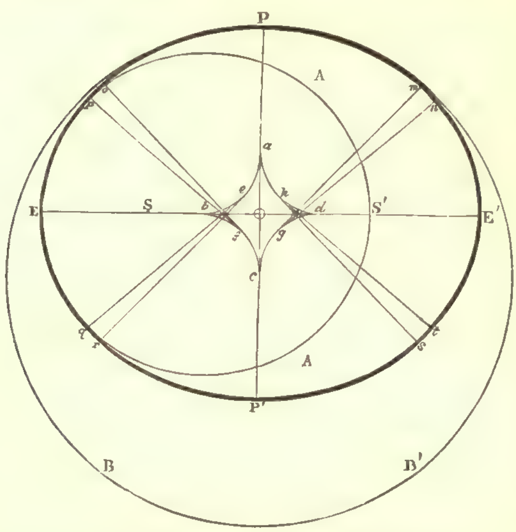


Fig. 63.—Effects (exaggerated) of the Ellipticity of a Meridian.

(196). In fig. 63, P E P′ E′ is supposed to represent a globe compressed as the earth's globe is, but in much greater degree, so that the figure illustrates, in greatly exaggerated manner, the effects of the ellipticity of a terrestrial meridian. The small circle A E A′ shows the greater curvature of the meridian where it crosses the equator E E′ at E ; while the large circle B P B′ shows the small curvature of the meridian at the pole P. The centres of these circles are at *b* and *c* respectively. The curvature gradually diminishes from E and E′ towards P and P′ along the ellipse E P E P′.[1]

(200.) It does not seem desirable to enter here at any length into the discussion of the processes by which the dimensions and shape of the earth have been determined by modern astronomers—meaning by modern astronomers those who have taken part in

[1] For instance at *o*, the line *o f* touching the arc *b f c* at *f*, is the radius of the circle which determines the curvature at *o* ; and exceeds *b* E in length by the length of the arc *b f*.

It is perhaps hardly necessary to remark that the curvature of an arc increases as the radius of the circle of curvature diminishes.

The four-pointed curve *a b c d*, in figs. 63 and 65, is called the evolute of the ellipse, because the ellipse may be evolved by the end of a cord of constant length unrolling over the arcs *c f b*, *c g d*, &c., in the way indicated in fig. 63, where *o f c*, *p f c*, *g e a*, *r e a*, &c., indicate different positions of the unrolling string. The evolute of the ellipse approaches in shape the quadricuspid hypocycloid (traced by a point on the circumference of a circle rolling inside another of four times greater radius) as the eccentricity of the ellipse is diminished; but *a c* is always greater than *b d*. The evolute of the ellipse is an orthogonal projection of the four-pointed hypocycloid. At pp. 72, 73 of my *Geometry of Cycloids*, the properties of the last-mentioned curve are discussed and it is pictured at p. 67 of that work.

this work during the last two centuries and a half. A really complete account would fill a volume much larger than the present, and even then would be but an abstract of matter collected by others. A sketch extending to twenty or thirty pages would be in no sense original matter, and would occupy space which could be ill spared. Nevertheless so much must be explained as may be necessary to give the reader a clear idea of the care and labour which astronomers have bestowed on the measurement of the one planet they can examine close at hand. This is the more necessary that, small though the dimensions of the earth are, compared with the vast distances dealt with in astronomy, terrestrial measurements supply in reality the base lines on which all other astronomical measurements, even the vastest, depend.

(201.) All measurements of the earth must, in turn, be based on measurements of meridional arcs. From the time of Eratosthenes (and how much earlier we know not) until now, this has been the case. The astronomer must first determine the length of the arc separating two stations on the same meridian, and then he must ascertain the difference between the latitudes of the two stations, or, which is the same thing, the difference in the elevation of the visible pole of the heavens as observed from these stations (after due correction for atmospheric refraction, aberration of light, &c.) Yet the measurement of the distance between two stations depends on the accurate measurement of a base-line and that on the accurate measuring of short distances, and this finally on work requiring the use of the microscope to ensure due accuracy. So that even the most tremendous depths fathomed by the telescope may be said to have been determined with the aid of the microscope.

(202.) The measurement of a determinate base-line, which seems a simple task, is in reality exceedingly difficult—at least when such accuracy is to be sought as modern science requires. Owing to differences of temperature at different parts of the region measured and at different times, the measuring instruments undergo changes; they change even while processes of measurement are actually in progress; and devices must be adopted to cause such changes to be self-correcting. The rods used for measurement must not come into actual contact, or disturbances seriously affecting the accuracy of the work would inevitably arise; but since they are separated by a space which, though it may be minute, is not absolutely evanescent, this space must in every case be measured. For this work the microscope is brought into play. Obviously the measuring-rods must be of moderate length, or strains and pressures interfering with the accuracy of the result would inevitably arise: but since the addition of each rod-length to the measured distance introduces the occasion for microscopic measurement of the distance separating rod from rod, the importance of extreme accuracy in the work will be recognised. Suppose, for example, the base-line to be measured has a length of five miles, and that the measuring-rods, or the portions of them used, measure but one yard, then 8,801 microscopic tests have to be applied, including one at each end. Supposing an average error amounting to the 100th part of an inch at each comparison, there would be a possible total error of 88 inches, approximately, in the final result. This would correspond to an error of about 3,760 yards, or more than two miles, in the determination of the earth's mean diameter—an error which the astronomers of old times would have regarded as trifling, but which would be deemed very serious in the astronomical work of modern times. How accurately the work of measurement is managed in our time will be seen when it is mentioned that the greatest possible error in a base-line of between seven and eight miles, measured near London-

derry, has been calculated at not more than two inches; the probable error may be about an inch, corresponding to a probable error of only about thirty yards in the estimated length of the earth's mean diameter.

(203.) Having measured convenient base-lines in different latitudes, the surveyor proceeds to connect them by triangulation. As the length of a line at one extremity of a triangulated space ranging many miles in latitude, can theoretically be calculated from the known length of the base-line at the other end and the data obtained in triangulation, the surveyor by comparing the length thus calculated with the measured length of this end-line, has a test of the accuracy of his triangulation. The measurements which have been made in this way, during recent times, in Sweden, Great Britain and Ireland, France, Russia, India, Africa, and America, have been of the most trustworthy nature.

(204.) But measuring arcs of meridians can give no determination of the earth's dimensions until we have ascertained precisely how much the position of the pole of the heavens differs as observed at the two extremities of the arc thus carefully measured. In other words we must determine with great accuracy the latitudes of two stations, separated by a measured distance north and south, or the accuracy of the measurement of this distance will be useless.

(205.) Now the latitude of a fixed observatory can of course be accurately determined by means of the mural circle or the transit circle described in the last chapter. But instruments of this kind are not available for determining the latitude of the stations used in measuring meridional arcs on the earth's surface. The transit instrument cannot be trusted to give altitudes of heavenly bodies with the accuracy required in such work. The equatorial, again, cannot be employed with advantage, because the adjustment of this instrument is a matter partly depending on the very element of position which is required in the work of measuring the earth: moreover an equatorial such as can be trusted for determining positions with great accuracy is an instrument for the observatory, not for surveyors. We require, to determine the latitudes of the stations occupied in such a survey, an instrument which can be readily moved from place to place, and which, when set up at the stations occupied, can be adjusted with reference to the horizon and the zenith, not with reference to the equator and the visible pole.

(206.) The zenith-sector and the portable prime vertical instrument are among those which may be satisfactorily employed in the work we are considering.

The *Zenith-sector* has been employed, in one form or another, from the time of Picard's survey (which set Newton's theory of gravitation on its feet) until the present day. It is an instrument for observing on the meridian stars of known position which pass near the zenith. The meridional distance of a star from the zenith of a place, if accurately observed, gives of course the polar distance of the zenith itself, if the polar distance of the star is known; and the polar distance of the zenith of a place is the complement of the latitude of that place. It is not necessary to describe the zenith-sector in detail, or to give an illustration of one or other of the forms in which it has been constructed. The principle of the instrument is sufficiently simple. If two straight lines extend from a point near the object-end of a telescope directed to a star on the meridian (and nearly overhead), one being parallel to the optical axis of the telescope and the other vertical (as a plumb-line), the angle between these lines is the apparent zenith distance of the star. For accuracy it is best to make two observations, or rather two sets of observations, of each star; each set being made with the optical

axis of the telescope and the reading arc in the meridian, but the instrument being turned through 180° in azimuth between each pair or each set of observations. The mean of the results thus obtained will be nearer the truth than the result obtained from one observation, or one set of observations, only; while the difference between the results obtained either way, which should be small, affords a satisfactory test of the accuracy of both sets of observations.

(207.) Observations of this class have the important advantage of being practically free from errors due to the variable effects of atmospheric refraction. At the zenith, of course, refraction vanishes, and at 10° from the zenith it amounts but to 10″: the corrections affecting this amount are very small; while the proportion of unavoidable error outstanding after the mean refraction has been corrected for thermometric and barometric variations is but a small fraction even of this small amount. The zenith-sector, however, has not as yet done quite such good work as had been expected from it.

(208.) The *Prime Vertical Instrument* may be described as a transit instrument set to sweep the prime vertical instead of the meridian. Certain peculiarities of detail have to be attended to in the construction of an instrument intended for work on the prime vertical; but the student will sufficiently appreciate the quality of the work if he compares the portable prime vertical instrument with a portable transit instrument with its axis set north-and-south instead of east-and-west, and the *Prime Vertical Circle* with the transit circle, correspondingly set with north-and-south horizontal axis.

(209.) Used to determine latitude, the prime vertical instrument, like the transit instrument, is directed to the measurement of *time*. A star is selected which crosses the prime vertical at a convenient height; and the times of crossing the prime vertical on its eastern and on its western quadrants are noted much as in the case of a transit of the meridian, except that the passage takes place aslant across the parallel wires, being of course aslant to the horizon. The interval between the times of passage is that occupied by a known star at the observer's station in traversing that portion of its horary parallel which lies to the south of the prime vertical. It is obvious that the latitude can be deduced at once from this, by very simple formulæ of spherical trigonometry.[1]

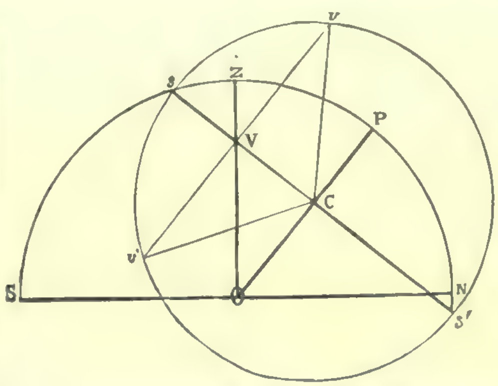


FIG. 64.—Illustrating use of Prime Vertical Instrument for determining latitudes.

[1] It is, easy, indeed, to indicate the formulæ even for those who are not acquainted with spherical trigonometry:—

Thus let S Z N, fig 64, be supposed to represent the visible half of the celestial sphere, S O N the north-and-south line through O the observer's station, Z his zenith, O Z the prime vertical, P the pole, $s\,C\,s'$ at right angles to O P and bisected by O P in C, the foreshortened view of the small circle traversed by a star of known north polar distance $P\,s$ or $P\,s'$, crossing the prime vertical on its eastern and western quadrants at V. Suppose this circle turned round its diameter $s\,s'$ until it is opened out into the circle $s\,v\,s'\,v'$. Then obviously $v\,V\,v'$ at right angles to $s\,V\,s'$ divides this circle into two arcs $v\,s'\,v'$ and $v'\,s\,v$, which are the portions of the star's circuit respectively north and south of the prime vertical. Now $s\,C$ is known, since $s\,C \div O\,P = \sin s\,P = \cos \delta$, where δ is the star's north declination; and the angle $v\,C\,s$ is known, being half the horary angle indicated by the star's observed time of passage from

(210.) The latitudes of stations at a measured distance from each other in a north-and-south direction having thus been determined, the mean length of a degree of longitude for the arc thus surveyed can be calculated, and the exact length of a degree of longitude at the middle of the arc inferred. The following table presents the results of a number of measurements which have been made during the last century and a half.[1]

Country	Latitude of Middle of Arc			Arc measured			Measured Length in Feet	Mean Length of a Degree at the Middle Latitude in Feet
	°	′	″	°	′	″		
Sweden,[2] A B . . .	+66	20	10·0	1	37	19·6	593277	365744
Sweden, A	+66	19	37	0	57	30·4	351832	367086
Russia, A	+58	17	37	3	35	5·2	1309742	365368
Russia, B	+56	3	55·5	8	2	28·9	2937439	365291
Prussia, B	+54	58	26·0	1	30	29·0	551073	365420
Denmark, B	+54	8	13·7	1	31	53·3	559121	365087
Hanover, A B . . .	+52	32	16·6	2	0	57·4	736425	365300
England, A	+52	35	45	3	57	13·1	1442953	364971
England, B	+52	2	19·4	2	50	23·5	1036409	364951
France, A	+46	52	2	8	20	0·3	3040605	364872
France, A B	+44	51	2·5	12	22	12·7	4509832	364572
Rome, A	+42	59	—	2	9	47	787919	364262
America, A	+39	12		1	28	45·0	538100	363786
India, A B	+16	8	21·5	15	57	40·7	5794598	363044
India, A B	+12	32	20·8	1	34	56·4	574318	362956
Peru, A B	− 1	31	0·4	3	7	3·5	1131050	362790
Cape of Good Hope, A . .	−33	18	30	1	13	17·5	445506	364713
Cape of Good Hope, B . .	−35	43	20·0	3	34	34·7	1301993	364060

the prime vertical on the east to the prime vertical on the west, say $v\,C\,s = h$. Hence

$$V\,C = s\,C \cos h = O\,P \cos \delta \cos h,$$

but

$$V\,C = O\,C \tan P\,O\,Z = O\,P \sin \delta \cot \lambda$$

(where λ represents the latitude of the observer, which is the element to be determined). Hence

$$\sin \delta \cot \lambda = \cos \delta \cos h,$$

or

$$\cot \lambda = \cot \delta \cos h;$$

whence, from the observations determining h, λ can be calculated.

[1] In the earlier measurements of the arc of a degree in different places, the observed variation in length appeared to indicate an equatorial instead of a polar compression. James Cassini, who conducted the experiments, was disposed to believe that the apparent increase in the length of a degree as the equator was approached, coincided with what theory required. When his mistake was pointed out to him, he still maintained the accuracy of his measurements. The result was that a new series of measurements, having a wider range of latitude (made, namely, in Lapland and at the equator), and more carefully conducted, was effected, and the increase of a degree towards the poles was established. It is from the part taken by Maupertuis in these measurements that Carlyle derived the epithet *earth-flattener* for that mathematician, though why scientific labours of this sort should be considered fit reasons for contempt Carlyle did not explain. Sydney Smith conceived jestingly the idea that disrespectful speech about the equator would be blameworthy; possibly Carlyle considered that he did well to be angry with one who seemed to him to have maligned the meridians by showing them to be eccentric.

[2] The astronomers by whom these measurements were executed were as follows:—

Sweden, A B Svanberg.
Sweden, A—Maupertuis.
Russia, A—Struve.
Russia, B—Struve, Tenner.
Prussia—Bessel, Bayer.
Denmark—Schumacher.
Hanover—Gauss.
England—Roy, Kater.
France, A—Lacaille, [Maraldi, and] Cassini [de Thury] [commenced 1739].
France, A B—Delambre, Mechain.
Rome—Boscovich.
America—Mason and Dixon.
India, 1st—Lambton.
India, 2nd—Lambton, Everest.
Peru—La Condamine, Bouguer.
Cape of Good Hope, A—Lacaille.
Cape of Good Hope, B—Maclear.

Astr. Nachr. 574.

(211.) These results, though obviously affected by errors of observation so as to be wanting in absolute uniformity, yet clearly show that the length of a degree increases as we pass from the equator towards either pole. We can deduce from them the following dimensions :—

Length of the earth's polar axis . . .	7898 miles
Length of the earth's equatorial diameter .	7924 ,,
Polar compression	$\frac{1}{301}$

(212.) From comparisons of the best modern observations, however, effected independently by Capt. (now Colonel) Clarke, R.E., and by General Schubert, it appeared that the polar flattening, as deduced from variations in the length of a degree of the meridian, is different on different meridians, ranging, according to Clarke, from $\frac{10}{2875}$ in the meridian through longitude 14° $\frac{2}{3}'$ east to $\frac{10}{3083}$ in the meridian through longitude 104° 23′ east (whose plane makes a right angle with that of the former). General Schubert made the range of variation considerably less. Such results, would, of course, tend to show that the earth's equator is not a circle, but an ellipse, the meridian indicating the greatest compression being that corresponding to the longest axis of the equator's elliptic boundary. Capt. Clarke found the longest equatorial diameter, that from long. 14° 23′ east to 165° 37′ west of Greenwich, 2 miles longer than the diameter at right angles to it; the polar diameter of the earth 41,707,796 feet long; the longest and shortest diameters of the equator respectively 41,852,864 feet, and 41,843,896 feet. Later, he slightly corrected these results.

(213.) General Schubert, dealing with the same measurements, save that he excluded the French arc and gave undue weight to Russian as compared with Indian surveys, assigned to the equator an ellipticity of $\frac{1}{8885}$, placing the vertices of the longer axis in longitudes 41° 4′ east and 138° 56′ west of Greenwich.

(214.) The difference between the estimated compression for meridians through different longitudes seems to lie within the probable errors of observation, and cannot be regarded as demonstrably due to a real ellipticity of the equator. It is quite possible, however, that there may exist an irregularity of this sort, and not only so, but that no section of the earth is either a perfect circle or a perfect ellipse even when minor or contour irregularities are neglected. These details scarcely belong, as yet, to exact science.

(215.) The dimensions of the earth adopted in this work are slightly different from those indicated above. They are as follows :—

Earth's equatorial radius .	=	3,963·296 miles		
Earth's equatorial diameter	=	7,926·592 ,,	=	502,228,800 inches
Earth's polar diameter	=	7,899·166 ,,	=	500,491,200 ,,
Polar compression . .	=	$\frac{1}{289}$		

This length of the equatorial diameter is that deduced by Colonel Clarke from the best modern observations; the polar compression, still a relatively doubtful element (quantitatively), is that which the latest observations seem to render most probable.

(216.) In fig. 65 the true relations of a meridian curve are exhibited on a scale of one inch to 1,500 statute miles. The curve *a b c d* of fig. 63 has sunk into the outline of the small black star *a b c d* at the centre of the plate. The foci of the elliptical outline are at S and S′; and it will be seen that the *eccentricity* is a much more appreciable quantity than the *ellipticity*. The Arctic circles A K A′ and a k′ a′ span the arcs A A′ and

a a′ which are greater than the arcs T t and T′ t′ spanned by the tropics T T′ and t t′. If the earth were a true sphere each of the arcs A A′ and a a′ would be equal to each of the arcs T t and T′ t′, each being an arc of about $23\frac{1}{2}°$. But the arc T t is less than the arc A A′, because the verticals at A and T, instead of passing through the centre of the figure *a b c d*, are tangents to the arc *b c*; thus each is thrown slightly *towards* E, by which the arc E T is diminished while the arc P A is increased. And so of the other corresponding arcs.

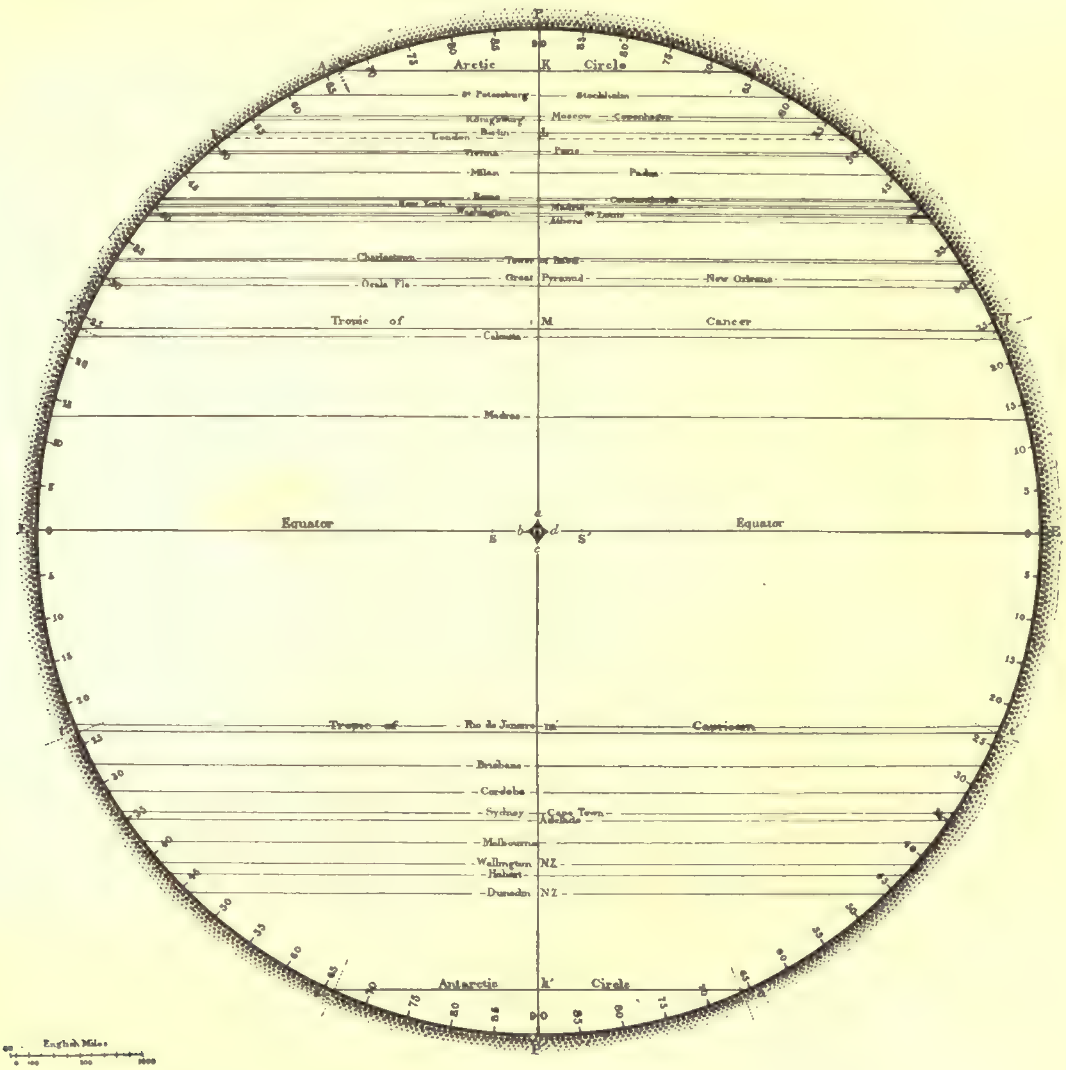


FIG. 65.—The Earth's true figure.

(217.) The latitude-parallels of some of the most important or (for various reasons) interesting positions on the earth are shown in fig. 65, as also the latitudes to every five degrees round the elliptical outline.

(218.) The area of the earth, estimated from the dimensions above indicated, is no less than 196,840,000 square miles. The land surface of the earth has an area of about 51,600,000, while the sea surface occupies about 145,240,000 square miles. The

volume of the earth—which, by the way, is more easily calculated than the area, precisely as the area of an ellipse is more easily calculated than its perimeter—is about 259,868,200,000 cubic miles.

(219.) Fig. 65 indicates the probable extent of the earth's atmosphere, about which, however, we know less than we do about the total mass of the air, which lies between 5,000,000,000,000,000 and 5,400,000,000,000,000 tons.

(220.) We shall presently see that, compared with the dimensions of the earth's orbit around the sun, her own dimensions are small. But the consideration of the vast distances presented to us in the study of astronomy, must not hide from us the fact that the absolute dimensions of the earth are far from being insignificant. Indeed there is one way of viewing these dimensions by which they are made to appear comparable even with the dimensions of the planetary orbits. Thus the surface of a path one mile wide extending from one side of the earth's orbit to the opposite, would be less than the earth's surface; and the earth's volume would suffice to form a solid column having a base more than $44\frac{3}{4}$ square miles in area and a height equal to the diameter of Neptune's orbit.

(221.) This seems the proper place to discuss the methods of mapping which may be employed in representing parts of the globe, or even the whole globe. For, besides that geographical relations, which specially require such maps in illustration, have an important bearing on the study of our earth as a planet, the same problems arise in the construction of maps of the heavens as in the construction of terrestrial maps. Indeed, in constructing star-maps we find the selection of suitable methods of projection even more important than in the case of geographical mapping. For, the earth being a globe (in mapping we may overlook its slight compression) we may fairly represent its features on a globe-surface, the only disadvantage being that a globe is inconvenient in form. But since the stars seem to be spread over the concave surface of the celestial sphere, they cannot be satisfactorily represented upon the convex surface of a globe. On a globe, the star-groups must either be represented as they actually appear in the heavens, or in such a manner that they would appear in their just positions to an eye supposed to view them from the centre of the globe. The apparent distances of the stars from each other can be accurately given in either way; but the first brings the convexity of the globe into direct contrast with the concavity of the heavens; and the second (the method always adopted) reverses the positions of the star-groups as respects east and west.[1] Star-charts are therefore very necessary to the student who desires to become acquainted with the actual configuration of the constellations.

(222.) When any portion of a globe is represented upon a plane surface, such representation will exhibit some or all of the following defects,—distortion of large figures, distortion of small figures, variation of scale, and variation of area. The first and third defects are unavoidable, but are more sensible in some projections than in others. One or other of the second and fourth defects may be made wholly to disappear, but of course, not both at once.

[1] The change is not exactly the same as that which results when the stars are viewed through an astronomical telescope. Thus a group of stars, N, S, E, and W, which appears to the naked eye in the position $\overset{\text{N}}{\underset{\text{S}}{\text{E}\quad\text{W}}}$, would be represented on a globe in the position $\overset{\text{N}}{\underset{\text{S}}{\text{W}\quad\text{E}}}$, and would appear in an astronomical telescope in the position $\overset{\text{S}}{\underset{\text{N}}{\text{W}\quad\text{E}}}$.

(223.) The principal projections[1] of the sphere are the *gnomonic*, the *stereographic*, the *equidistant*, and the *orthographic projections*; and among *constructions* not properly called projections may be mentioned Mercator's, Flamsteed's, the conical method, and isographic or equal-surface methods. I propose to examine the advantages and defects of these—except Mercator's, which has no value for astronomical or celestial charting.

(224.) In fig. 66, let P A S represent half a great-circle of a sphere, of which G is the centre, and P S′ a tangent at P; so that, if the figure were to revolve about S P it would generate a sphere and the tangent-plane at P. Suppose the sphere to be marked with meridians and parallels (like a terrestrial or celestial globe) to every fifth degree. Then, since the spaces formed by these lines on a sphere have definite and easily determinable figures, we can obtain a convenient measure of the value of any method of projection by comparing the projected figures of these spaces with the actual figures on the sphere. For instance, assume the point of projection to be somewhere in the line P Q, and suppose A P to be a quadrant of the equator, divided by the meridians into eighteen parts, then we may compare the actual and the projected figures of the eighteen spaces which lie between the arc A P and a quadrant of the neighbouring parallel; or again we may suppose P to be a pole of the sphere, P A a quadrant of a meridian divided into eighteen parts by the declination-parallels, and we may compare the actual and projected figures of the eighteen spaces which lie between P A and a quadrant of the neighbouring meridian. It is clearly unnecessary to consider the variations of the figures of these spaces in more than one or two directions, *since in all axial projections the variation of scale and the distortion of figure are the same along all radii from the centre of the projection.* Projections are usually supposed to be made on a great circle of the sphere, but as the gnomonic projection cannot be so made I shall consider all projections to be made upon the tangent-plane at the extremity of the axis of projection. Thus in fig. 66, the point of projection will assume successively different positions along P Q, and the plane of projection will be the tangent-plane at P. This

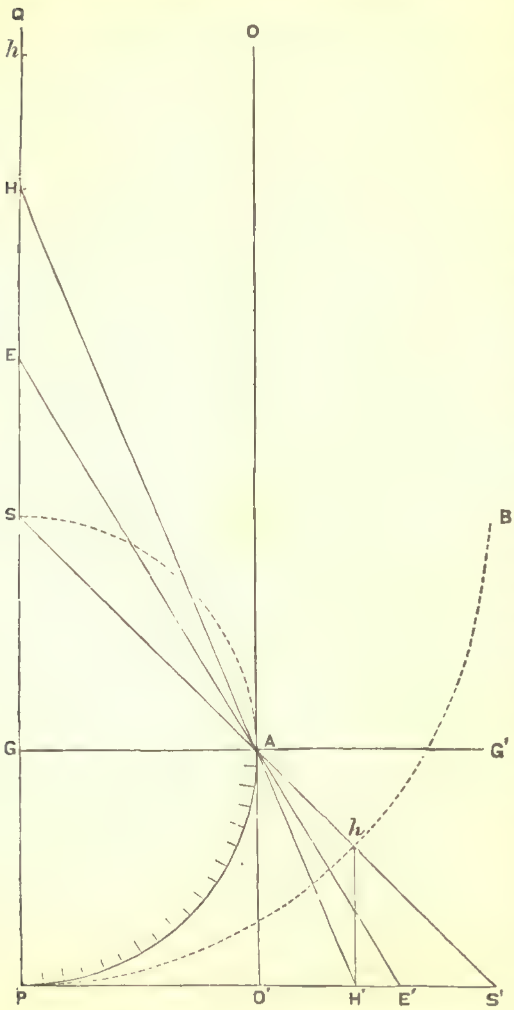
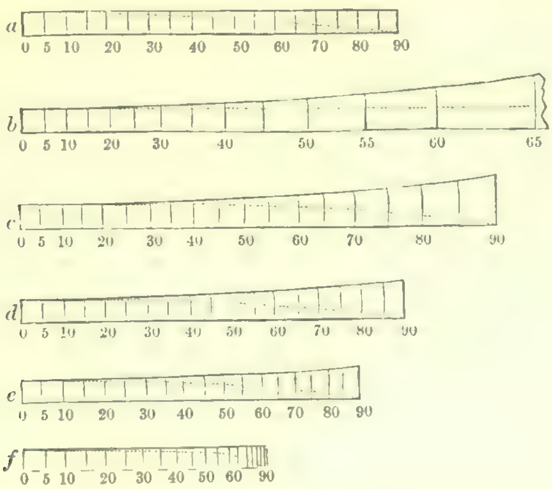


Fig. 66.—Illustrating the principal Projections of the Sphere.

[1] The term projection has come to be applied, in mapping, to any mode of construction founded on some definite geometrical principle. The strict definition of the term is as follows:—If from a fixed point lines be drawn to all the points of a given curve, the curve in which these lines meet a given surface is the projection of the given curve on the given surface for the fixed point.

method, though unusual, has one important advantage,—that the undistorted part of each projection (the part, namely, near the point P—called the *principal point*) is on the same scale in each projection, and that scale the scale of the globe itself.[1]

(225.) Two strips of a sphere corresponding to the two considered in the preceding paragraph, that is, a strip five degrees broad along a quadrant of the equator, and a strip between quadrants of two meridians five degrees apart, are represented on a large scale in fig. 68. The first forms a row of eighteen squares, the second forms a lenticular figure divided into eighteen compartments—whereof the lowest is a square, the uppermost an isosceles triangle, and the intermediate figures are quadrilaterals of



a. A strip of the sphere between quadrants of the equator and of a declination-parallel 5° from the equator, crossed by parts of declination circles 5° apart.

b. Part of the same strip on the *gnomonic* projection, 0 being the principal point.

c., *d.*, *e.*, *f.* The same strip on the *stereographic* / *equidistant* / *isographic* / *orthographic* projection, 0 being the principal point.

NOTE.—The dotted line in fig. *a* (taken with the base-line and the cross-lines) indicates the form of a strip between quadrants of two meridians 5° apart, crossed by parts of declination-parallels 5° apart. The forms of the various projections of the same strip are indicated in a corresponding manner by the dotted lines in figs. *b*, *c*, *d*, *e*, and *f*.

FIG. 67.—Illustrating the advantages and defects of the principal Projections of the Sphere.

varying form. Fig. 67 (*a*) exhibits the first strip reduced to the scale of fig. 66, and the dotted outline gives a sufficient approximation to the second strip.[2] With these strips and the figures into which they are divided by the cross-lines, we can compare the corresponding strips and figures resulting from different modes of projection. A line drawn from the centre of projection (in P Q, fig. 66) through any division of the arc

[1] The student will find a certain definiteness given to his notions of these projections, by considering the sphere as transparent, the meridians and parallels as opaque, the centre of projection as a brilliant luminous point, and the projection itself as the shadow of the meridians and parallels upon the tangent-plane at P.

[2] In reality, of course, even such narrow strips of a globe are not developable, and the chord and arc of five degrees are not equal, as they are assumed to be throughout the investigation in the text. Since, however, the arc of five degrees exceeds the chord by less than $\frac{1}{3000}$th part of either, the error, on all ordinary scales, is altogether inappreciable.

P A gives the distance from P of the cross-line corresponding to that division, and the length of the cross-line is determined by increasing the true length in the proportion of the whole length of the secant-line so drawn, to that part of it which lies between the point of projection and the division-point in P A.

(226.) Proceeding from P towards Q, the first point we meet with suitable for a centre of projection is the point G, the centre of the sphere. A projection having this

N. P. D.	Length of arc of 5° of longitude or R.A., arc at Equator being taken as 1.	Latitude or Dec.	Figs. of developed strips of Globe	Latitude or Dec.	Area included between meridians and Dec.-parallels 5° apart	N. P. D.
°		°		°		°
0	0·000	90		90		180
					0·044	
5	0·087	85		85		175
					0·131	
10	0·174	80		80		170
					0·217	
15	0·259	75		75		165
					0·301	
20	0·342	70		70		160
					0·383	
25	0·423	65		65		155
					0·462	
30	0·500	60		60		150
					0·538	
35	0·574	55		55		145
					0·609	
40	0·643	50		50		140
					0·676	
45	0·707	45		45		135
					0·738	
50	0·766	40		40		130
					0·791	
55	0·819	35		35		125
					0·844	
60	0·866	30		30		120
					0·888	
65	0·906	25		25		115
					0·925	
70	0·940	20		20		110
					0·954	
75	0·966	15		15		105
					0·977	
80	0·985	10		10		100
					0·992	
85	0·996	5		5		95
					1·000	
90	1·000	0		0		90

FIG. 68.—Showing a strip of a Terrestrial or Celestial Globe 5° wide, 90° long.

point as centre is called *gnomonic* from its relation to the art of dialling. Drawing lines from G to the successive division-points along P A, we find that the corresponding divisions along the projection of P A (which projection is, of course, a straight line) fall farther and farther apart, at first gradually, then more and more rapidly. Since G A is parallel to P S′, the projection of A does not fall on the plane of projection, a relation which is expressed by saying that the projection of A falls at an infinite distance from P. Thus a complete hemisphere cannot be represented on the gnomonic

projection. The lengths of the cross-lines, determined as stated in the preceding paragraph, also increase, but not so rapidly as the distances between successive divisions. The first thirteen of the spaces corresponding to the squares of *a*, fig. 67, are exhibited at *b*. The dotted line parallel to the base-line marks the position of the meridian corresponding to the dotted meridian of *a*. It is clear that this projection only gives a satisfactory delineation of those parts of the globe which lie near the principal point. The greatest variation of scale lies in the direction of lines through the principal point, but there is considerable variation of scale in all directions for parts far removed from the principal point.

(227.) The gnomonic projection possesses several interesting geometrical properties. Since lines from the centre of a sphere to the circumference of a great circle lie in the plane of that circle, the projection of a great circle is the intersection of its plane with the tangent plane of projection, and is therefore a straight line. It follows from this important property that the equator, ecliptic, meridians, and longitude-lines are all represented by straight lines in gnomonic star-maps; stars which appear to lie in the same straight line in the heavens will be in the same straight line in a gnomonic map; and further, if we have obtained the projections of any two points of a great circle, we obtain the projection of the circle by simply drawing a straight line through the two points. Lines from the centre of a sphere to the circumference of a small circle lie on a circular cone, so that the projection of a small circle is the intersection of a cone with the tangent-plane of projection, and therefore is one of the conic sections. If the whole of such a circle lies within the hemisphere nearest the plane of projection, it is clear that the projection is a closed curve, which (being a conic section) must be either a circle or an ellipse, according as the plane of the small circle is parallel or inclined to the tangent-plane. If part only of such a circle lie within the hemisphere nearest the tangent-plane, the projection will not be a closed curve. The part of the circle lying within such hemisphere will be projected into a curve extending indefinitely, and though, in such projections as we consider in mapping, the other part of the circle would not appear, yet the strict mathematical projection of this part would give another indefinitely extended curve. Therefore, since the projection is a conic section having two indefinite branches, it must be a hyperbola. In the intermediate case, in which a small circle touches the boundary of the hemisphere nearest the tangent-plane, the projection will be a single curve indefinitely extended, and therefore (being a conic section) will be a parabola. Thus we obtain the following rule—the gnomonic projection of a small circle is an ellipse, a parabola, or an hyperbola, according as the distance of the nearer pole of the circle from the principal point is less than, equal to, or greater than, the complement of the spherical radius of the small circle. The projection reduces to a circle when the pole of the small circle coincides with the principal point.

There are few examples of the gnomonic projection in this work. The maps illustrating the motion of Saturn, here and in my treatise on 'Saturn and its System,' are on this projection; as is also my 'Gnomonic Star-Atlas,' as its name implies.[1]

[1] The gnomonic projection is recommended, and much used, for recording the paths of meteors, because of the property above mentioned that great circles are projected into straight lines in gnomonic charts. I believe, however, that the great distortion in gnomonic maps introduces errors in recording the observed positions of a meteor which are but poorly compensated by the circumstance that a straight line through any two positions of a meteor marks the great circle along which the meteor travelled. If we have wrong positions for marking in our straight line, we shall have a wrong great circle. The stereographic projection is preferable.

(228.) The next point suitable for a centre of projection is S, the extremity of the diameter P G S. A projection having this point as centre is called *stereographic.* Drawing lines from S to successive points along the arc P A, we find that the distances between successive points of division increase from the centre of the projection, but not nearly so rapidly as in the gnomonic projection. Since S A produced meets P S′ at a point S′ such that P S′ = 2 G A, the whole hemisphere is projected into a circle whose radius is twice that of a great circle of the sphere. The cross-lines (which are not straight as in the gnomonic projection) increase in length from the centre at the same rate as the spaces between successive divisions. The cross-line corresponding to the point A is clearly twice the length of the cross-line corresponding to the point P. Thus the eighteen spaces corresponding to the eighteen squares of *a*, fig. 66, are represented, as at *c*, by eighteen figures, not differing greatly from squares, but varying in size, the area of the greatest being four times that of the least.[1]

(229.) The stereographic projection possesses many elegant properties. Amongst these the principal are the following:—All circles, great or small, are projected into circles (excepting, of course, circles which pass through the centre of projection, which are projected into straight lines); intersecting lines on the sphere are projected into lines intersecting at the same angle; and very small figures on the sphere are projected into similar figures.[2] The first property is a useful one; since it follows that, if we can determine the projections of three points of any circle on the sphere, the circle described through those points is the projection of the circle. The other two properties are also very useful. The stereographic is, on the whole, the most valuable simple projection for mapping purposes.

Plates III. and IV. present the Earth on the stereographic projection to a distance of 150° from the north pole in Plate III., and from the south pole in Plate IV.

(230.) The next point suitable for projection is the point E, so taken that S E is equal to half S A.[3] This point is selected with the following object—that lines to the equidistant divisions of P A may meet P S′ in points as nearly equidistant as possible. If E A meet P S′ in E′, then a point from E to the bisection of the arc P A bisects the straight line P E′. For smaller divisions the law of *equidistant* division is not exactly fulfilled, and of course it is impossible to find any point which gives more than an approach to the law.[4] In the construction of maps the law is supposed to be strictly fulfilled, and the projection thus derives its name of the *equidistant* projection. But

[1] The approximation of these figures to the square form depends on the properties examined in the following note.

[2] These properties may be very easily established. Thus suppose *c*, a point in P A (fig. 66), to be the pole of a small circle, and that A *c* P meets this circle in the points *a* and *b*, then lines from S to this circle lie on an oblique circular cone, and the intersection of the tangent-plane at P with this cone is a circle, since the inclination of P S′ to S *b* is equal to the sum of the angles S P S′, P S *b*, that is, the sum of the angles S *a* P, P *a b*, or to the single angle S *a b*, so that the tangent-plane intersects the oblique cone in a subcontrary section. The second and third properties are also easily demonstrated; since they obviously depend on the property that the tangent-plane at any point *c* on the sphere and the tangent-plane at P are inclined to S *c* at the same angle, the complement namely of the angle P S *c*. [To avoid confusion, some of the lines and points mentioned in this and other notes are not given in fig. 66. The student can add them.]

[3] A point distant from S (towards E), one-half the radius of the sphere, has been made use of by Sir H. James, but this projection possesses no properties deserving of particular comment.

[4] The problem of equidistant projection is very similar to that of isographic projection mentioned further on. We obtain, as in that case, not a single point of projection, but different points of projection along P Q for each small circle about P as pole. The projection, thus interpreted, may be applied to the whole sphere.

the ellipses into which circles on the sphere would in general be projected are replaced by circles. It is a portion of this modification of the true projection which is supposed to be represented at *d*, fig. 67; the base-line, being taken equal to P E′ in fig. 66, is divided into eighteen equal parts; the cross-line opposite O is taken equal to one of these parts, and the cross-line opposite G O is the eighteenth part of a quadrant about the point marked O as centre; the arc of a circle cutting the first-named cross-line at right angles at its upper point and passing through the upper point of the second cross-line limits the remaining cross-lines. The cross-lines increase in length successively, but not so rapidly as in the case of the stereographic projection. Hence the successive spaces vary in shape and area, but the area of the greatest is not quite $1\frac{3}{5}$ times as great as that of the least, instead of being four times as great, as in the stereographic projection. Neither the projection from E nor the modification adopted in mapping possesses any geometrical properties worthy of special notice. But the value of the latter for mapping purposes is very great, and especially for areas of moderate extent. The trouble required (not that this is great) to calculate the positions of meridians and parallels on this projection when the centre of the map is not a pole of the sphere, has probably prevented it from coming into general use. But if the globe is symmetrically distributed among twelve maps, as in my 'Library Star-Atlas,' and 'School Star-Atlas,' and in geographical atlases I am preparing, the maps are singularly free from distortion and from variation of scale; they present the whole surface of the Earth in twelve equal circular overlapping maps.

(231.) The sole remaining projection rightly so called, commonly used, is the *orthographic*, in which the centre of projection is supposed to be in P Q, but at an infinite distance from P. A portion of this projection is represented at *f*, fig. 67, the base-line being equal to P O′, fig. 66, determined by drawing O A O′ parallel to Q P. It is clear that portions near the circumference of the projected hemisphere are greatly contracted in the direction of lines drawn to them from the principal point, but not in the direction at right angles to such lines. The projection possesses many elegant and valuable properties. It is freely used throughout this work, and in the chapter on the Earth as a planet forty-eight views of the Earth on this projection are given, together with the constructions necessary for preparing such projections. I have had occasion to draw thousands of them.[1]

(232.) At *e* (fig. 66) is represented a strip of a map, constructed on an equal-surface central construction. In this method of mapping equal areas on the globe are represented by equal areas on the map. M. Babinet, who first proposed such a construction, called it the *homolographic* projection of the globe; the term *isographic* seems preferable, however; and *equal-surface* (being English) seems best of all.

It is stated in Nichol's 'Cyclopædia of the Physical Sciences' that Cauchy, the celebrated mathematician, solved Babinet's problem; though it is not easy to see

[1] In Nichol's *Cyclopædia of the Physical Sciences* it is stated that the orthographic is the projection commonly seen in the 'pair of hemispheres' of atlases. Sir J. Herschel also, in his *Outlines of Astronomy*, speaks of this projection as 'chiefly employed in maps.' It may possibly have been used in some old atlases, but is never employed in modern atlases. Pictures of the moon, sun, and planets, as they appear in the telescope, are orthographic projections of the spherical surfaces of those luminaries. In the earlier editions of my treatise on the moon a map of the moon on a less distorted projection was introduced; but I removed it from later editions, as it was not needed by mathematicians, and misunderstood by others.

what difficulty Babinet could have found, since the problem admits of many simple solutions. I am unable to say whether Cauchy's solution corresponds with any of those I am about to indicate.

(233.) The advantages of isographic projection for special purposes are obvious. Maps thus constructed are not necessarily much distorted; though, of course, when the whole of the sphere is represented in a single projection, the distortion of portions of the sphere is very great.

(234.) The method illustrated in fig. 69 results from the solution of the following problem:—Two neighbouring latitude-parallels being taken, including between them a very narrow belt of surface, required to find a point on the polar axis from which this belt would be projected into a ring of equal area on the north-polar tangent-plane. The solution of this problem gives a formula from which it results that each such belt must be projected from a different point;[1] in other words, that there is no single point for which any finite area of the globe can be isographically projected.[2]

(235.) The construction for this projection is simple. If the meridians and parallels are to be drawn to every tenth degree, proceed as follows:—Describe a circle with a radius equal to twice that of the globe to the scale of which the projection is to be drawn; divide the circumference to every fifth degree; draw a pencil-line from the centre to one of these divisions, and a series of other pencil-lines—which will cross

[1] The formula is $x = 2\ r\left(1 + \cos\frac{\lambda}{2}\right)$, where r is the radius of the sphere, λ the mid-latitude of the belt, and x the distance of the point of projection for the belt from the north pole of the sphere. The plan had already been described by Sir John Herschel in his noble work, *Observations made at the South Cape*. I was not aware of this when I first described this projection in my *Handbook of the Stars* (now out of print).

[2] There is a simple and rather elegant geometrical method of obtaining the construction by a sort of double projection which is worthy of notice. If B h' P, fig. 66, represent a quadrant of a hemisphere of which S is the centre, and S P the radius, then, if we project the sphere S A P from S (that is, quasi-stereographically) on this hemisphere, and project the resulting projection orthographically on the tangent-plane at P, we shall obtain an equigraphic projection of the complete sphere. For the hemisphere of which P A is a quadrant we obtain the radius P H$'$, by drawing S A h', and then h' H$'$ perpendicular to P S$'$; the radius for the complete sphere will clearly be P S$'$ (= S B). The mathematical reader will find no difficulty in proving that this geometrical method corresponds with the formula obtained above, or in establishing the correctness of either method. The following is a sketch of the proof that the geometrical method gives an equal-surface projection:—Let A be *any* point on the sphere P A S (that is, suppose for the moment that A is not a particular point, viz. the extremity, of the quadrant P A), A O the tangent at A; let S A h' represent a cone of minute vertical angle enclosing a minute element of the surface of the sphere S A P at A and of the sphere B h' P at h'; lastly, let h' H$'$ represent a cylindrical surface formed by perpendiculars from every point of the boundary of the element of area at h' to the tangent-plane at P, enclosing therefore an element of area at H$'$; it will be sufficient for our purpose to prove that the element of area at H$'$ is equal to the element of area at A. Now the tangent at h' is parallel to P A, and H$'$ h' is parallel to S P; hence the inclination of the tangent-plane at h' to H$'$ h' is equal to the angle S P A; also (Euc. III. 32) the angle S A O is equal to the angle S P A; hence (from the known relation connecting areas with their orthogonal and conical projections)—

The element at H$'$: the element at h'
:: sin A P S : 1
:: S A : S P,

and

the element at h' : the element at A
:: $(\text{S}\,h')^2$ sin S A O : $(\text{S A})^2$
:: $(\text{S P})^2$ S A : $(\text{S A})^2$ S P
:: S P : S A,

therefore *ex æq.*

the element at H$'$: the element at A
:: S A : S A;

that is, the element at H$'$ is equal to the element at A. It follows that by the double method of projection described every element of the surface of the sphere is projected into an element of equal area; hence the result is an equigraphic projection of the complete sphere.

the first at right angles—connecting divisions equidistant from the first on either side of it; describe circles concentric with the first through the points in which the first straight line is crossed by the others—these are the *parallels*; and, lastly, draw straight lines from the centre to alternate divisions round the outer circle—these are the *meridians*. In other words, if we omit alternate meridians in the *polar orthographic* projection of the meridians and parallels of a *hemisphere* to every *fifth* degree, we have the meridians and parallels of the *polar isographic* projection of a *complete sphere* to every *tenth* degree. We may now darken the middle parallel, which represents the equator, mark in the tropics and arctic circles in their proper places, as shown by the dotted circles, and draw in the continents and islands according to their proper longitudes and latitudes. It is convenient also to darken two meridians at right angles to each other; for this purpose we may select the meridian separating the old and new hemispheres (so called) from each other, and the meridian at right angles to the former. In other words, the darkened meridians and parallels of fig. 69 (as of the other figures) correspond with the circumferences and the horizontal and perpendicular diameters of the maps of the two hemispheres commonly given in our atlases.

FIG. 69. The World on an equal-surface projection (central).

(236.) I have dealt with this construction at some length because I have had to employ it frequently in star-mapping, when dealing with questions relating to stellar distribution, for which this method is manifestly suitable; some such method, indeed, is essential for such work. Many of the maps in this book are on the central equal-surface projection—in particular the chart of 324,000 stars, on which chiefly I base my disproof of the long-accepted theory of the structure of our galaxy.

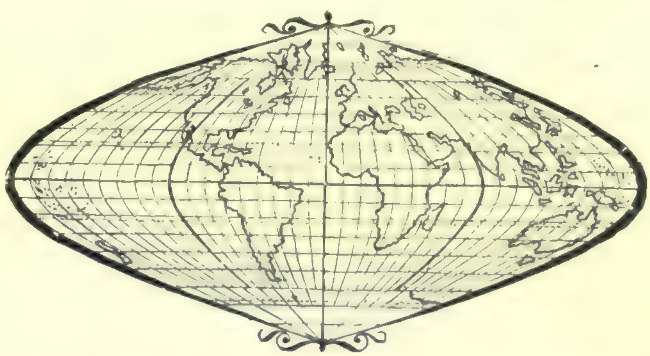


FIG. 70.—The World on an equal-surface projection (Flamsteed's).

(237.) The method illustrated in fig. 70 is an extension of Flamsteed's projection to the whole globe. The construction is simple: a series of equidistant parallels represent the parallels of

latitude, a perpendicular cross-line representing a meridian bisects all the parallels, which are made equal in length to the actual parallels on a globe of the scale of the figure, the distance between them being also equal to the true distance separating

FIG. 71.—The World on an equal-surface projection, showing the Mean Annual Isotherms, or lines of equal mean temperature throughout the year.

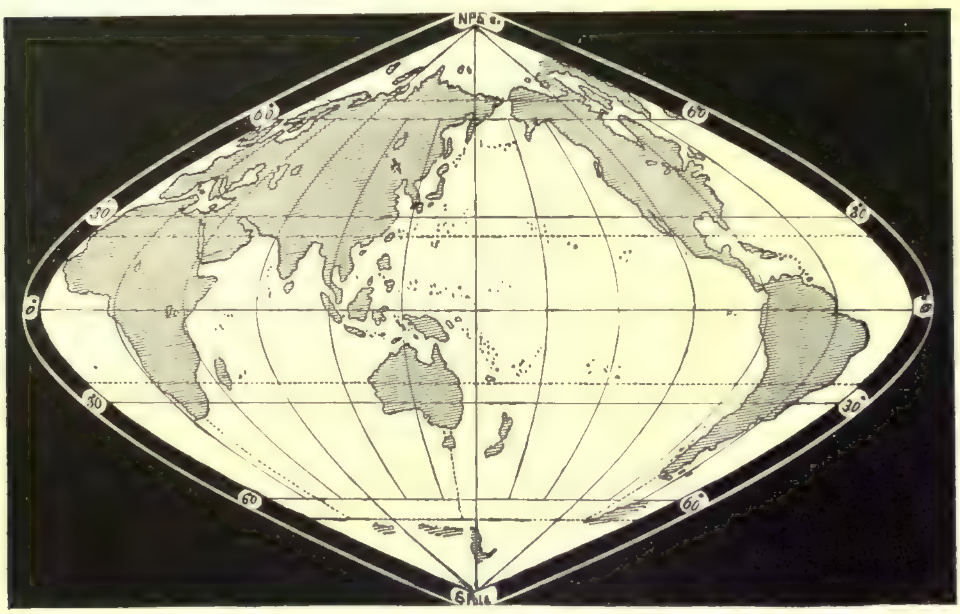


FIG. 72. -The World on an equal-surface projection, showing the Pacific Ocean nearly centrally.

successive parallels on such a globe. The parallels being divided into equal parts, corresponding points of division are connected by curved lines representing the meridians, as shown in fig. 70, in which meridians and parallels are laid down to

every tenth degree. It is obvious that this projection is isographic, for the spaces near the central meridian represent the corresponding spaces on the globe both in size and shape; and all the spaces between any pair of parallels are equal, though they vary in shape; for each may be divided into two unequal triangles, and we see that the greater and less triangles of any one space are equal to the corresponding triangles of another, since they have equal bases respectively, and lie between the same parallels. This method is employed in the large folding map of stars visible to the naked eye, in my 'Other Worlds than Ours,' which appears also in the present volume. I have also used the method and certain modifications of it in my 'Elementary Physical Geography' to illustrate climatic relations, for which, where the whole Earth is represented, it appears to me altogether better suited than Mercator's. For instance, consider its use in illustrating climate, as in fig. 71 (from my 'Elementary Physical Geography'), where it presents correctly two important features which are quite masked in a Mercator's chart, viz. the distances separating the isothermal lines and the areas corresponding to given ranges of mean annual temperature.

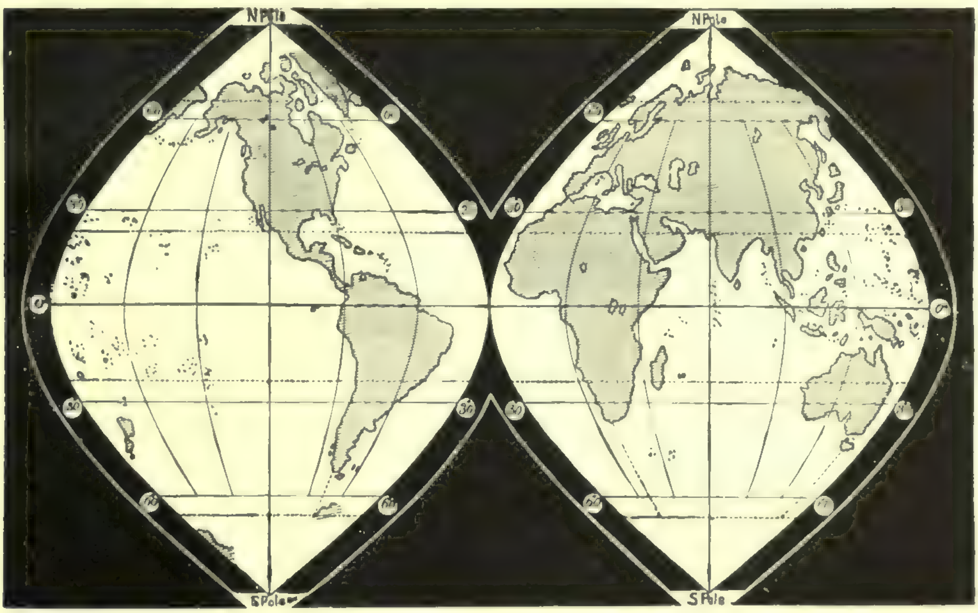


FIG. 73.—The Western and Eastern Hemispheres on an equal-surface projection.

(238.) We can vary the central meridian of our maps according to the features which we wish the map to illustrate. Thus in fig. 72, also from my 'Elementary Physical Geography,' we have the same projection, the central meridian being so selected that the map shows well the relative areas and the positions of the Pacific and Indian Oceans. Comparing this map with fig. 71, we recognise also the relative areas of the Pacific, Indian, and Atlantic Oceans.

(239.) Or the Earth may be represented isographically on this projection modified as in fig. 73, showing the so-called eastern and western hemispheres separately, with less distortion than when the whole sphere is shown without any break.

(240.) Another equal-surface method, devised by me (so far as I know),[1] is repre-

[1] It was independently rediscovered by Prof. Piazzi Smyth, some seven years after it was published in my *Essays on Astronomy* (in 1872), from which fig. 73 is taken.

sented in fig. 74. It is founded on the property that if a sphere is enclosed in a cylinder, any two planes parallel to the base of the cylinder enclose between them equal belts of surface of the sphere and cylinder. Now, suppose that the polar axis of a globe is the axis of the enclosing cylinder, and that this axis is luminous, but can only emit rays of light at right angles to its own length; then, if the meridians, continent-outlines, etc. are opaque and the sphere transparent, shadows of these lines will be cast on the enclosing cylinder, the points on each parallel of latitude being projected in the plane of their parallel, owing to the supposed peculiarity of the luminous axis. If the cylinder be now opened along a line parallel to its axis and unrolled, we shall obtain the isographic projection represented in fig. 74. The construction is simple. Taking a horizontal cross-line to represent the equator, and therefore equal in length to the circumference of the globe, we divide it into equal parts and through the points of division draw perpendiculars representing the meridians: these must be equal in length to the diameter of the globe, and must be bisected by the equator. On the outside meridians describe semicircles (in pencil), and divide their circumferences into half as many equal parts as the equator was divided into, and through corresponding points of division draw parallels to the

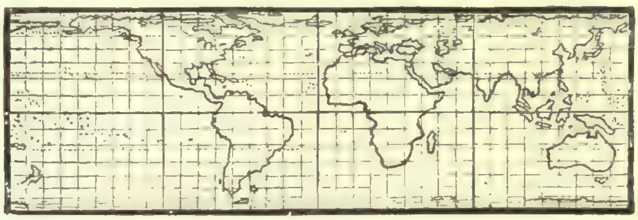


Fig. 74. The World on an equal-surface projection (cylindrical).

equator; these represent latitude-parallels, and in fig. 74 meridians and parallels are drawn to every tenth degree.

(241). For delineating small portions of the Earth's globe various methods have been devised, some of which have been in use for centuries. In fact, the heterogeneous collection of constructions, some of very questionable quality, used in the leading modern atlases, and the entire neglect of any attempt to picture the relative proportions of different countries, or even, in many cases, their actual configuration, must be regarded as not altogether creditable to modern geographers. Considering that for one celestial atlas at least 100 geographical atlases have been published, there is more reason to look for variety and originality in these atlases than in star-maps; whereas the same projections are used, in nearly all those cases which present any difficulty, in the geographical maps of our time as in those which were in vogue 200 years ago! Consider, for instance, the map of Africa in any modern atlas. Here the projection used is that already described as devised by Flamsteed (the first Astronomer-Royal, more than 200 years ago) for use in star-maps. If we suppose the central part of the equator in Africa brought to the centre of the projection in figs. 69, 70, and 71, we get the construction used for Africa in modern maps. The result is that in the neighbourhood of the two central cross-lines we have little distortion; but, as we leave them, and especially as we approach the corners of the map, the distortion becomes very marked. Spain, for example, and Asia Minor are so distorted that to the young student they seem like different countries from Spain and

Asia Minor as they appear in a map of Europe. It may be said that since Spain and Asia Minor are not parts of Africa, this distortion does not matter. But in reality it does matter considerably, because the student seeing these parts distorted naturally distrusts the whole map. Moreover parts of Africa, as Morocco and Egypt, are nearly as much distorted as these outlying portions ; though the student, who is apt to be more familiar with the forms of Spain and Turkey-in-Asia, notices the distortion in their case more readily.

(242.) Again, in the map of Asia, a curious modification of Flamsteed's projection is used with still more misleading results. Equidistant concentric circles replace the equidistant parallels ; and arcs are measured along those circles, of equal length along any of them, but diminishing with distance from the equator as in the other case. The distortion thus resulting in such portions of the map as Egypt, New Guinea, Kamschatka, and north-western Siberia, is monstrous considering the purposes such maps are meant to subserve. Sweden, which comes in with the map of Asia thus formed, is egregiously distorted. It will be found amusing and instructive to trace in the outline of Sweden from one of these maps of Asia, and directly compare the figure thus obtained with the shape of Sweden as shown in a map of Europe, or still better as shown in a map of Sweden itself.

(243.) Much better maps of Africa, Asia, and North America can be formed by using meridians and parallels which can be drawn with far less trouble. In the case of Africa, indeed, a series of equidistant rectangular cross-lines, dividing up the chart into squares, gives results altogether more exact and trustworthy than those shown in our atlases. With Asia and North America we cannot get such results quite so simply ; still we can get them simply enough by using the conical construction, of which, indeed, the very simple construction just indicated for Africa, South America, and any region in a terrestrial or celestial map crossed centrally by the equator, is but a special case.

(244.) As the conical construction is of great value to the student of astronomy, I shall give here a sufficient though brief account of the processes necessary for making maps on this plan. There is no easier way of learning to know the stars than by occasionally mapping a constellation or a star-group, until one has filled a portfolio with useful sketches of the heavens. In well-constructed maps, on a large scale, and comprising but a small region of the heavens, we can mark in the place of any double star or other object of interest we may wish to examine, and so immediately learn whereabouts to look for it on the heavens. One cannot do this with ordinary star-atlases ; because each map covers so large a part of the heavens that the scale must be small or else the maps will be unwieldy ; while too often, from the same cause, the maps are in places so distorted that even if one marks in a star by the data given in the list of objects, one can yet form no clear notion of its position with respect to neighbouring objects. Besides, it is pleasant to have large-scale sketches of favourite groups or constellations. In such sketches one can do what is quite impossible in small-scale maps, viz. give to the large stars something like their true proportionate magnitude (size being the only means we have for representing brilliancy) without their covering a space as large perhaps relatively as four or five moons.

(245.) I propose to give some simple rules, by following which the student can lay down in a few minutes the meridians and parallels corresponding to any part of the heavens, afterwards filling in the stars at leisure :—

The true shape of a strip of the globe, between the pole and the equator, and bounded by meridians five degrees (say) apart, is shown on a small scale in fig. 67. The bounding lines, though slightly curved in reality, may be looked upon as straight for any short portion of their length without introducing sensible error. It is on this fact that the conical construction depends. If any portion of the strip represented in fig. 67 had really straight edges, we could bring another similar portion alongside of it without gaps or overlapping, another next to that, and so on, until we had space enough for a constellation or star-group belonging to that part of the globe. By *making* the sides straight, which we can do without appreciable error, we get such a space, the meridians and parallels of which are sensibly correct; and there is no accumulation of error as in other modes of mapping. Each space has the same error of shape as its next neighbour—no more and no less.

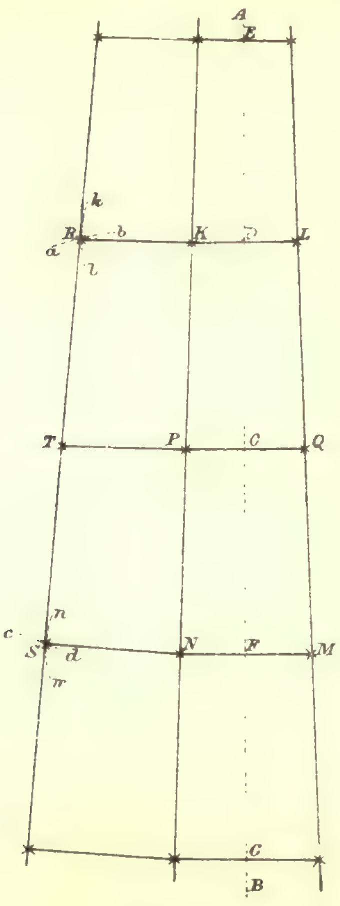


FIG. 75.
Illustrating a method of readily constructing accurate Maps.

The numbers down the left side of fig. 68 indicate the proportionate length of the cross-lines opposite them; the numbers down the right-hand side indicate the distance from the equator in degrees.

(246.) Now suppose we wanted to make a map of a part of the sky the middle of which is 55° from the equator—there or thereabouts. And suppose further that the length of the region we wanted to map measures, from north to south, about 20°. Then the shape of a strip between two meridians, 5° apart, should be the same as the shape of the part of fig. 68 between 45° and 65°. But we must straighten the sides. We do this with the least possible error by joining the ends of the cross-lines opposite 40° and 60°.

(247.) Fig. 75 shows this done on an enlarged scale.

Draw first the central line A B. Along it measure off four equal parts, C D, D E, C F, and F G. The length of any one of these parts is to be the basis of measurement. Divide one of the parts C D into ten parts, and one of these again into ten; this will enable us to measure off any number of hundredths we please. A plotting scale may be made in the usual way.

Draw the perpendicular cross-lines K D L and N F M, and take K D and D L each 25 hundredths of D C—the number 50 being found opposite 60° in fig. 67. Similarly take N F and F M, each equal to 32 hundredths of D C, draw the lines K N and L M. These are two of our meridians.

Bisect K N in P, and take off parts equal to K P or P N, along the produced line. Do the like with L M. The points obtained in this way belong to the parallels of our map. (It need hardly be said that if the range of the map is greater, the line we should get corresponding to K N would have to be divided into more than two parts.)

(248.) Now notice that all we require further is to repeat the figure we have obtained as often as may be required to give the map the necessary breadth to include our star-group. The following is the geometrical construction for the purpose (it is simple enough, but an easier and much more exact method follows it):—

With K as centre and radius K L describe the arc *k l*; with N as centre and radius N M the arc *n m*; and with radius N L or K M, and centre successively at K and N, describe the arcs *c d* and *a b*. The points R and S thus obtained belong to the meridian next to K N: we join them and divide as we divided K N and L M. Next, on the side L M we repeat the process. Then on the side R S, and so on, on alternate sides, until we have as many meridians as we want. The construction has also given us the points along our parallels, and we can join these as shown in the figure. Our map is then ready for filling in the stars.

(249.) That is the mathematical method; but the following being easier and more accurate is in truth more scientific:—

Having drawn and divided the lines K N and L M, fig. 75, place a piece of tracing-paper upon the figure and mark in all the division-points. Then shift the tracing-paper so that the division-points at L and M fall respectively on K and N, the division-points which had been at K and N now falling on R and S respectively. With a fine pointer mark the five points along this line, *through* the tracing-paper. Then, shifting the latter so that the points originally at L and M fall on R and S, repeat the process. Continue this on the left-hand side of A B, as far as may be required, and so repeat the process; and do the same on the right-hand side of A B: the points wanted are thus obtained with the utmost ease and certainty.

(250.) The meridians being straight, nothing need be said about the way of drawing them in. The parallels are parts of circles, and when the meridians corresponding to K N and L M intersect at a convenient distance, the point of intersection is to be used as the centre of these circles.

(251). Fig. 76 is an example of the conical construction employed for mapping the constellation Cassiopeia on a small scale. The central meridian *a b* is divided into equal parts in *c*, *d*, *e*, and *f*; from centre *g* arcs *h r*, *l s*, and from centre *k* arcs *l s*, and *h r*, give the points *r*, *s*, and the construction proceeds as already described. Then the stars are filled in from the star-atlas or catalogue, as much detail being introduced as may be required.

(252.) In fig. 77, showing the constellation Orion, we have a map representing a region centrally crossed by the equator. For such a region the conical construction becomes cylindrical (the apex of the imagined enveloping cone passing off to an infinite distance), and the meridians as well as the parallels are equidistant straight lines, the enclosed spaces being square.

(253.) In fig. 78 parts of the constellations Sagittarius and Capricornus, which lie to the south of the equator, are mapped on the conical construction, the meridians closing in towards the bottom of the map in this case. The map shown in fig. 78 is supposed to have been constructed for the purpose of showing the path of the planet Jupiter during the opposition of 1866. The student of astronomy will find it an instructive exercise to draw the paths of the planets from the data given in the 'Nautical Almanac' (five years in advance) on maps constructed in this way. But he

Fig. 76.—The constellation Cassiopeia mapped on the conical construction.

Fig. 77.—The constellation Orion similarly mapped.

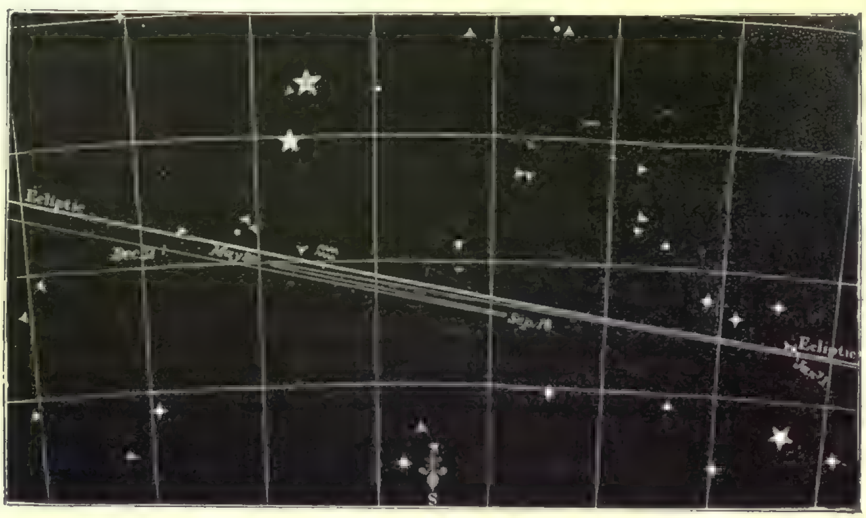


Fig. 78.—Parts of Sagittarius and Capricornus on the conical projection, showing the path of Jupiter during his Opposition in 1866.

should employ a larger scale than that of figs. 76, 77, and 78. The twelve zodiacal maps in the next chapter, showing the sun's position from day to day upon the celestial sphere, were constructed for my own use in such work, tracings being made by me from the original drawings whenever I had occasion to mark in a planet's course.[1] For the planets never leave the zone presented in those twelve maps. The maps do not seem to be on the conical construction; for, in fact, the meridians and declination parallels, which alone show in the maps, were not the lines originally laid down. The lines of celestial longitude and latitude, which bear the same relation to the ecliptic that the lines of terrestrial longitude and latitude bear to the Earth's equator, were first laid down, on the conical—which, for the zodiacal zone, becomes, of course, the cylindrical—construction; then the declination circles and parallels were marked in, according to their proper positions in longitude and latitude. But the maps have all the accuracy of the cylindrical projection, and have this advantage—a property never before presented in zodiacal maps that the parts which overlap are identical. Or, if it be preferred, each map may be limited to the sign it represents by cutting off all outside the 30 degrees belonging to that sign; after which the twelve maps can be formed into one long strip showing the whole zodiac on the true cylindrical construction.

(254.) Students of astronomy who use star-maps like those in my 'Library Star-Atlas' and 'School Star-Atlas' will often find it convenient to make enlarged drawings of small parts of the heavens, and for this purpose the conical construction as above described is the readiest which can be used, and sufficiently exact for all practical purposes. If enlargement is not needed, a tracing from the constellation, as shown in the atlas, will serve rather better than a conical construction, unless the stellar region to be drawn falls near or across the edge of one of the maps. Even then the meridians and parallels for the map can be traced from a part (in the proper declination) nearer the centre of a map, and the stars then marked in, with less trouble than making a conical construction would give. For my own work, in this way, I use the original drawings for my 'Library Atlas,' which are on the scale of a 30-inch globe. (My 'Library Star-Atlas,' in the later editions, photo-lithographed

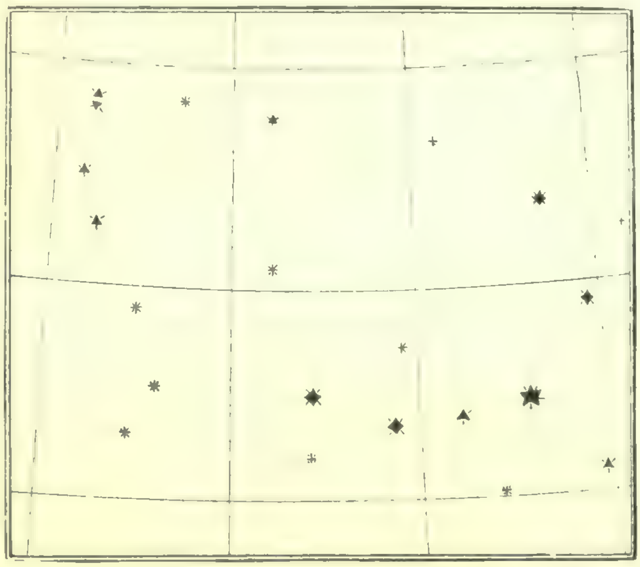


FIG. 79.—The Northern Crown on the conical projection, scale of a 2-foot globe.

[1] It was because I had thus found them useful in my own work that I republished them—in white, and in black without the sun—among many other illustrations in *The Seasons Pictured.* I may remark that a large proportion of the books I have hitherto published have been in like manner suggested by the convenience I had found in using material originally collected for note-books or for my astronomical portfolio. I have written and drawn little with the primary idea of teaching anyone but myself.

rather larger than the first, is on the scale of a 19-inch globe.) But students using the large gnomonic atlas published by the Society for Diffusing Useful Knowledge will find it nearly always necessary to lay down meridians and parallels (for their portfolio-maps) on a construction showing small distortion, filling in the stars from their indicated right ascension and declination in the atlas. Otherwise the smaller star-groups will often be scarcely recognisable so far as shape is concerned, while their scale may be so altered that the comparison with other star-groups not so distorted may be altogether misleading. For instance, in fig. 79 the well-marked group of stars forming the Northern Crown (probably, also, in ancient times, the uplifted arm

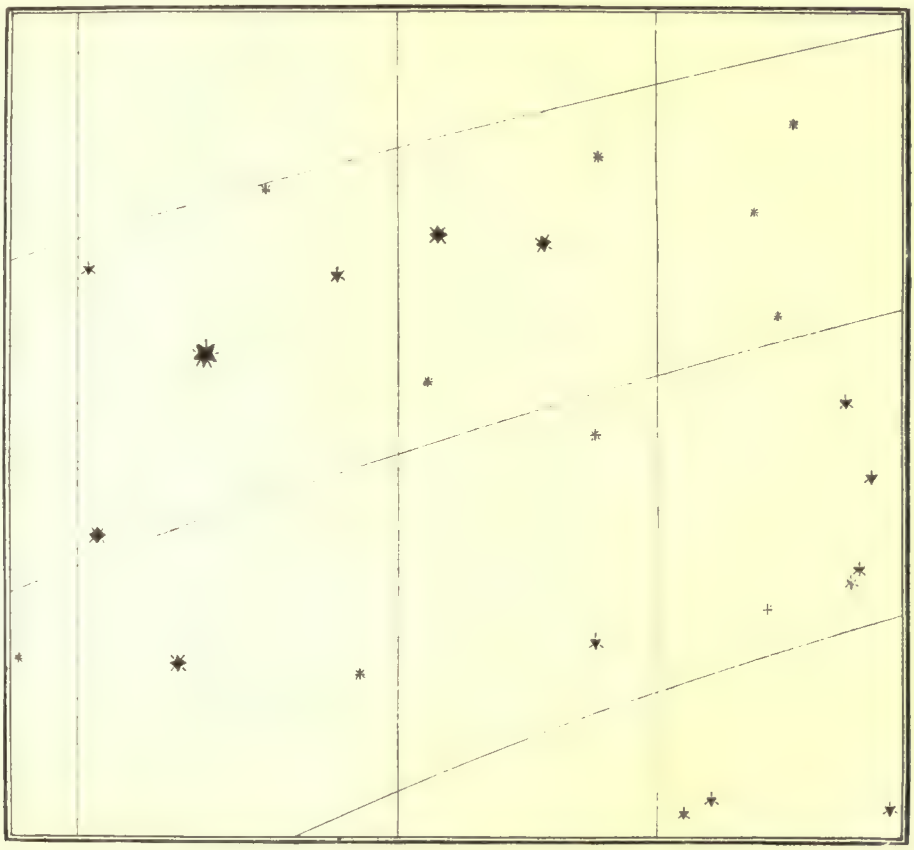


Fig. 80.—The Northern Crown, on the gnomonic projection of same scale (pricked off from the large maps of the S.D.U.K.)

of the Herdsman) is represented on the conical projection on the scale of a globe 2 feet in diameter. Fig. 80 represents the same group as it appears in the gnomonic maps of the S.D.U.K. *on the same scale* at their centres, but, as the figure shows, monstrously distorted and changed in scale near their edges.

(255.) For the ready construction of geographical maps, showing small regions, the conical projection is the best. Our atlases would be greatly improved if this projection were applied also to large regions. But in reality we much need, alike for geographical and celestial atlases, the use of a uniform method of projection. In an atlas of high class every map of a large area should be on the equidistant central projection. All distances from the map's centre are thus on a constant scale; while changes of scale

and distortion not only take place uniformly with increase of distance from the centre, but are very slight (as my 'Library and School Star-Atlases' show) even at distances of thirty or forty degrees from that point.

(256.) The plan which is undoubtedly best for a celestial atlas, and as certainly best for any series of terrestrial maps intended to be drawn to a single scale, is that of dividing the globe into twelve parts corresponding to the twelve faces of an enclosing regular twelve-faced solid (the dodecahedron) pictured in fig. 81. Each face is a regular pentagon, and in fig. 81 P is supposed to represent the point in which the north pole of either the celestial or terrestrial sphere touches one face, the south pole touching in like manner (necessarily at its centre) the opposite face, and the plane of the equator cutting the remaining ten faces in the regular decagon A B C D E, etc.

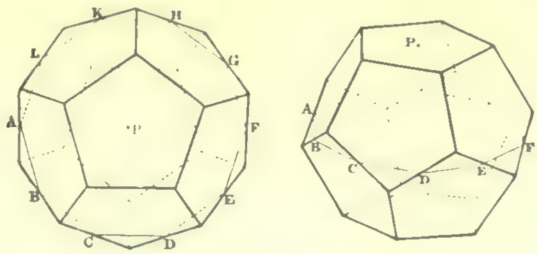


FIG. 81. The Dodecahedron.

(257.) Fig. 82 represents the way in which the twelve maps may be arranged, each being so projected as to form a perfect pentagon, viz. into two sets of six, each showing five equal pentagonal maps on the five edges of the central or polar pentagonal map. Fig. 82 illustrates the actual arrangement adopted in my 'Gnomonic Star-Atlas,' the lines and letters in the figure all relating to celestial relations. Thus *e e e* represents the ecliptic, which has no place in terrestrial maps; *n p* and *s p* are the north and south poles of the ecliptic; E C and S C are the colures, equinoctial, and solstitial respectively. In my 'Library Star-Atlas' (to which the maps of the 'Gnomonic Atlas' served in the first edition as index plates) the twelve maps are circular, so that they overlap; a convenient arrangement for passing from one map to another. The same arrangement is adopted in my 'School Star-Atlas,' which is, in fact, a reduced and simplified edition of the larger work. In each atlas the overlaps are of such extent that, instead of showing only a twelfth part, each map shows a tenth of the star-sphere.

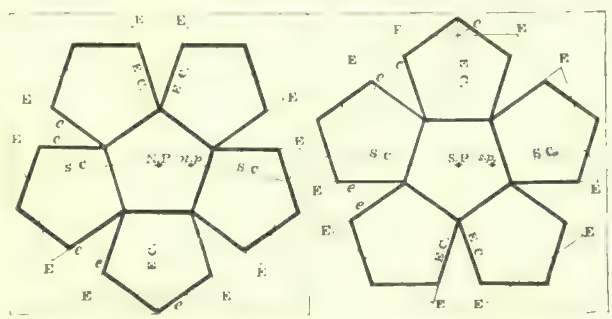


FIG. 82. Illustrating the division of the Globe into twelve five-sided Maps.

(258.) The chapter on the Earth as a planet may be regarded as illustrated by a series of twelve coloured maps of the Earth on the same plan (that is, on the equidistant projection, and similarly distributed into two polar maps, and ten others crossed alternately, near their southern and northern edge, by the equator) which I have formed into a terrestrial atlas (on the same scale as my 'School Star-Atlas'). This will, I think, form an atlas convenient not only for use in the study of the Earth in its astronomical aspect as a planet, but also for use by the student of geology, geography, history, and travel.

(259.) The two maps, Plates III. and IV., are useful illustrations of the Earth, as viewed in the present chapter. Plate III. shows the greater part of the Earth in a planisphere, having the north pole as centre. Plate IV. shows an equal portion in the same way, but with the south pole as centre. These planispheres are on the stereographic projection, and, although the distortion outside the equator in each map is enormous, the proportions of small areas are correctly presented. The maps in this respect resemble Mercator's charts, but have the great advantage of being central and polar projections. It may be interesting to the young student to observe that if a surveyor in any part of the Earth imagined the Earth to be a plane surface with a pole (either the northern or southern) as centre, the proportions of the region he surveyed, wheresoever he might be, would force him to the conclusion that the set of relations among meridians and parallels presented in one or other of these maps must be that existing among the meridians and parallels of the Earth: for, the true *proportion* between degrees of longitude and latitude prevails in every part of each map. It will be seen from Plates III. and IV. how greatly the *scale* of different parts of the earth, as estimated by degrees of longitude and latitude, would seem to differ.

(260.) A number of interesting features of the Earth as a globe will be found to be illustrated by the maps in Plates III. and IV. when thoughtfully examined with due reference to what has been explained earlier respecting the stereographic projection. Among these may be mentioned the relation which originally led me to the construction of these maps. It is often convenient for the student of astronomy, as well as for the seaman, traveller, or surveyor of lines of communication on the Earth, to know how to draw, in terrestrial charts, the shortest course between stations far apart. As we know that the great circle connecting two such points is always the shortest course between them, the maps in Plates III. and IV. considered in connection with the known properties of the stereographic projection enable us at once to draw the shortest courses between any two stations shown in either of these maps. The great circle between two points A and B will be represented on either map by some circle passing through the two points; and to get the right circle we may have to notice that a great circle through any point on the Earth necessarily passes through the point's 'antipode.' All we have to do, then, is to find in the chart the antipodes either of A or B (preferably of both), then the circle through A B, and the antipode of either will be the great circle required, and will pass through the antipode of the other.[1]

[1] I have constructed stereographic charts of the Earth 19 inches in diameter, of which indeed Plates III. and IV. are photographic reductions, specially for the use of seamen; for which purpose such charts seem specially suited, since not only can the great circle course from any point to any

(261.) The student of the Earth will find it a useful exercise to make a tracing of such charts as are shown in Plates III. and IV., and on this tracing to mark in by the method indicated the great circle-tracks, joining a number of pairs of places taken at random. This will give him a good idea of the actual effects of the rotundity of our Earth's shape in determining the tracks connecting different parts of her surface.

other point be indicated by the above easy method in ten or twelve seconds a composite course in half a minute—but, precisely as in Mercator's projection, the true bearings of the course are indicated everywhere along its length. Details of construction and interpretation are indicated in a small accompanying pamphlet.

CHAPTER III.

APPARENT MOTIONS OF THE SUN, MOON, AND PLANETS.

(262.) The observations considered in the beginning of Chapter II. taught the ancients to regard the Earth as a globe set in the midst of space. The region surrounding the Earth's globe appeared to have the form of a vast sphere on whose interior surface the stars seemed set in fixed positions. This star-strewn region around them appeared to move as if turning with absolute uniformity round a fixed axle, which for each station on the Earth seemed more or less inclined to the horizon-plane, so that one axle-end or pole rose above the horizon. The absolute uniformity of rotation, recognised in the stellar sphere, would naturally have led the astronomers of old times to take the rotation of the star-sphere as the most convenient measure of time; but long before these features had been recognised by the astronomers of any race, custom had thoroughly established the method of measuring time by the more obvious solar day, which was not easily displaced even among astronomers. Moreover the measurement of time was not so precise in those days that the inequality of solar days (even when measured from noon to noon, not by the mere duration of sunlight) would be clearly recognised.

(263.) At an early stage of the study of the heavens astronomers strove to interpret the movements of those bodies which, unlike the stars, did not retain fixed positions on the great enclosing sphere. These bodies, the sun, the moon, and five which looked like stars but seemed to move among the stars, they regarded as capable of independent movements whose laws might be ascertained. It seemed to them, indeed, that it might be well worth their while to determine the nature of those movements, since they saw reasons for believing that the fortunes of men and nations depended in large degree on the influence of the Sun and Moon, Mercury, Venus, Mars, Jupiter, and Saturn. These seven orbs were for the ancients the seven moving bodies, or planets, i.e. wanderers. Astronomers regarded these orbs as altogether unlike the fixed stars, which rather seemed to mark the pathways of the planets than to be in any way akin to them. The planets exerted their influences

in varying ways as they varied in position. The fixed stars seemed arranged in groups of various forms, more or less striking and suggestive, never changing in form either as their diurnal rotation carried them round the polar axis, or as the year progressed, though they came to their culminations at different elevations, or at changing hours, as the observer changed his place upon the Earth.

(264.) The first of the moving orbs to be considered were naturally the sun and the moon. The latter was the more easily dealt with, because her movements over the star-sphere could actually be watched, whereas though it was obvious that the sun also moved on the star-sphere, his place day after day among the stars could not be directly observed, since the stars are invisible when he is shining. Yet as the sun's apparent movements on the star-sphere are simpler than those of the moon, we may with advantage take them first, then the moon's, and then the movements of the planets. Although we consider these movements as they appeared to the astronomers of old times, it is well for students of astronomy (especially the younger who have more time for such inquiries) to observe the sun, moon, and planets for themselves, after the manner indicated here; for by such observations much clearer ideas of the apparent mechanism of the heavens will be gained than by merely reading descriptions. The movements of the sun, moon, and planets are still in progress: they can be watched in the heavens themselves—now as in the days of yore.

(265.) Observing the sun during any one day after the manner already described in the first chapter, one might suppose that he is carried round by the same rotation of the celestial sphere which seems to sway the unchanging star-groups round the polar axis—visibly during the night, and actually though not visibly during the day. For several days in succession the sun may be watched revolving apparently with the utmost steadiness round the polar axis of the heavens, even as any star, watched in the same way, is observed to do. And doubtless in the childhood of any race, when days and weeks seem long—in this sense, that memory goes back little farther and foresight cares little to look much farther forward—men noticed little more than this.[1] We have to look very far back towards the savage stage of each race's life to recognise the time when the rising of the sun was a matter to be rejoiced over, his setting an occasion for anxiety—though the religious as well as the superstitious of every race, even the most cultured, are permeated with the products of those long-forgotten emotions of anxiety and of hope.

(266.) The changes, however, which affect the sun's course from day

[1] The recognition of the sun as 'ruler of the day' belongs to a much earlier stage of the development of races than the recognition of his more important office as ruler of the year; yet even the latter belongs to the childhood of races.

to day throughout the year were such that long after the childhood of each race was passed they must have been watched,—with hope as the sun seemed to gain in power, with anxiety, even with terror, as his influence seemed daily to fail during periods so long that the hearts of all but the priests of the sun (the astronomers of those days) must have sickened with the fear that he was passing away for ever from the world he had nourished and illumined.

(267.) Let us suppose our observations to begin at the time of the autumn equinox. The sun has been passing day after day for weeks towards the south, his rising place drawing nearer and nearer towards the east and his setting place nearer and nearer towards the west, while day after day he attains less and less elevation above the southern horizon. We have seen how these movements can be followed with the rough equatorial pointer pictured in fig. 2, p. 20. To determine, however, more precisely the actual changes of the sun's elevation, suppose that we set up a pointer, A B, fig. 83, as the Egyptians set up an obelisk, to indicate by the length of its midday shadow B *b* *e*, the midday elevation A *e* B of the sun. Then we should find the shadow as observed day after day at noon growing longer and longer, but changing less and less each day, until at last its extremity would have reached the position *w*, and (somewhere about December 17 or 18) we should be unable to recognise any further increase. The

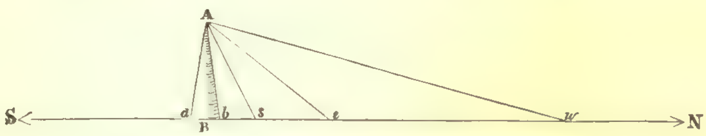


FIG. 83.—The midday shadow of the Sun.

midday sun would seem to occupy for several days in succession the same unchanging position on the southern sky, so far as this method of observation was concerned. Continuing to note the shadow, however, we should find that somewhere about December 25 the extremity of the noon shadow would show signs of returning towards *e*. At that time, then, the noonday sun would seem no longer to occupy the same unchanging position in the southern sky that it had held during the preceding week or so. What the old astronomers regarded as a real solstice, or standing still of the noon sun during a measurable time—not a mere momentary stay as his direction of apparent motion on the meridian changes—would be over. From that day our modern student of astronomy can watch with more or less interest the sun's gradual increase of midday elevation ; but the observer during the childhood of any race regarded that day with a much deeper interest, nay, rather with fear and anxiety. It was for him the birth of the sun as ruler and god of the year.

(268.) The sun gradually passes to his lowest position, and thence returns (regarding only his meridian elevation) without any actual standing still. To determine the true time of the solstice by the length of the midday shadow cast by A B in fig. 83, the observer should notice on what day the increasing shadow has a certain length, measurably less than its maximum, and again the day when the diminishing shadow has that same observed length ; the day midway between these will be near the solstice. He will also perceive the advantage of substituting for the horizontal plane surface S N of fig. 83, a fixed surface on which the shadow of the point A is thrown more squarely,

as in fig. 84, to receive the shadow of the horizontal cylinder c C c'. For this purpose it will be well to use a strip of card set quadrantally, as in fig. 84, to receive the shadow of the horizontal cylinder c C c', with its axis east and west, at the centre of the quadrant, the arc A B being carefully divided into ninety equal parts or degrees with transversals for smaller divisions if the scale will permit. C K L M, a flat quadrantal card, will be useful by enabling the observer to set the arc A B in the meridian by getting the shadow of K L M upon it at true solar noon, obtained from the almanac and a good clock. But I do not enter into details either of arrangements for making such observations, or as to the methods probably adopted by astronomers of old times, because my purpose here is simply to show how the first comparatively rough determinations of the solstices and equinoxes were effected, and to indicate the significance which the sun's varying midday position throughout the year would have for the observers of old, regarding him as the god of the year, giving life as well as light and warmth.

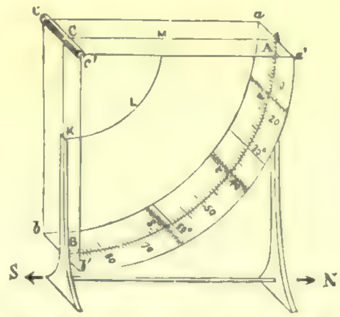


FIG. 84. A simple contrivance for following the Sun's midday shadow during the year.

(269.) Day after day from the observed time of the winter solstice, or birth of the new year's sun, the midday shadow observed in fig. 83 grows shorter and shorter, or passes lower and lower, as observed in fig. 84, indicating the growth of the sun in midday height, that is, in strength and light. By the method illustrated in fig. 2 it can be seen also that the place of sunrise shifts towards the east, and the place of sunset towards the west. Moreover, it is found that these changes take place with increasing rapidity, until when the sun is approaching due east and west at sunrise and sunset respectively, his midday elevation, as indicated by the position of the shadow (figs. 83 and 84), increases at the rate of two-fifths of a degree daily. At this time the direction of the pointer of such an instrument as is shown in fig. 3 is nearly at right angles to the polar axis, and the sun is nearly on the circle midway between the poles of the heavens. On or about March 20 it is found that he is actually upon that important circle, or crossing the equator, passing from the winter half to the summer half of his career. It is evident if we consider the motion of the pointer of fig. 3, directed sunwards at midday, and carried round by the rotation of O P on its axial pivots, that in this position only does the pointer move in a plane—the plane, namely, at right angles to O P through the pivot of the pointer. Thus if in the instrument of fig. 84 we substitute a flat card or other plane surface for the cylinder C, and set the card so that at any hour of the day it casts a linear shadow on the arc A B, the shadow will remain linear throughout the day. This is a ready, though of course only a rough, way of determining when the sun is on the equator.

(270). After the equinox the sun still passes higher each day at noon, the shadow observed as in fig. 83 still growing shorter and shorter. The sun's point of rising passes steadily to the north of east, and his place of setting to the north of west. To the observer merely intent on noting the sun's movements these changes are of interest as indicating how the sun's distance north of the equator increases,—at first rapidly, afterwards more slowly as he approaches the summer solstice. To the earlier observers of the sun's annual motion as measured by his midday elevation these movements indicated the steady ascent of the ruler or god of the year above that median line which

separates the region of his summer glory from the region of wintry imprisonment or burial. It is because of this idea that we find the sun's motion from the time of his passing the equator called his Ascension—a term which seems ill chosen if not unmeaning as the astronomer has used it in later times, that is, within the last two or three thousand years. It is obvious that the first observers, who thought much more of the sun as the life-giving and life-preserving ruler of the year than as an astronomically celestial body, would pay much more attention to the apparent motion of the midday sun athwart the mean position he occupied in spring and autumn than to his motion along his apparent path on the star-sphere, about which they probably knew little.[1] So that when he began to rise above his mean position on the equator, they would speak of that as his rising or ascension, and measure such ascension from that crucial rising point. Each year's passing of the sun above the equator would in those earlier days, the childhood of races, be not only a passover of their god, but as truly for them a resurrection as the daily rising of the sun had been for their still more simple forefathers.[2]

(271.) Thence onwards the sun's midday elevation is observed to increase, the shadow, as observed in fig. 83, growing shorter, and, as seen in fig. 84, passing still lower. As midsummer approaches the daily change grows less and less, till at about June 17 it becomes insensible. Then comes the summer solstice, and the beginning of the sun's apparent return towards the equator, just observable about June 24 or 25.[3]

[1] It is the same with the rising and setting of the sun; to this day scarce one observer in a hundred notices the motion of the sun from left to right when rising and setting, the rising or sinking of the sun being all that attracts attention. Yet in latitudes higher than 45 degrees the former motion is more rapid than the latter.

[2] The ancient Easter festival corresponded as closely with the rising of the sun above the equator as observances at sunrise (that is, at the daily eastering of the sun) correspond with the rising of the sun above the horizon. It is probable that the forty days of Lent, or lengthening of the days before the passing of the equator, were originally derived from Egyptian sun-worship; since forty days before the vernal equinox the sun as seen from the Great Pyramid Observatory-Temple would at midday be just half-way between the horizon and the zenith; a passage doubtless held to be scarcely less critical than the passage of the circle midway between the poles. Forty days after the ancient Egyptian passover the sun at midday would be half-way between the equator and the zenith, as seen from the Great Pyramid. This would correspond with our Ascension Day (forty days after Easter) now, as in the days of the sun-worshippers of old, were Easter now kept at the time of the equinox, or rather on the day corresponding to March 25. It is noteworthy that March 25, the sun's true crossing time, when December 25 and June 24 mark the solstice, remained long a festival day among ancient races. The first of May, again, was the true Saxon Ascension Day, and many of the ceremonials of the former fixed feast day correspond with those which, in Christian times, were assigned to the corresponding movable feast. On the fortieth day after the equinox the sun crossed the 15th parallel of declination into the mid-heaven of the Great Pyramid skies, i.e. 15 degrees from the zenith, and it was doubtless a day of special rejoicing among the Egyptian sun-worshippers. The time of the winter solstice or birth of the sun, of his equinoctial crossing, or passover, of his ascension into the mid-heaven, and of the summer solstice or full glory of the sun-god were astronomically determined dates in the time of the pyramid builders. But in earlier days equinoctial passage could only be inferred from observations of the first full moon occurring after the passage. And precisely as the older way of celebrating the moon's return when the new moon was first actually visible remained in use long after astronomers had learnt how to determine the time of true new moon (her conjunction with the sun), and was held the only proper and really sacred method of observing her; so the older and easier, though rougher way of determining the equinoxes, by observing the next following full moon, remained in religious use long after the proper astronomical methods had been invented. Indeed, these older methods remain in use to this day, as a glance at the prefatory matter of every Book of Common Prayer will serve to show.

[3] As the full moon attains on the average her greatest meridional elevation when the sun's is least, and her least when the sun's is greatest, June 24 may be regarded as in the same sense the birthday of the moon of the year that

(272.) After attaining his greatest elevation at this season the midday sun is found by similar observations to gradually descend again, very slowly at first, more rapidly afterwards, until at or about September 29 he is again on the equator. The time of this descending passage of the equator can be determined as the time of the ascending passage had been. In the childhood of races it was naturally regarded as a time of anxiety and mourning, because the sun-god had passed to the winter half of his career, and was daily losing power as rapidly as at the spring passage he was gaining power.[1]

(273.) And finally during the remaining three months of the year we find the midday sun sinking lower, at first rapidly (about two-fifths of a degree each day), then more slowly, till again on about December 17 he seems to reach his lowest, remaining thus (or in solstice) for a week, after which the first signs of increase of midday height begin to be discernible.

(274.) The exact number of days elapsing between the beginning of one solar year as determined in the manner above shown by the winter solstice, and the beginning of the next solar year, might perhaps be determined within a day or two, but not much more accurately. Since, however, there would be probably no greater error in determining the number of days in two such periods, the error in the determination of one solar circuit would in this way be halved. In three such periods it would be reduced to one-third of its former amount, in four to one-fourth, and so on. We can understand then that even by such comparatively rough methods, continued during many years, ancient races could determine the length of a year with considerable accuracy. A people like the ancient Egyptians, who kept records of days during generation after generation, and dynasty after dynasty, might very well have determined the true length of the solar year, that is, the period during which he passes through all

December 25 is the birthday of the sun of the year. Accordingly we find June 24 regarded as the day of the nativity of the lesser luminary, whose chief elevation is obtained in the watery sign Capricornus, the birth cave (also the stable or stay-ing place) of the sun. Probably on this account, certainly not from historical reasons, June 24 was taken as the day of the nativity of John, the baptizer with water, who decreased as the baptizer with fire increased. But we must remember that in some forms of the solar myth we find the sun of winter, or the watery sun, who had his birth at the summer solstice, distinguished from the sun of summer, or the fiery sun, whose nativity has for its date the time of the winter solstice. It is not known, by the way, at what time or at whose suggestion the days held sacred by sun-worshippers came to be associated with the events recorded in Gospel pages; or rather, it is not known when certain among these events came to be associated with the details of the most ancient solar myth. The emperor Hadrian's well-known letter to Servianus, in which he confounds Christianity with the worship of the sun-god Serapis, shows that in his time the usage had already been adopted and had become already misleading. But this letter, probably written about A.D. 138, probably antedates even the oldest of the canonical gospels—in its final form. Assuredly no contemporary of Jesus is responsible for the many manifestly solar (and mythical) parts of the gospel of Matthew.

[1] We learn from the Jewish Fast of Expiation, assigned to this time, and from the importance attached to its due observance, that among the sun-worshipping races from whom all the Jewish fasts and festivals had been derived, this was the chief fast of the year. And naturally in the old days of sun-worship, alike among the Egyptians and among other races during the simplicity of racial childhood, this descending passage of the sun was a day of gloom and lamentation, when whatsoever soul was not afflicted as the God of the Year sank below the great mid-circle of the heavens, the same was to be 'destroyed from among his people.' The Feast of Tabernacles among the Jews corresponded as obviously with the rejoicings of the people, at harvest and vintage, over the fruits of the sun's summer work. There are abundant traces of this feast among ancient non-Jewish races. Plutarch (*Sympos.* iv. 6) supposes the festival to have been in honour of Bacchus, who was undoubtedly a sun-god.

the alternations above described—within a minute or two of its true length. Thus, suppose the time of the winter solstice determined within ten hours, as it very well might be by Egyptian astronomers, of whose accuracy the Great Pyramid gives us evidence ; and that then after one hundred years had passed, during which the days had all been carefully recorded, the time of the winter solstice was again determined within ten hours : then the error in the measurement of those hundred years would not exceed twenty hours, and would probably not exceed ten hours. Dividing this by 100, we get a possible error of twelve minutes in the measurement of the length of the year, and a probable error of only six minutes. In 300 years, or ten generations, the probable error in the determination of the length of the year would be but two minutes. Moreover, the estimate obtained from the observation of the sun at the equinoxes, when his midday elevation changes most rapidly, would be probably much more exact. The sun's midday height at the winter solstice and at the summer solstice could be very accurately observed, because the change of midday altitude near the solstices is very slow. It would soon be recognised that the sun's midday elevation at the equinoxes is exactly midway between his midday elevation at midsummer and mid-winter. Hence it would be a comparatively easy task, at or near the equinoxes, when the sun's midday elevation changes most rapidly, to determine the time when he was actually on the equator.

(275.) We need not wonder, then, that Chaldæan and Egyptian astronomers had determined the length of the solar year within a minute or two—according to some within a few seconds. Continuing their observations, and keeping their records for many hundred years in succession, they had opportunities of correcting their first rough estimates to a degree which would be marvellous were it the work of but a single generation. It is to be feared that very much credit cannot be given to the astronomers of those old times for their zeal in such researches—at least, it cannot be praised as pure zeal for science, seeing that the observers of those days hoped chiefly for some material advantage from the power of predicting accurately the movements of the sun from day to day and from year to year. Probably also there was strong religious feeling in the matter, though not of the highest type. Those who regarded the sun as a god considered naturally that he would be jealous in regard to the manner of his worship and especially as to the due observance of times and seasons.

(276.) Observing the sun year after year in the manner described above, and noting also the changes taking place in the stellar skies as each year progressed, the astronomers of old times ascertained that the time in which the sun completes those yearly changes which measure the year of seasons is equal to that in which the star-strewn sphere (observed night after night *at the same hour*) completes its annual apparent circuit. The two periods are not absolutely identical, but as observed during many years in succession they appear so.

(277.) The meaning of this clearly is that the sun's apparent track in the star-sphere, could it but be followed by direct observation (that is, were but the stars visible by day) is unchanging year after year. At the beginning of the solar year, that is, at the winter solstice, the sun must occupy always the same position on the star-sphere ; since otherwise his circuit and the annual rotation of the star-sphere would not be synchronous. So with his position at the equinoxes and at the summer solstice. Along one circuit he must travel steadily year after year, always following the same path among the stars.

(278.) To determine precisely what that circuit is would be the next point, then, with the astronomers of old:—

In the first place the observations already made, indicating the rate at which in different parts of the year the sun's distance from the equator altered, sufficed to show that the sun's course upon the star-sphere is a great circle, inclined to the equator at an angle of between 23 and 24 degrees. In the days of the pyramid builders, some 3,400 years or so B.C., the inclination of the sun's track to the celestial equator (not then the same circle as the equator of to-day) was nearly 24 degrees. The observations of Eratosthenes already cited (section 150) show that in his day, which we may set at 220 B.C. (he was born 276 B.C.), the sun's path or the ecliptic was inclined 23° 50′ to the equator. He himself called the double of this angle, or twice the obliquity of the ecliptic, 11-83rds of a circumference—that is, 47° 42′ 39″—making the obliquity of the ecliptic as then observed 23° 51′ 19″·5, or nearly that assigned later by Hipparchus, and (probably not independently) by Ptolemy. This must have been very near the true value; for, though at present the obliquity of the ecliptic is but 23° 27¼′, we know that for many past centuries the obliquity has been decreasing. (It is now decreasing at the rate of more than ⅘ths of a minute of arc per century, or 1′ in about 130 years.) Taking this into account the determination of the obliquity by Eratosthenes was very near the truth. But the Chinese astronomer Tcheou Kong, who in the year 1100 B.C. assigned a value corresponding to about 23° 54′ 2½″ to the obliquity of the ecliptic, was yet nearer the truth. And we may be permitted to believe that the astronomers who built the Great Pyramid came nearer still to the value of the obliquity in their time, which must have exceeded 24°.

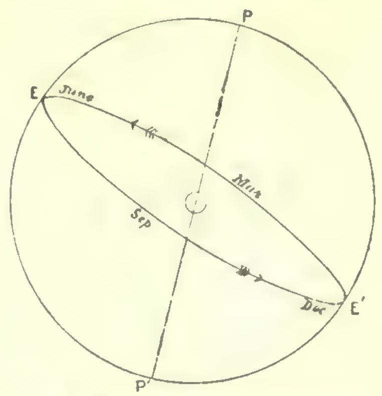


FIG. 85.—The Sun's path or ecliptic.

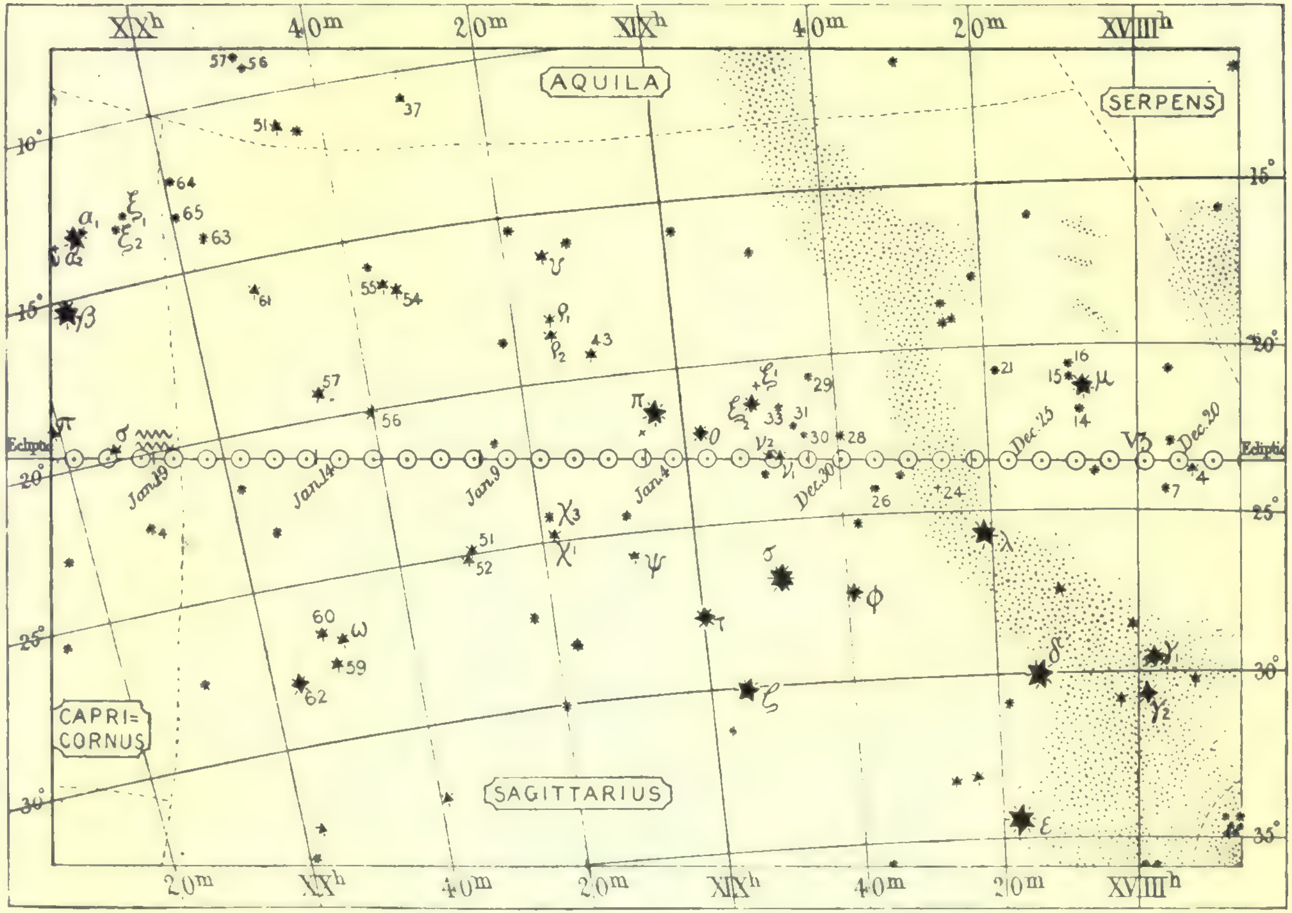
(279.) The sun's path, then, was early recognised as a circle such as E E′, fig. 85, not (like the equator) at right angles to the polar axis P P′, but inclined to the equator at an angle of about 24°, and now at an angle of 23° 27′ 15″.

(280.) Further, the ancient astronomers would be readily able to determine at what points on the star-sphere the sun's path or ecliptic crossed the equator, and attained its greatest distance north or south of that circle. Suppose, for example, that it had been ascertained by midday observations that the sun had crossed the equator ascendingly at a particular moment; then all that would be necessary to determine the whole track of the sun on the star-sphere would be to ascertain what point on the star-sphere observed at night was on the meridian exactly twelve hours after (or before) that moment, at the known elevation of the celestial equator above the southern horizon. This would be the position of the point on the ecliptic where the sun crosses the equator descendingly. Half-way round the equator from this point would be the point where the sun crosses the equator ascendingly; and midway between the points thus determined would be those where the sun's path attains its greatest distance north and south of the equator. Thus the equinoctial points and the solstices would be determined by this single observation, though of course repeated observations and time-measurements would be required to determine these points satisfactorily.

(281.) The actual track of the sun among the stars was nearly the same three or four thousand years ago as now. At least we need not consider just here the change which we now know to be produced by the disturbing action of the planets on the Earth in her actual course around the sun—modifying therefore the apparent course of the sun around the Earth. The position of the points where the sun crosses the equator, and attains his greatest distance north and south of it, has changed, not through a change in the course of the sun around the star-sphere, but (chiefly at least) through a change in the position of the celestial equator, or (in reality) of the axis on which the Earth rotates. Thus the present track of the sun around the star-sphere as pictured in the twelve zodiacal maps of my 'Seasons Pictured' (figs. 86, 87, 88, . . . 97) has practically been the track of the sun since astronomical observations began.

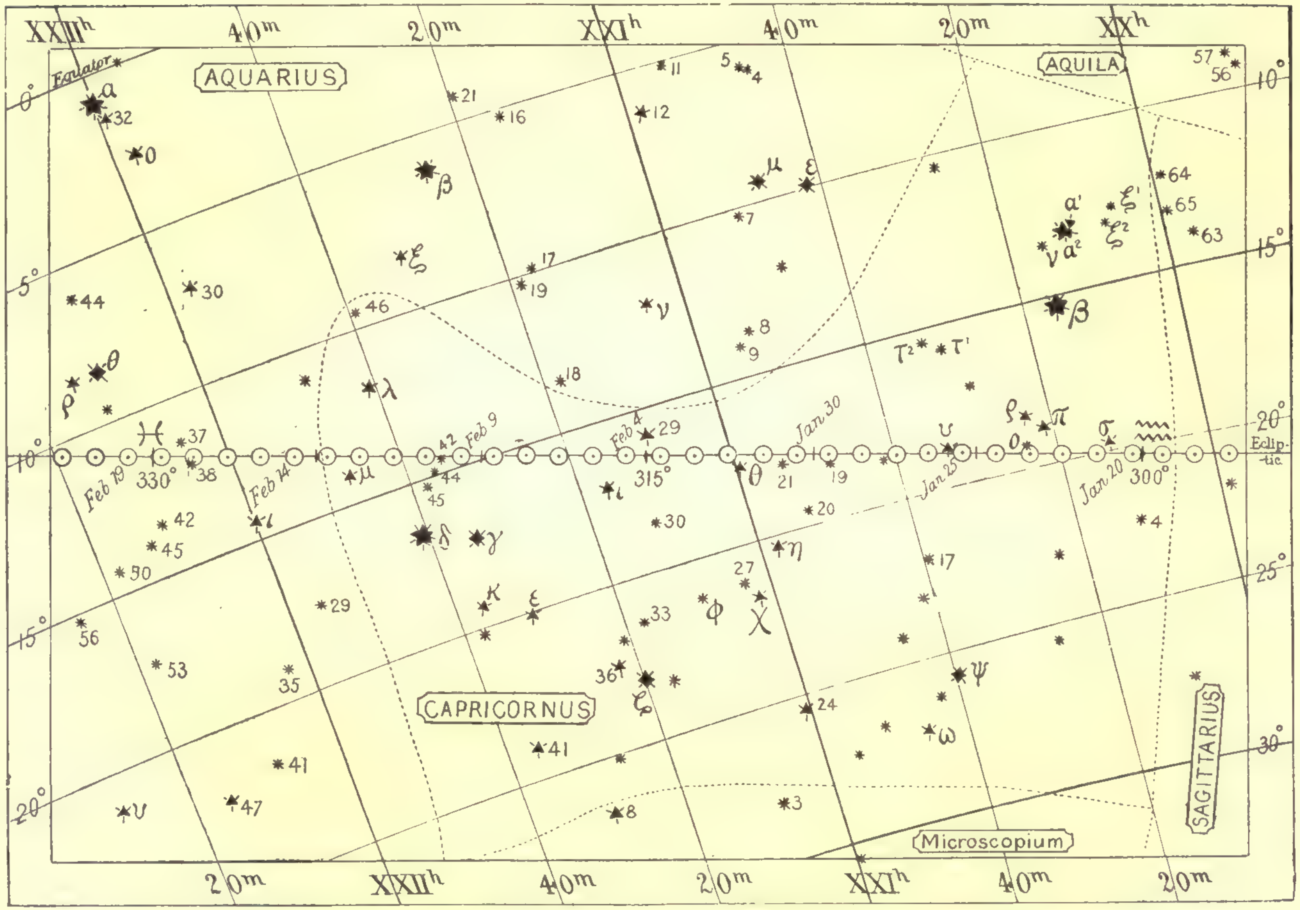
(282.) The student will find it a useful exercise to follow carefully the sun's progress through the zodiacal signs as illustrated in these twelve maps, noting both the present course of the sun in each month and the course which he pursued during the corresponding months in the days of the astronomers of old times. Many curious astrological superstitions (some of them still surviving) and many traditions bearing on religion and on history find their interpretation in the relation which the sun's course in old times bore to the zodiacal constellations as the ancient astronomer interpreted their configuration.

FIG. 86.—THE SUN'S COURSE THROUGH THE SIGN CAPRICORNUS.



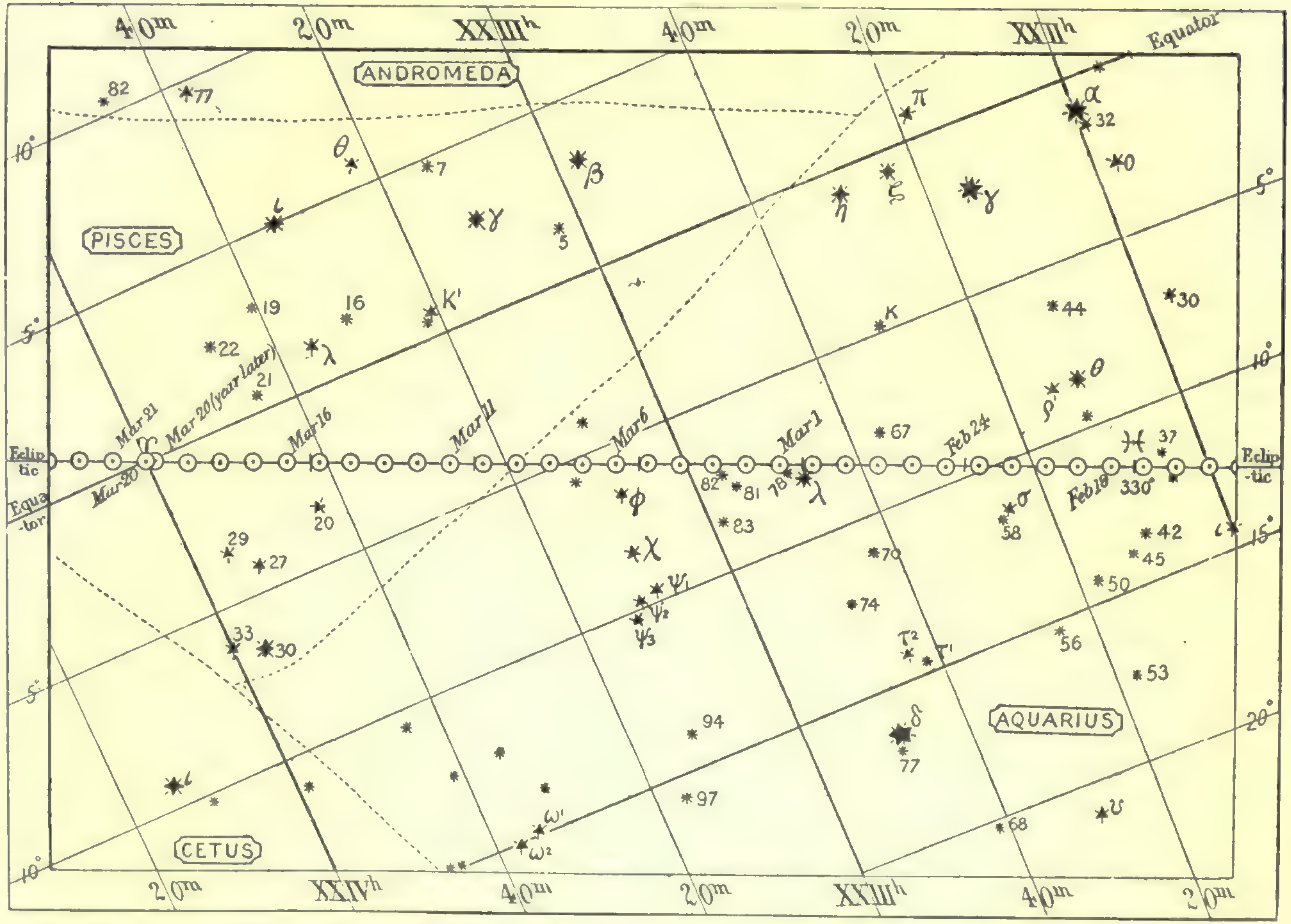
(283.) From about December 21 to January 20. The first sign after the Winter Solstice. The real sun gains on the mean sun because crossing the hour-circles squarely where they are nearer than at equator; also, because, the Earth passing perihelion on January 30, the sun's apparent motion is greatest in this sign. Hence, since this eastwardly gain causes the real sun to come later to the meridian than the mean sun, the clock gains on the sun rapidly in this sign. (They had been together about December 24–25.)

Fig. 87.—THE SUN'S COURSE THROUGH THE SIGN AQUARIUS.



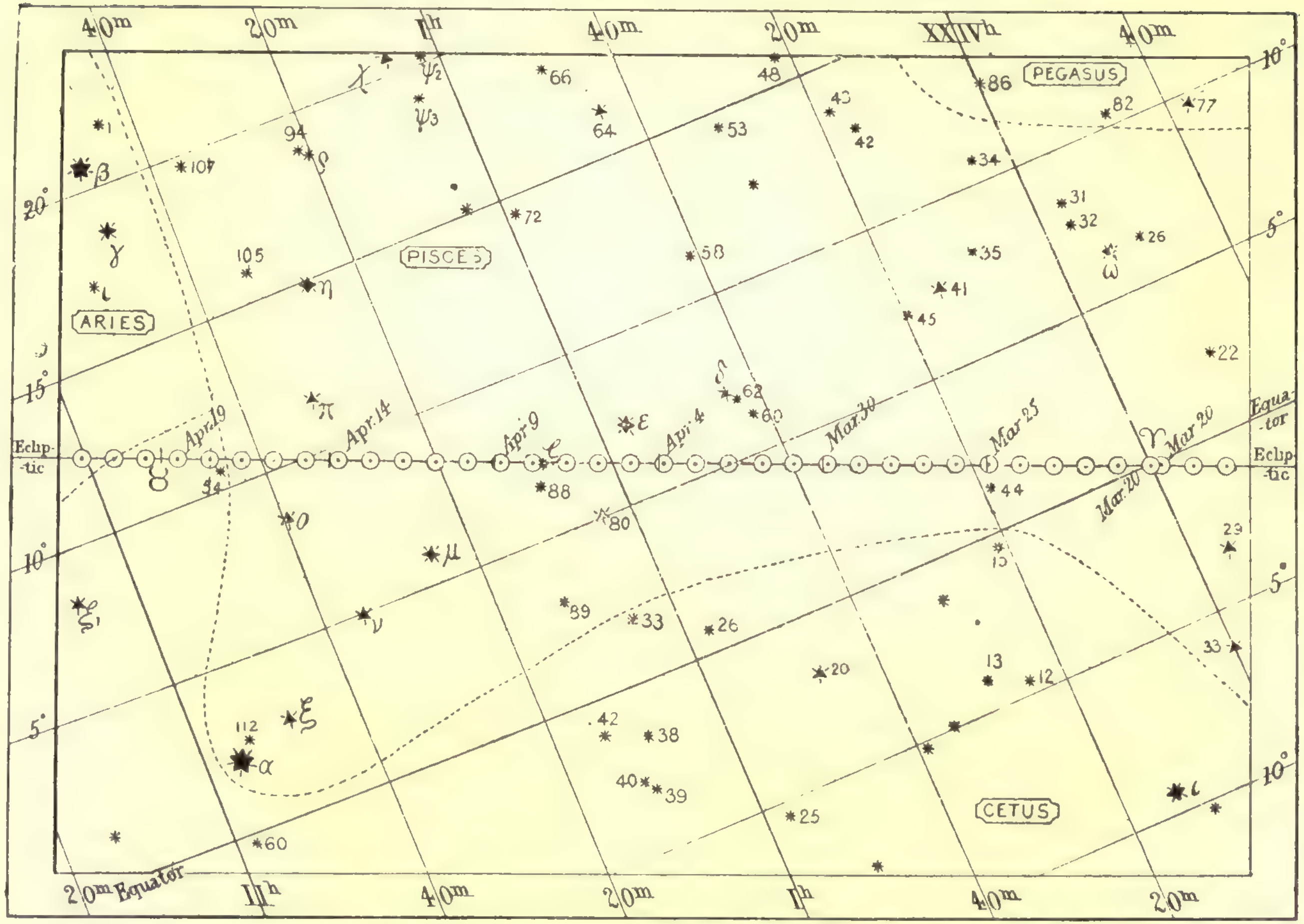
(284.) From about January 20 to about February 18. The second sign after the Winter Solstice. The real sun still gains on the mean sun in this sign; but not (on the whole) because of the position of his path, which in the middle of the sign is such that he loses as much by the slanting of his path across the hour-circles as he gains by their being nearer than at equator; he moves, however, with more than his mean velocity. Thus, the real sun passing still eastward from the mean sun, the clock still gains on the sun in the beginning of this sign, attaining its greatest gain (about 14 m. 27s.) on or about February 11. After that the sun begins to gain on the clock.

FIG. 88.—THE SUN'S COURSE THROUGH THE SIGN PISCES.



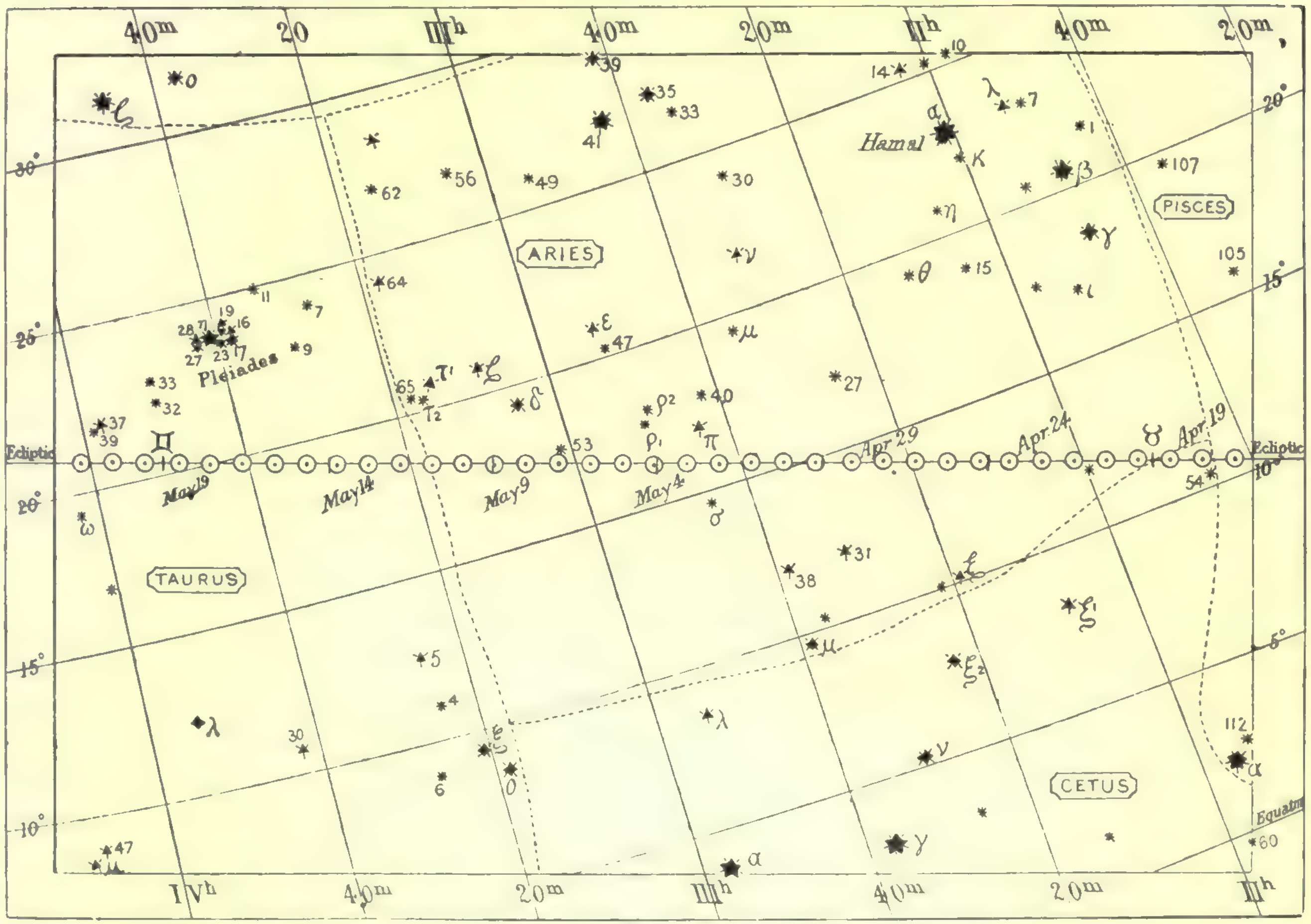
(285.) From about February 18 to March 20 21. Sign before the Vernal Equinox. The real sun loses on the mean sun, because he is travelling aslant athwart the hour-circles, which attain near ♈ their full great-circle distance apart. The sun still moves with more than his mean velocity, but is nearing his mean distance when at ♈; and his gain from this cause does not balance his loss from the other. Thus, the real sun beginning to return westwards towards the mean sun, the clock, though still ahead of the sun, loses part of the gain it had attained.

FIG 89.—THE SUN'S COURSE THROUGH THE SIGN ARIES.



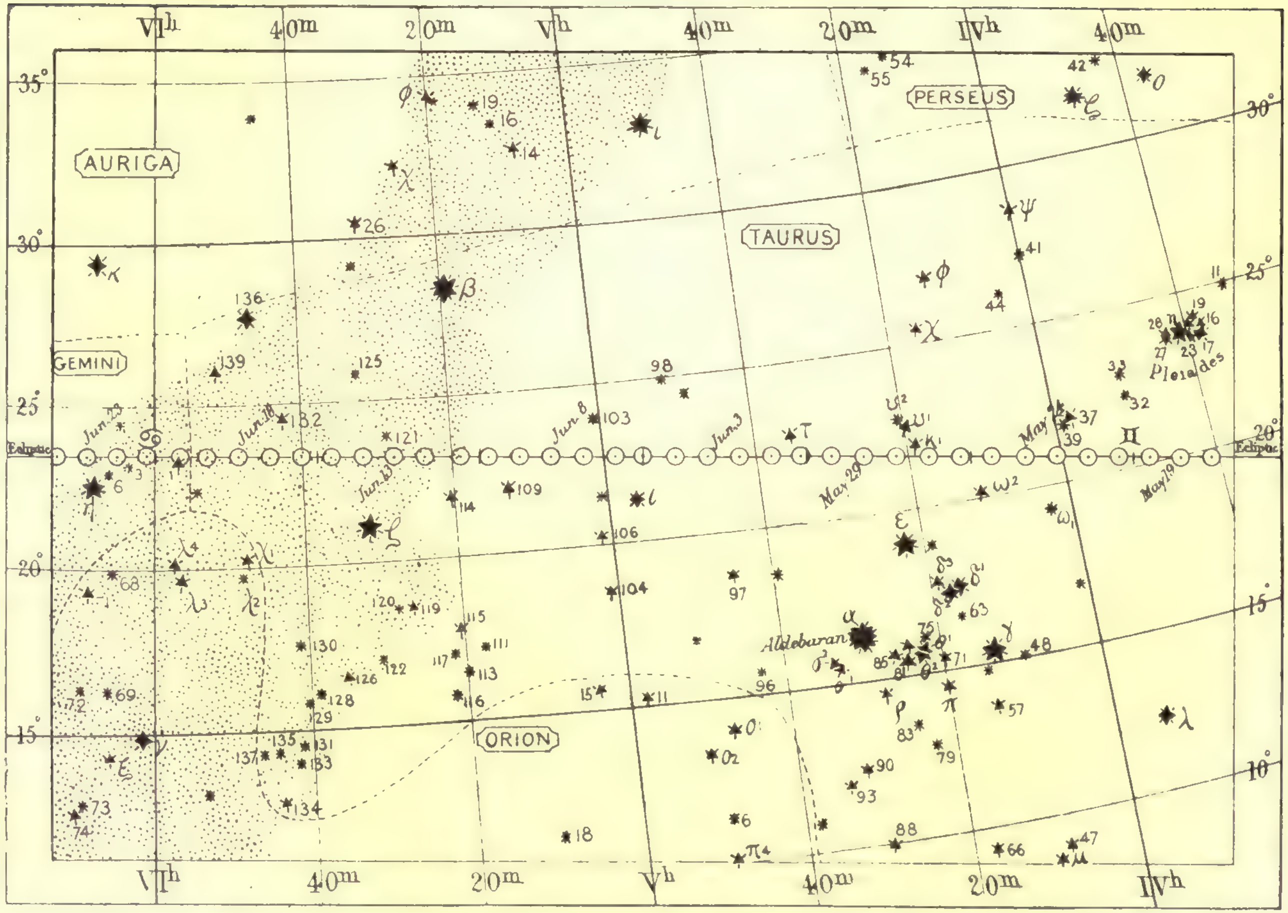
(286.) From March 20–21 to April 19–20. The first sign after the Vernal Equinox. The hour-circle through the point ♈ (always shifting slowly backwards or westwards) marks the beginning of right ascension. The real sun comes back to the mean sun in this sign. He is still moving slantingly athwart the hour-circles where they are farthest apart, and as he passes his mean distance April 1–2, moves on the whole with less than his mean velocity. Thus the clock loses the rest of its gain on the sun, and about April 15–16 the real sun on the ecliptic and the mean sun on the equator are on the same hour-circle, or clock and sun are together.

Fig. 90.—THE SUN'S COURSE THROUGH THE SIGN TAURUS.



(287.) From April 19-20 to May 20-21. The second sign after the Vernal Equinox. The real sun falls further behind the mean sun in this sign. His motion across the hour-circles has on the whole its mean value in this month, being aslant, but where the hour-circles are nearer than on the equator; but the sun, having passed his mean distance, is here moving with less than his mean velocity. Thus the real sun, passing westwards, comes to the meridian before the mean sun, or the clock falls behind the sun.

FIG. 91.—THE SUN'S COURSE THROUGH THE SIGN GEMINI.



(288.) From May 20 to 21 to about June 21. The sign preceding the Summer Solstice. The real sun, though now moving with less than his mean motion (the Earth passing aphelion on July 1–2), gains on the mean sun in this sign, because the hour-circles which he crosses nearly squarely are here nearer than on the equator. Thus, the real sun returning eastwards (with respect to the uniformly moving hour-circle through the mean sun), clock and sun are together again about June 14–15.

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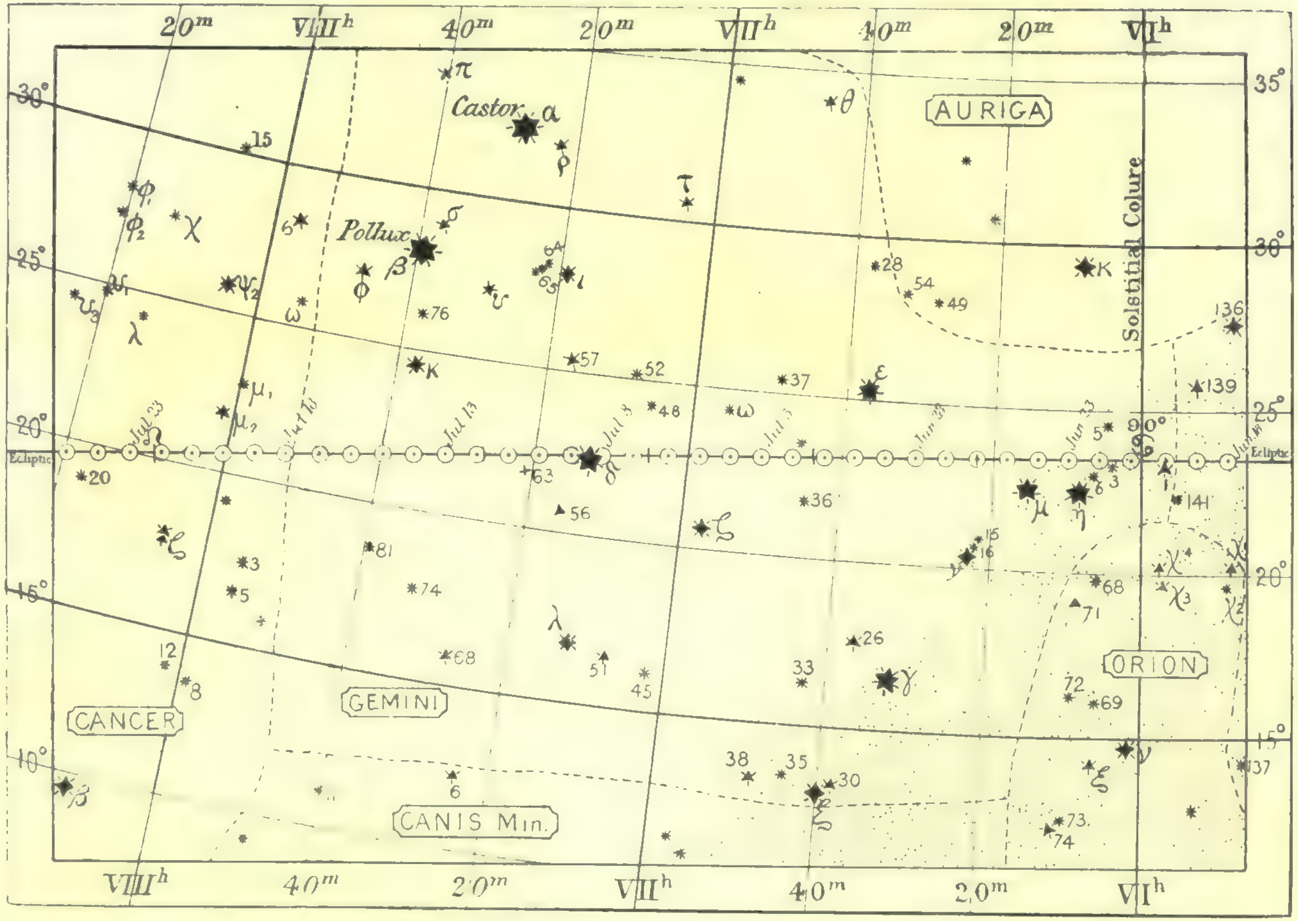
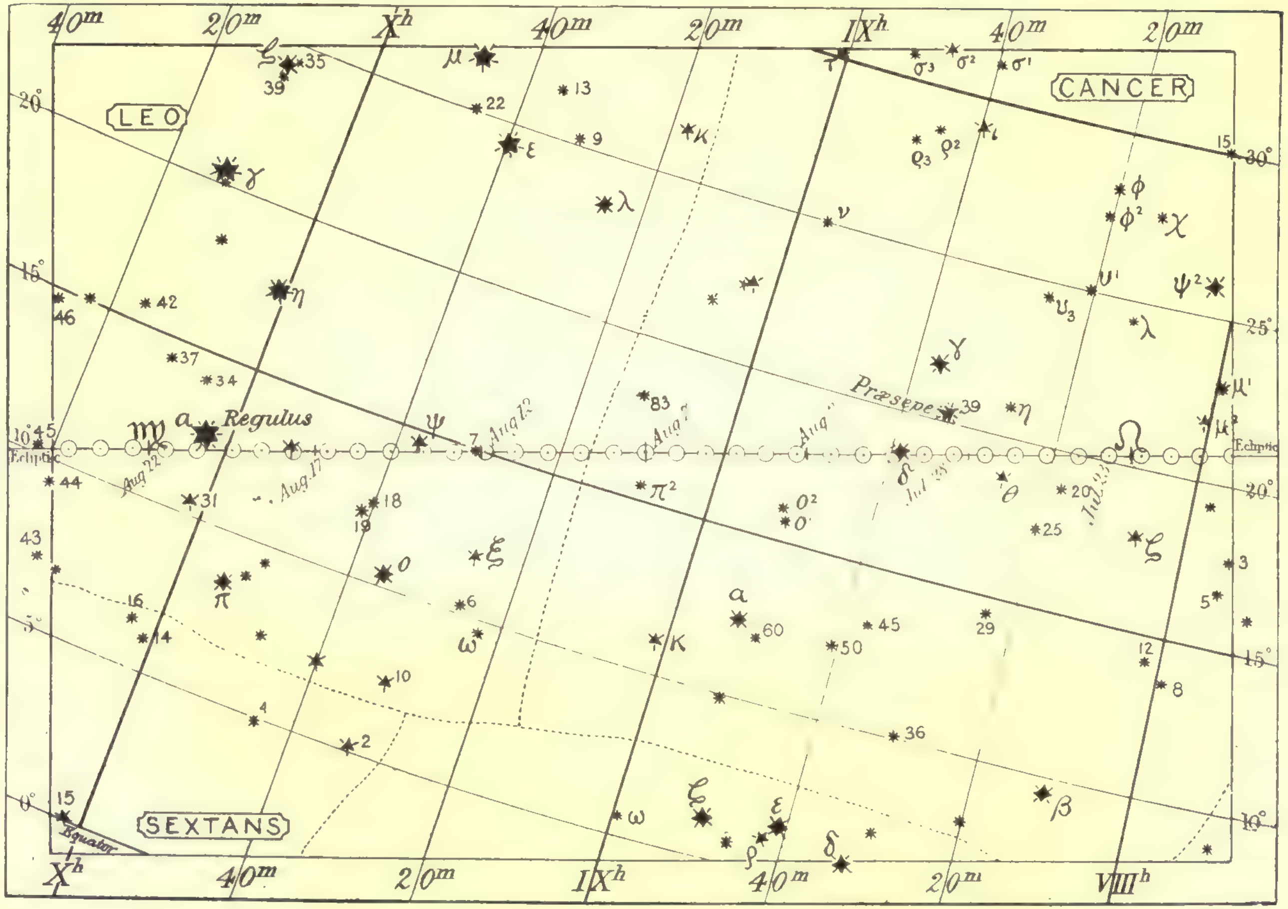
FIG. 92.—THE SUN'S COURSE THROUGH THE SIGN CANCER.



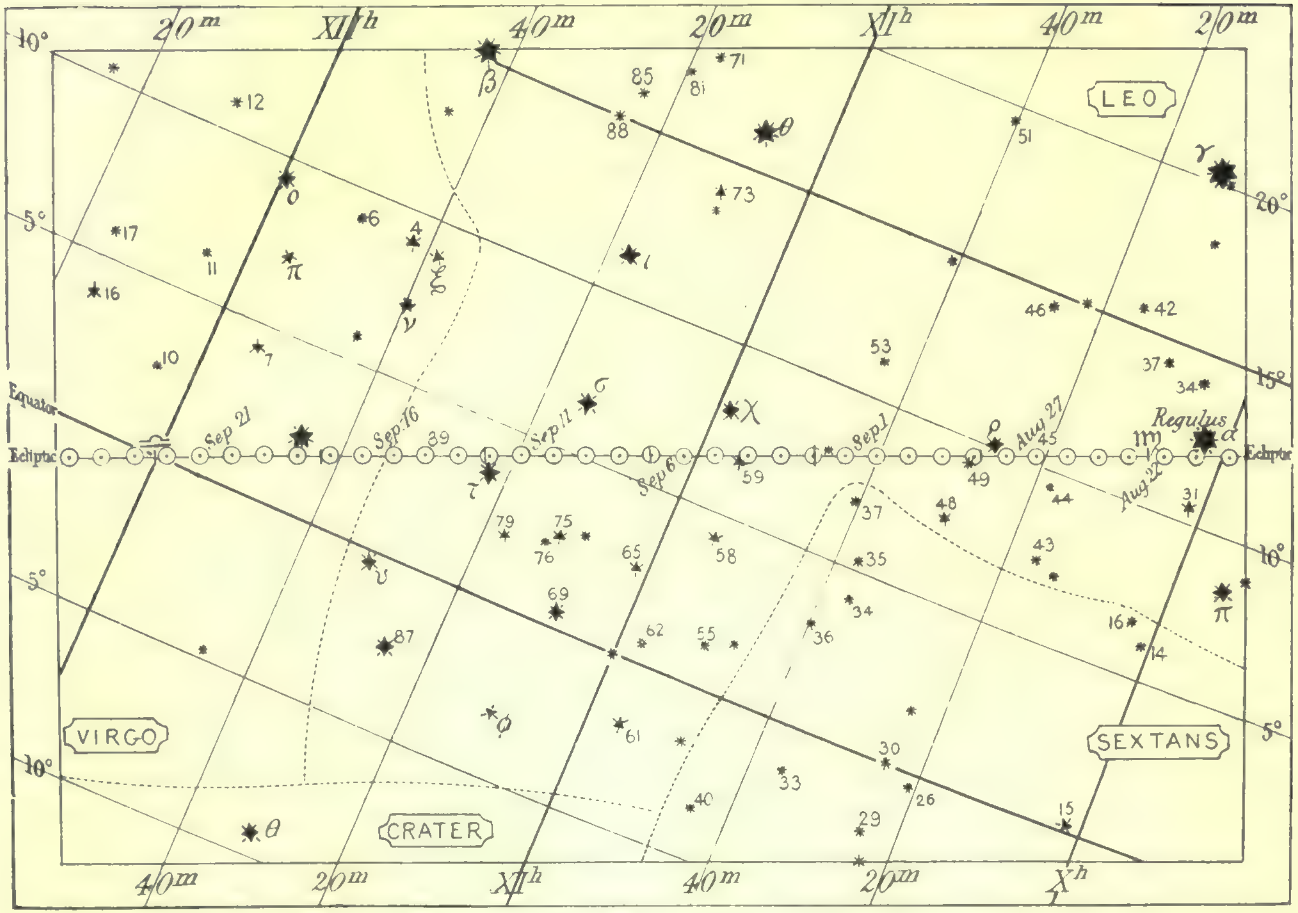
(289.) From about June 21 to about July 22. The first sign after the Summer Solstice. The real sun, passing apogee on July 1–2, loses on this account with respect to the mean sun, but gains more on account of the nearness of the hour-circles, over which he passes almost squarely. Thus the real sun is still passing eastwards (with respect to the uniformly moving hour-circles through the mean sun), and the clock gains on the sun, but very slowly towards the end of this sign.

FIG. 93.—THE SUN'S COURSE THROUGH THE SIGN LEO.



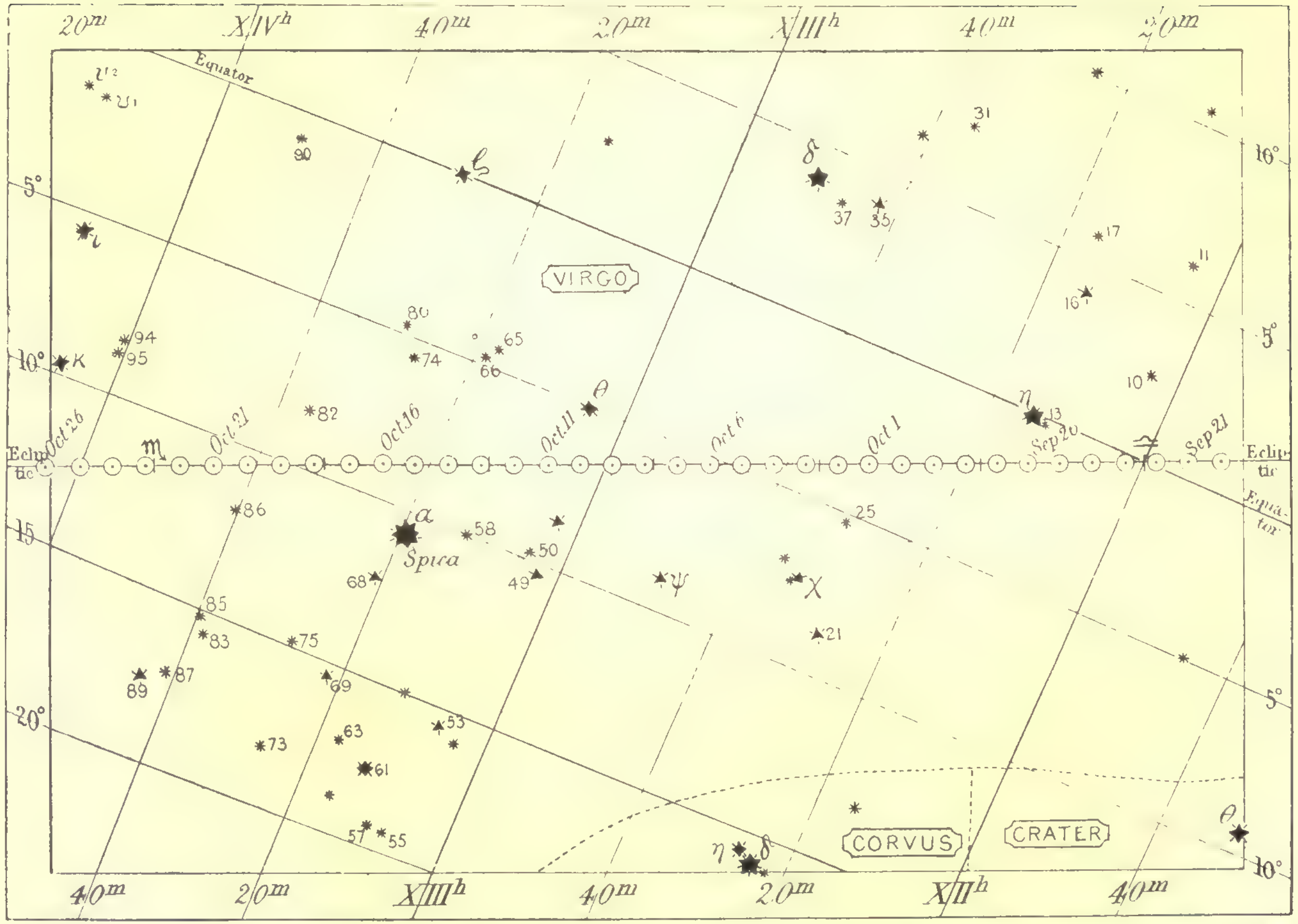
(290.) From about July 22 to August 22 23. The second sign after the Summer Solstice. The real sun gains in motion across the hour-circles in the first half of this sign, losing as much in the latter half; but, moving with less than his mean apparent velocity, loses on the whole, returning westwards towards the hour-circle through the mean sun. Thus in regard to the diurnal or eastwardly motion, the clock after attaining its maximum gain on July 26-27, loses in regard to the sun.

FIG. 94.—THE SUN'S COURSE THROUGH THE SIGN VIRGO.



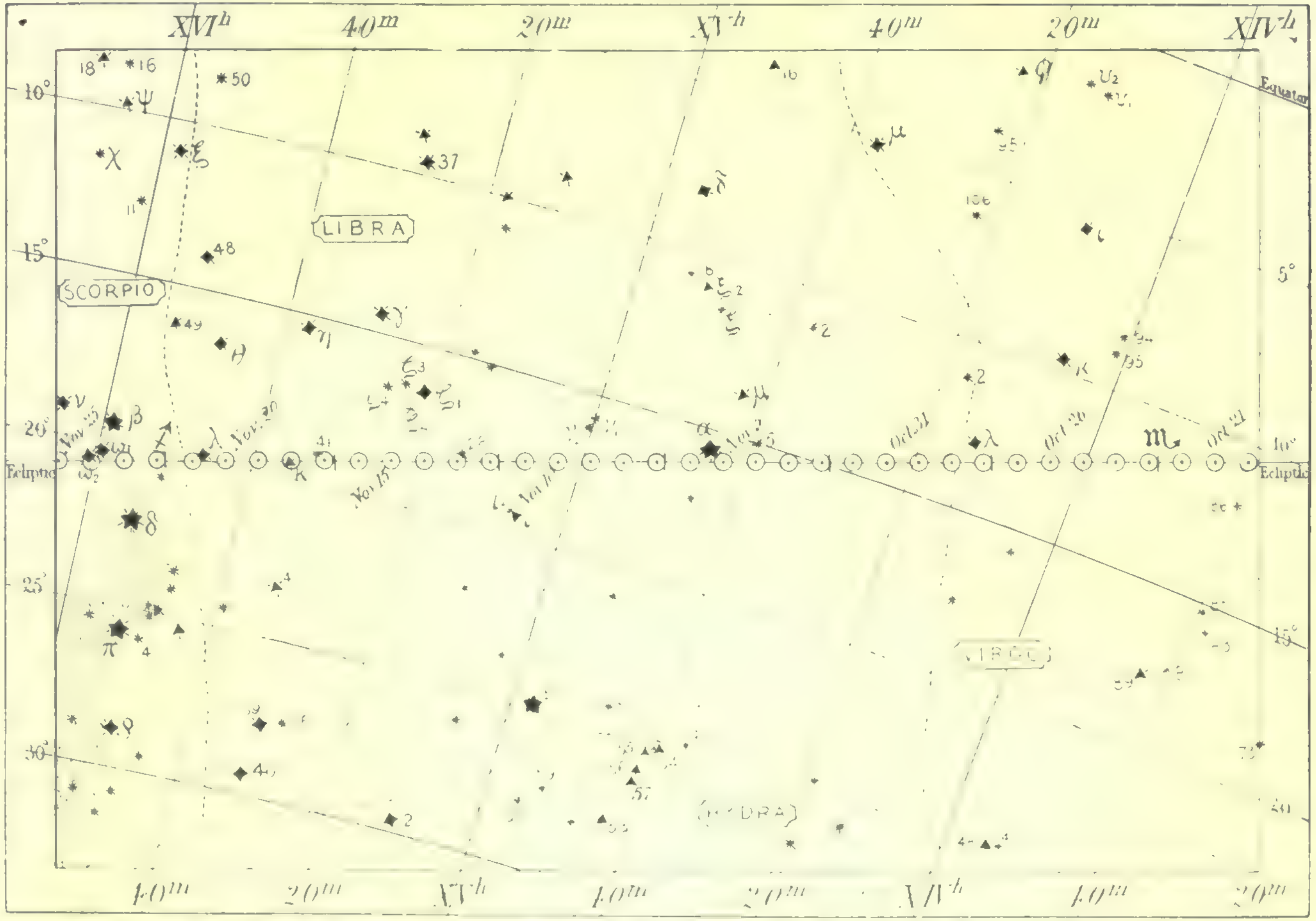
(291.) From August 22–23 to September 22–23. The sign next before the Autumnal Equinox. The real sun, still moving with less than his mean velocity, and now losing on account of his slant motion across the hour-circles where farthest apart, passes westwards with respect to the uniformly moving hour-circle through the mean sun. Thus the clock falls behind the sun. They are together about September 1, after which sun time is before clock time.

Fig. 95.—THE SUN'S COURSE THROUGH THE SIGN LIBRA.



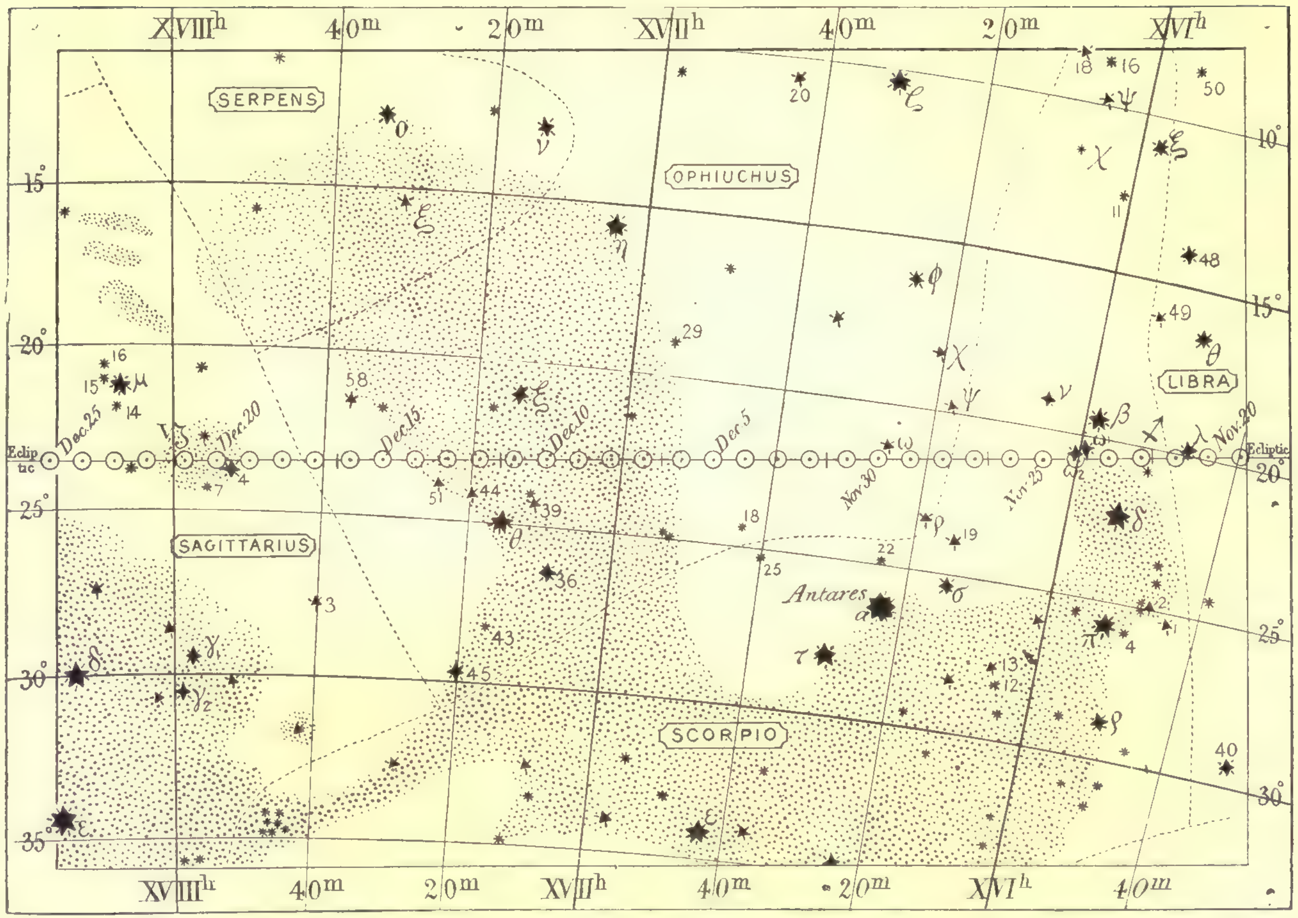
(292.) From September 22–23 to October 23. The sign following the Autumnal Equinox. The real sun is still losing on account of his slant motion across the hour-circles where farthest apart; and passing his mean distance about October 3, moves with little more than his mean apparent motion. Thus the sun still passes westwards of the hour-circle through the mean sun, and the clock falls farther behind the sun.

Fig. 96.—THE SUN'S COURSE THROUGH THE SIGN SCORPIO.



(293.) From about October 23 to about November 22. The second sign after the Autumnal Equinox. The real sun loses in first half and gains (equally) in second half, on account of the position of his path with respect to the hour-circles; but the sun gains, on the whole, because now past his mean distance and nearing perigee. Thus he draws eastwards towards the hour circle through the mean sun, after attaining his greatest westwardly recession about November 3. The clock then, though behind the sun throughout the sign, gains after November 3, and on the whole during the month.

FIG. 97.—THE SUN'S COURSE THROUGH THE SIGN AQUARIUS.



(294.) From about November 22 to about December 21. The sign preceding the Winter Solstice. The real sun is gaining on the mean sun both on account of his moving squarely across the hour-circles where nearest together, and because he is nearing perigee and moving with more than his mean velocity. Thus in this sign nearly the whole of the sun's westerly departure from the hour-circle through the mean sun is worked off. The clock gains on the sun, and sun and clock are nearly together (clock still behind) when the sun reaches the solstice.

NOTES ON THE TWELVE ZODIACAL MAPS.—Figs. 86 97.

Fig. 86.—The *sign* Capricornus falls now in the *constellation* Sagittarius, having travelled about one sign backwards since the days of the earlier Greek astronomers. At the building of the Great Pyramid, about 3350 B.C., the arc ♑ to in this map was traversed by the sun between October 11 and November 10, the Winter Solstice being at that time in Aquarius (between ϕ and λ).

Fig. 87.—The *sign* Aquarius falls now in the *constellation* Capricornus, having travelled one sign backwards in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the arc ♒ to ♓ in this map was traversed by the sun between November 10 and December 10, the sun when at ♓ in the map being still nearly half a sign from the Winter Solstice.

Fig. 88.—The *sign* Pisces falls now in the *constellation* Aquarius, having travelled one sign backwards in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the arc ♓ to ♈ in this map was traversed by the sun between December 10 and January 10, the sun passing the Winter Solstice at about the place of the sun-disc for March 3 in the map, near to the star 82.

Fig. 89.—The *sign* Aries falls now in the *constellation* Pisces, having travelled one sign backwards in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the arc ♈ to ♉ in this map was traversed by the sun between January 10 and February 9, the sun being still among the watery constellations in this winter part of his career.

Fig. 90.—The *sign* Taurus falls now in the *constellation* Aries, having travelled one sign backwards in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the arc ♉ to ♊ in this map was traversed by the sun between February 9 and March 10 (lambing time—whence probably the idea that the stars group like a ram), the sun not having reached the place of his passover by nearly half a degree.

Fig. 91.—The *sign* Gemini falls now in the *constellation* Taurus, having retreated one sign since 270 B.C. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♊ to ♋, between March 10 and April 10, crossing the equator (the passover of the sun-god) at about his place for June 3 in the map. This corresponds with Virgil's description of the opening of spring—'*candidus aperit cum cornibus annum Taurus.*'

Fig. 92.—The *sign* Cancer falls now in the *constellation* Gemini, having travelled backwards one sign in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♋ to ♌, between April 10 and May 11, the sun on or about our May Day (then as now) passing to 15 degrees north of the equator, or ascending into the region of his midsummer glory.

Fig. 93.—The *sign* Leo falls now in the *constellation* Cancer, having retreated about one sign since the days of the early Greek astronomers, when Cancer was the summer constellation. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♌ to ♍, between May 11 and June 10, drawing near his solstitial glory.

Fig. 94.—The *sign* Virgo falls now in the *constellation* Leo, having retreated one sign in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♍ to ♎, between June 10 and July 11, passing the Midsummer Solstice at about the sun's place for September 6 in the map, near χ Leonis.

Fig. 95.—The *sign* Libra falls now in the *constellation* Virgo, having retreated one sign in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♎ to ♏, between July 11 and August 10, now declining from his midsummer glory, but still high in the mid-heavens at noon.

Fig. 96.—The *sign* Scorpio falls now in the *constellation* Libra, having retreated one sign in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♏ to ♐, between August 10 and September 10, drawing towards its crossing-place at the Autumnal Equinox.

Fig. 97.—The *sign* Aquarius falls now in the *constellation* Scorpio, having retreated one sign in the last 2155 years. At the building of the Great Pyramid, about 3350 B.C., the sun traversed this arc, ♐ to ♑, between September 10 and October 11, making his descending passover at about the sun's place for December 5 in the map.

(295.) The ancient astronomers in determining the sun's path round the celestial sphere, and timing his motion from equinox to solstice, and from solstice to equinox, recognised the variable velocity with which the sun moves. Probably not even the best of the ancient Egyptian and Babylonian astronomers had done this with the degree of accuracy indicated in the twelve maps, fig. 86 to 97. In fact, it is probable that the gradual nature of the increase and decrease of the sun's velocity was inferred rather than actually observed, the difference between the intervals from solstice to equinox and from equinox to solstice being what was actually noted. We have no record of the results those ancient astronomers actually obtained, and must be content to consider the work of Hipparchus at a much later age, because it is the most ancient of which the details have reached us.

(296.) Hipparchus obtained good estimates of the epoch of the summer solstice: and he had estimated with even greater exactness the epochs of the equinoxes: but he does not seem to have determined accurately the time of the winter solstice. One can readily understand how he might have been prevented from making satisfactory midwinter observations, even in the clear climate of Rhodes and of Alexandria. At any rate we only learn his estimate of the duration of the intervals from spring to midsummer, from midsummer to autumn, and from autumn to spring again. Yet from these he was able to deduce the law of the sun's motion in such a way that he could predict the position of the sun far more accurately than his predecessors among the Greek astronomers, besides in other ways assigning the character of the sun's apparent orbit round the Earth. His way of treating the problem is worth studying.

(297.) Around C as a centre (fig. 98) strike the circle *v m f w*, and draw the two diameters, *w* C *m*, *v* C *f*, dividing the circumference into the four quadrants, *v m*, *m f*, *f w*, and *w v*. Now if the sun moved uniformly in a circle around the Earth as a centre, the points *v*, *m*, *f*, and *w* might be taken as representing the sun's positions at the vernal equinox, at midsummer, at the autumn equinox, and at the winter solstice respectively. Or we might equally well regard these as the positions of the Earth at those epochs respectively, and C as the place of the sun. A year lasting 365¼ days, nearly enough for our present purpose, the motion of the Earth from *v* to *m* would require 91 days 7½ hours; and so would the motion from *m* to *f*, from *f* to *w*, and from *w* to *v*.

(298.) If Hipparchus had found these intervals thus equal, he would have taken C as the true centre of motion. But instead of this, he found that the sun occupied about 94½ days in passing from the vernal equinox to the summer solstice, about 92½ days in passing from the summer solstice to the autumnal equinox, and only 178¼ in passing from the autumnal to the vernal equinox. It was necessary, then, to find a centre of motion, as S, so situated that when the cross-lines V S F, W S M, were drawn parallel to the diameters *v f* and *w m*, uniform motion round the circle

v M F W in 365¼ days, would give 94½ days for the arc V M, 92½ days for M F, and 178¼ days for F W V. Clearly this new centre must be somewhere within the angle *f* C *w*, as S is, for then the arc *v m* will be increased on both sides by *v* V and *m* M, so as to be the largest of the four divisions. Again S must clearly lie nearer to the diameter *w m* than to the diameter *v f*, for the division M F (from midsummer to autumn) has to be greater than *m f*; since it requires 92½ days, instead of a quarter of a year (or 91 days 7½ hours) to pass through M F by the uniform motion which Hipparchus had taken as the basis of his reasoning. As the arc *f* F is added, and the arc M *m* is taken away, we want *f* F to be greater than *m* M, so that M F may be in due degree greater than *m f*.

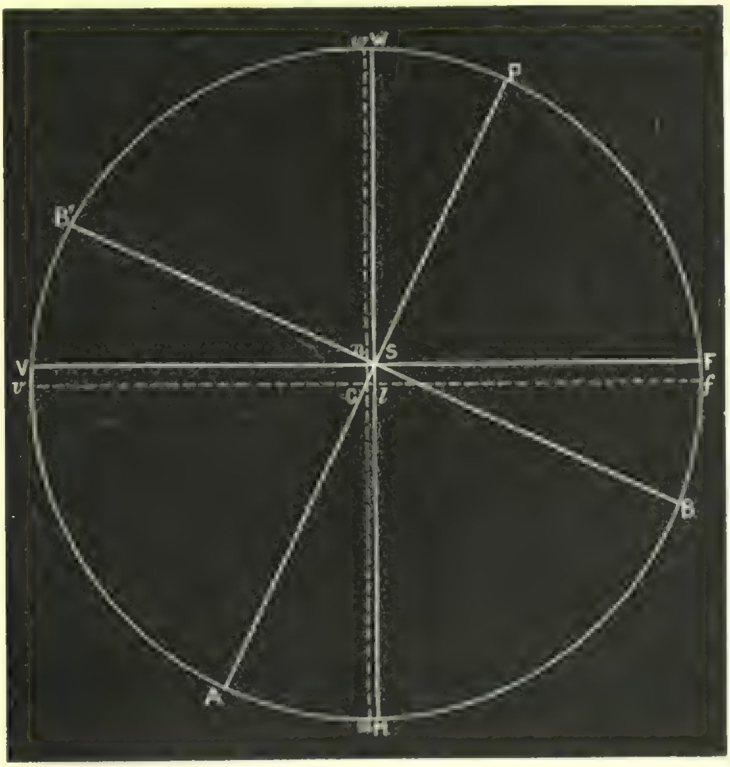


Fig. 98.—Showing how Hipparchus determined the position of the Sun's perigee and apogee.

(299.) And clearly we have in these relations the means of exactly determining the position of the point S. We require that V *v* and *m* M together shall be equal to the arc traversed in 94½ days less 91 days 7½ hours, or in 3 days 4½ hours; while we require also that the excess of *f* F or V *v* over M *m* shall be equal to the arc traversed in 92½ days less 91 days 7½ hours, or in 1 day 1½ hours. (The calculation here is only rough.) From this it follows that *v* V is equal to the arc traversed in 2 days 4½ hours, and M *m* is equal to the arc traversed in 1 day; for the sum and difference of these two intervals are those just mentioned, as required. Now knowing that the whole circumference is traversed uniformly (according to the initial supposition) in 365¼ days, we know exactly what portion is traversed in 2 days 4½ hours and in 1 day. We can then at once mark off the proper arcs V *v* and *m* M (*v* V is an arc of about 2⅙°, *m* M an arc of rather less than 1°); and then M W and

V F drawn parallel to $w\,m$ and $r\,f$, intersect in S the required place of the sun or of the Earth, according as we supposed the Earth to move round the sun, or the sun round the Earth.

(300.) It will be clear that as S lies very near to C, C n is nearly equal to V r, and S n nearly equal to m M. We have in fact C n and S n bearing to each other the same ratio as 1 day bears to 2 days $4\frac{1}{2}$ hours, or as 16 to 35. This determines the proper position of the line P S C A, P being the point of nearest approach to S, or the perihelion, and A the remotest point, or aphelion. Or, as Hipparchus viewed the matter, S was the place of the Earth, P the sun's place when in perigee, and A the sun's place when in apogee.

(301.) Also V r and m M being taken of the true lengths to represent the uniform motion in 2 days $4\frac{1}{2}$ hours and in 1 day respectively, we find the length of C S to be about one twenty-fourth part of the radius C P. This was deduced by Hipparchus. He made C S or the eccentricity of the orbit of the sun about the Earth, or of the Earth about the sun, one twenty-fourth of the radius; and he made A fall about 24° from M, the place of the midsummer solstice. Neither result was quite correct, even according to his hypothesis of uniform motion; but the approximation to the truth was very close for his time. And the position of the sun calculated from the tables based on this result agreed very closely indeed—so far as observations then possible were concerned—with the sun's observed position. Eclipses, also, could be correctly calculated from these tables for a long time in advance.

(302.) The more accurately the sun had been observed the more nearly would the eccentricity deduced have approached in value the true eccentricity doubled. The error arises from the assumption of uniform motion, which amounts to throwing the whole correction on the eccentricity, whereas half of it depends on the variation in the real rate of the Earth's motion, as will appear later on.

(303.) If Hipparchus had determined the true time of the sun's reaching the summer solstice, as well as the true epochs of the vernal and autumnal equinox, he would have made S C (fig. 98) about a thirtieth instead of about a twenty-fourth part of the radius C P. Correcting this error, which was unavoidable with such methods of observation as were alone available in his day, the eccentricity deduced from his hypotheses is twice the true eccentricity.

(304.) The error of a determination of the eccentricity of the sun's apparent orbit based on uniform motion would have been corrected if the sun's varying apparent diameter could have been accurately determined. But it does not appear that accurate measurements were made. The work would seem to have been within the power of the ancient astronomers, though their means for precise measurement were altogether inferior to those which modern astronomers possess. It would naturally occur to them to measure carefully the apparent size of the sun, as seen from different stations, at different hours and in different months. Astronomers quickly learnt that no change of position on the Earth affects the sun's apparent size, whence they would infer that the sun must lie at a distance so great that the dimensions of the Earth are by comparison very small. We shall see presently that the old

astronomers formed a very good idea of the moon's distance, and had independent evidence that the sun is much farther away than the moon. But there must have been something most impressive to the more thoughtful men of old times in the recognition of the way in which the sun's disc remained without change, no matter how men might journey over the Earth, which seemed to them so large.

(305.) The same lesson was taught by the unchanging apparent size of the sun as he passed through his daily course from the eastern to the western horizon. The astronomers of Egypt and of Babylon knew well what this signified. At the Great Pyramid, for instance, the midday midsummer sun is nearly overhead, and is therefore about one radius of the Earth nearer at that time than when he is on the horizon. But the enlargement of the sun when high above the horizon is too small to be measurable even with the best of our modern methods. The absence of any apparent enlargement was of course a most significant circumstance for the astronomers of old times. They saw that an approach of the sun by nearly 4,000 miles during the time of his motion from the horizon to his midday culmination produced no apparent increase in his disc. How far off, they reasoned, must an orb be when so great a difference of distance made no recognisable change in its aspect! Be it observed that the evidence thus obtained did not depend, in the slightest degree, on the question whether the sun moved round the Earth, or the Earth round the sun. It was absolutely certain, even in the days of the pyramid builders, probably 3,400 years before the Christian era, that a distance of 4,000 miles, small though measurable compared with the moon's distance, is a mere nothing compared with the distance of the sun.

(306.) But, observing the sun as the year passed onwards, the old observers (if they had applied to the task a tithe of the perceptive ability and observing skill which they certainly possessed, as we learn from the details of the Great Pyramid) might have recognised a decided variation in the apparent size of the sun's disc. For the change of size, though not obvious to ordinary observation, is readily observable, and even measurable, by means well within the power of the ancient astronomers. In those days the sun's diameter varied during the year, as now, by about one sixtieth of its mean value. The actual difference between the apparent diameter of the sun when at his nearest—which at the time of the building of the Great Pyramid was about the autumnal equinox [1]—and at his farthest, corresponded then, as now,

[1] There is a singular mistake on this point in Herschel's *Outlines of Astronomy*, which I note only because otherwise readers might be confused by the difference of statement, and not know where the truth really lies. He remarks that about the year 4000 B.C. the perihelion was in longitude 0°, or corresponded with the vernal equinox. As a matter of fact, the perihelion was in longitude 0° at about that time; but that position corresponded with the autumnal, not with

to the difference between 31 and 30, a ratio easily to be recognised. Supposing, for example, that the sun's light were received through a small circular hole, a quarter of an inch in diameter, into a darkened chamber, so as to fall on a screen 9 yards from the hole ; then the diameter of the solar image formed on the screen would be about three inches—not sharply defined, however, but with a penumbra about a quarter of an inch wide. This image would vary in width during the year from $2\frac{19}{20}$ inches to $3\frac{1}{20}$ inches, or by one-tenth of an inch ; and careful measurement in different parts of the year could not fail to show this change, though it would have been doubtless difficult to measure it.

(307.) Fig. 99 indicates the actual amount of change which the sun's disc undergoes during the year. P *p* P′ *p′* represents the sun's disc when he is in or near perihelion ; A *a* A′ *a′*, the disc when he is in or near aphelion. A circle midway between these would represent the sun's disc when he is at his mean distance. It is worth noting, however, that the relative dimensions of the sun's disc are indicated exactly in any diagram showing the eccentricity of the Earth's orbit. Thus in the picture of the Earth's orbit, illustrating the chapter on the Earth, the distances S A, C A, and S A′, which show the least, mean, and greatest distances of the Earth from the sun, show also, with equal correctness, the relative apparent diameters of the sun when at his greatest, mean, and least distances.[1]

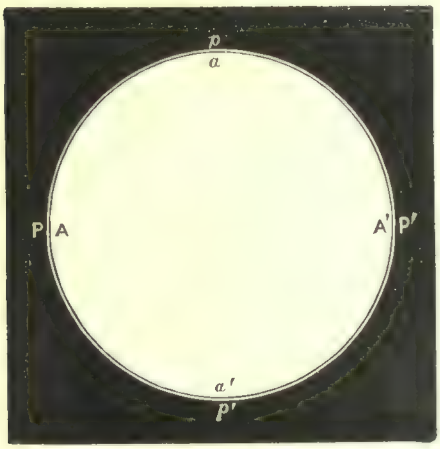


FIG. 99.—Sun's annual apparent change of diameter.

(308.) The ancient observers appear to have determined with great accuracy the time of the sun's circuiting the star-sphere ; the true year. They also recognised the fact that the year of seasons is somewhat shorter ; the intersection of the equator and the ecliptic moving steadily to meet the advancing sun, but very slowly—taking, in fact, 25,868 years to complete a circuit. On the other hand, the place of the perigee moves very slowly the other way (i.e. advancing), so that the interval from perigee to perigee is rather longer than a true year, or the time of one circuit of the star-sphere by the sun. The three years are as follows :—

1. Time from perigee to perigee (*anomalistic year*) . .	365 days	6 hours	13 min.	49 sec.
2. Time of circuiting the star-sphere (*sidereal year*) . .	365 „	6 „	9 „	10 „
3. Time of circuiting the signs (*tropical year*) . .	365 „	5 „	48 „	47 „

(309.) As I have said, the study of the moon's monthly movements on the celestial sphere must long have preceded the study of the sun's annual

the vernal, equinox. The sun's geocentric longitude is 0° at the vernal equinox, but the Earth's heliocentric longitude is then 180°. The perihelion is now advancing to meet the retreating vernal equinox at the rate of 61″ yearly.

[1] The change in the sun's apparent size is monstrously exaggerated in many books on popular astronomy. In Guillemin's *Heavens*, for example, it is increased fully tenfold.

movements ; for the simple reason that the moon can be watched moving among the stars, but the sun's motion can only be inferred from the change in the position of the constellations visible at night.

(310.) Doubtless however, the moon's changes of aspect attracted men's attention long before they noticed how she travels among the stars. They probably supposed these phases specially intended to help men in measuring time. In fact, as the sun was the primeval measurer of hours—rough but sufficient for the rough wants of early man—so the moon was the primeval measurer of days. (The sun's yearly count of days could not be recognised or measured till much later.) As a measurer of days the moon must have seemed imperfect and unsatisfactory ; yet probably men supposed that this was only because they had not caught the exact idea of their lunar time-piece. They must have judged that, whether the moon was set in the heavens to serve as a measurer of time, or whether she herself moved round the star-sphere and varied in her aspect of set purpose to measure time for man, she must measure the days effectively and well. Unless, perhaps, they supposed that, though originally a good time-measurer, she had become a bad one because of some offence of which man had been guilty. That such an idea would be natural to men imagining every detail of the celestial mechanism planned with sole reference to the wants of man, is shown by Milton's acceptance (or origination) of just such an idea ('Paradise Lost,' book x.) :—

> Some say He bid his angels turn askance
> The poles of earth twice ten degrees and more,
> From the sun's axle . . .
> . . . else had the spring
> Perpetual smiled on earth with verdant flow'rs
> Equal in days and nights.

If the sun might thus be supposed to be turned from his proper course to punish man, the moon might well be supposed to have been thrown out of gear, so as no longer to measure time well, though the only measurer man had.[1]

(311.) Be this as it may, men found the moon going through certain changes of aspect, from the fine crescent with which she was first seen in the west, after being two or three days out of sight, onwards to half-full, three-quarters, full, declining through three-fourths to half, and so to the crescent form again.

(312.) At first these phases would simply be noted as signs of the

[1] Even so late as the Council of Nice theologians and hierophants strove to make the moon keep perfect time in her sidereal, nodical, and anomalistic movements, by insisting that the relations presented in the Metonic Cycle must be exact instead of being, as they really are, approximate only.

progress of the month. Men would see in the easily recognised half-moon, before and after 'full,' a sign in the heavens that one-fourth of the month in one case, and three-fourths in the other, had passed; and though the true time of full moon cannot be so easily determined by direct observation, while the moon is altogether lost to sight when 'new,' they would quickly learn to determine the time of full moon and new moon with sufficient accuracy from the times of half-moon.

(313.) For the rough measurement of time in hiring labour this would do very well. The lunar month would seem conveniently divisible into four parts, each seven days in length, the time when the moon could not be seen making up the balance of about a day and a half by which a lunar month exceeds 28 days. This would involve an alternate rest of one day, and of two days at the time of the new moon in alternate months, by which the average length of lunar months would be made $29\frac{1}{2}$ days. Or probably, instead of this arrangement, those who attended to the measurement of time (which was early regarded as a religious duty) set each new moon as a special festival and day of rest, and had another rest after every week of weeks; that is, made the 50th day from each alternate new moon (a day falling near the third quarter of the second month) a day of rest and readjustment of their lunar time-measurement. There are traces of an arrangement of this sort in the Babylonian lunar calendar. The 7th, 14th, 21st, and 28th days of each month were days of rest; but the 19th also was a day of rest. Now this seems naturally explicable thus:—Suppose a lunar month just completed by the day dedicated to the new moon; then the 7th, 14th, 21st, and 28th days would be Sabbaths; the 29th would be a new moon festival; the 7th and 14th days of the next month would be the 36th and 43rd days of the bi-monthly period; and the 19th day would be the 50th day, or a day of jubilee, thrown in as a bi-monthly resting day, and bringing up the number of days in the two bi-monthly periods to 59, instead of 58 days, as it would have been if there had only been eight seven-day periods and two new-moon rests. An arrangement by which 59 days were assigned to two lunar months would be very near the truth for the primitive times we have been thus far dealing with. For, two lunar months last 59 days 1 hour and 28 minutes; so that an error of a full day would not accrue until more than sixteen bi-monthly periods, or about 2 years 7 months, had passed.

(314.) Later astronomers seem to have recognised the advantage of regarding the lunar month as approximating to 30 days, and dividing it into six parts, each five days in length. All such methods depended on the recognition of the lunar phases as a means of measuring time. To ordinary observation the half-moon phases are alone distinctly recognisable. But with

very little trouble observers can determine by measurement the time when the breadth of the illuminated part of the moon is either one-quarter or three-quarters of the full diameter; and these times correspond to the completion of one-sixth, one-third, two-thirds, and five-sixths of the moon's monthly circuit. Combined with the half-moons, these easily determined epochs of the lunar month divide the moon's circuit into twelve equal parts,[1] each very nearly equal to $2\frac{1}{2}$ days' advance with respect to the sun. This would have seemed very suitable and convenient to ancient astronomers. For they endeavoured (whenever they could) to secure the division of circles, cycles, and circuits generally, into twelve parts, as we see in the year of twelve months, the twelve signs of the zodiac, the division of the meridian of the Great Pyramid (in fact, in the selection of latitude 30° for this important observatory), and a number of other cases.

(315.) The interpretation of the moon's varying phases belonged of course to a much later time than the mere recognition of the phases. Still, observant men cannot long have failed to notice that when the moon is opposite the sun she shows a full face, as if fully lit up by his rays, whereas when nearly towards the same part of the sky as the sun she shows but a fine crescent of light, as if he were illuminating the farther side of her globe. It can scarcely be doubted that the more thoughtful not only recognised the moon as an opaque globe shining only by reflecting the sun's rays, but also formed a tolerably distinct idea of the relative proximity of the moon's path. For it would require but a slight acquaintance with geometry to show them that, unless the sun lay very much farther away than the moon, the times of exact apparent half-moon would not seem to divide the moon's monthly circuit into equal portions.

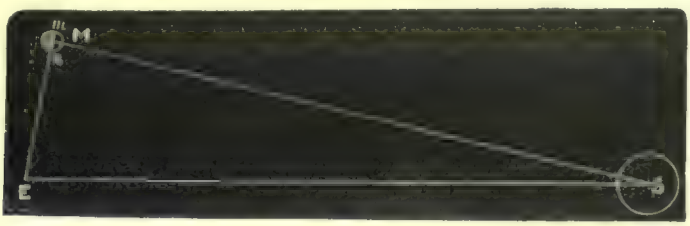


Fig. 100.—Illustrating the evidence given by the half-moon of the sun's greater distance, and therefore of his greater size.

(316.) This will be obvious from fig. 100, where S is supposed to be the sun, M the moon, and E the observer on Earth, at a time when the moon looks exactly half-full, and when therefore *m n*, the boundary of the illuminated face, is directed

[1] Or rather they indicate this division. The quarter-moons, half-moons, and three-quarter moons being the only phases readily measurable, we have the lunar month divided at the following stages: $\frac{1}{6}$, $\frac{1}{4}$, $\frac{1}{3}$ . . . $\frac{2}{3}$, $\frac{3}{4}$, $\frac{5}{6}$ that is, 2, 3, 4 . . 8, 9, and 10 *twelfths*.

exactly towards E. It is evident that S M E (letting M here mark the moon's centre) is a right angle, and therefore M E S is appreciably less than a right angle, unless we suppose the angle M S E very small indeed, which it can only be if S E is very much greater than M E. But as the moon is seen to be half-full when she has gone just one-fourth of the way round from the sun, that is, when the angle M E S is appreciably a right angle, it follows that the distance E S does thus exceed the distance M E. There are reasons for believing that Egyptian and Babylonian astronomers long before the days of Hipparchus and Ptolemy estimated the moon's distance at thirty diameters of the Earth. Hence they must have judged the sun to lie at an enormous distance from the Earth, and to exceed the Earth enormously in volume.

(317.) That the moon travels on an orbit inside the apparent orbit of the sun must also have been made clear to the ancient observers by the occurrence of eclipses of the sun and moon. For they observed that these occur when the moon passes between the sun and the Earth hiding the sun from men's view in greater or less degree, or when the Earth is between the sun and the moon and so casts the latter into shadow. It was probably the recognition first that eclipses occur under these conditions, and secondly that they do not *always* occur when the moon is new or full, which led astronomers to the careful study of the moon's apparent path on the star-sphere, in order that they might ascertain under what conditions on the one hand she casts the sun into shadow or passes herself into the Earth's shadow, or, on the other, though passing the critical parts of her orbit, yet fails to bring about the phenomena either of solar or of lunar eclipse.

(318.) Thus studying the moon's movements, astronomers recognised her apparent path among the stars in any given circuit of the heavens as inclined rather less than 5° 9′ to the ascertained path of the sun. At whatever time they began their watch observing the moon night after night for three or four months in succession, they found her always pursue a course round the star-sphere which crossed the sun's path or ecliptic at two points opposite each other, attaining midway between these points a range of 5° 9′ above or north of the ecliptic, and a range of 5° 9′ below or south of the ecliptic.

(319.) But if their observations were continued for a longer time—for a year or for several years—or if they returned to the inquiry after intervals of a year or two between the observations which had seemed to indicate a particular position for the lunar path, they must presently have found that, while the moon's path is always inclined at an angle of about 5° 9′ to the ecliptic, the crossing-places are not constant. Suppose, for example, that observing the moon's course during a single circuit they had found her passing across the ecliptic along the track A B, fig. 101, crossing the sun's track descendingly at N ; then, observing the moon's motion a year later they would find her passing across the ecliptic along the track *a b*, crossing the sun's track

descendingly at *n*. (These are the actual tracks followed by the moon from January 23 to January 26, 1887, A N B; and from January 13 to January 15, 1888, *a n b*.) Careful study would show that the shifting of the moon's crossing-places backwards or towards the west to meet the motion of the sun takes place irregularly, and indeed is sometimes replaced by a motion forwards or towards the east. But on the whole it takes place at such a rate as to complete one circuit in about $18\frac{2}{3}$ years.

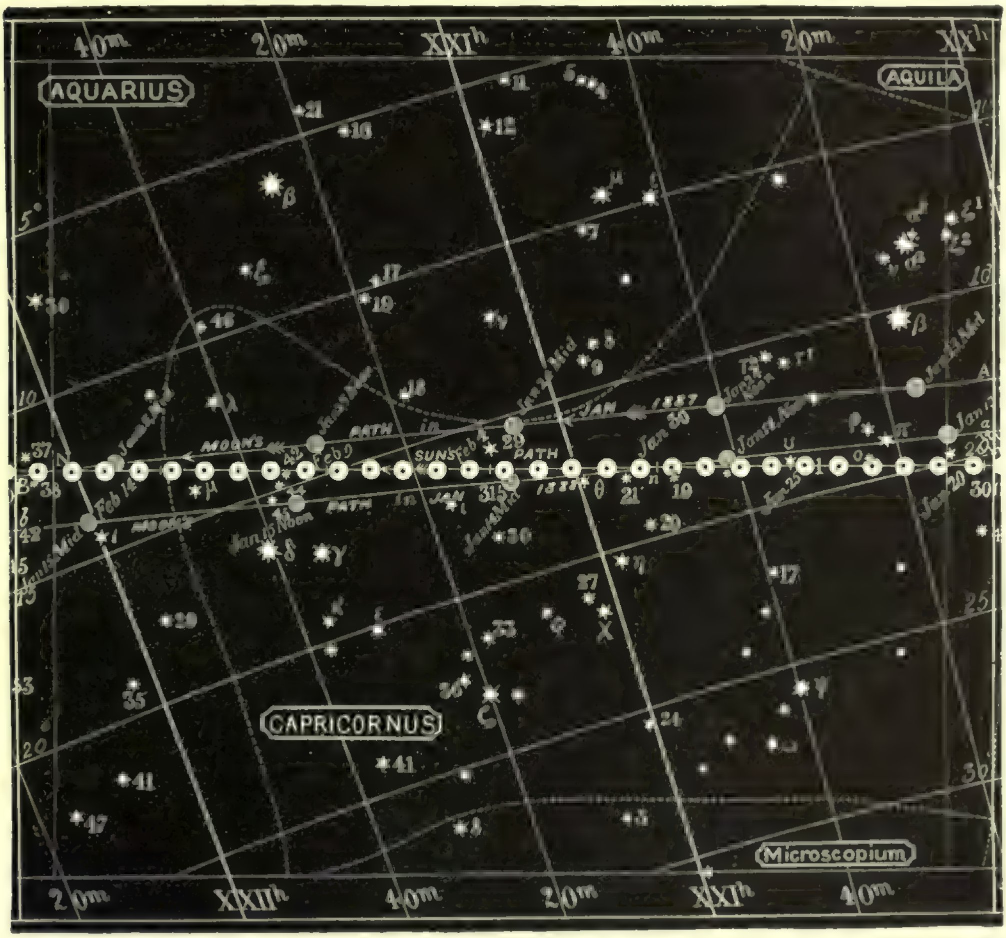


Fig. 101.—Path of the Moon across the sign Aquarius, constellation Capricornus, in January 1887 and January 1888.

(320.) It was evident, then, to the early observers of the moon that she was freer in her motions than the sun seemed to be. For, whereas he was seen to travel along one and the same path with no variation but a slight change of velocity, the moon was seen to travel on a path which varied in position within certain limits. The track of her centre always lay between the two parallels of latitude 5° 9′ north and 5° 9′ south of the ecliptic; but, subject to this condition, it was free to assume any position whatever.

Closer observation, indeed, would have shown that the inclination of the moon's path to the ecliptic was itself slightly variable, ranging from a minimum of about $5^\circ\ 3\frac{2}{3}'$ to a maximum of about $5^\circ\ 13\frac{2}{3}'$.

(321.) The actual path of the moon around the star-sphere varies not only in position, but in character in different months. Her track may have a greater inclination or a less inclination to the plane of the equator than the sun's track has, or about the same inclination as his, according to the position of the points of crossing. The principal cases are considered for the average inclination of the moon's orbit in what follows :—

(322.) Let S E N W (fig. 102) represent the plane of the horizon, N being the north point and S P N the visible celestial sphere. Let E Æ W Æ′ be the celestial equator, the arrow on this circle showing the direction of the diurnal motion; and let W *e* E *e′* be the ecliptic, the arrow showing the direction of the sun's annual motion. The ecliptic is only placed, for convenience of drawing, in such a position as to cross the equator on the horizon at E and W; twice in each day it occupies that position as it is carried round by the diurnal motion, and once in each day it is in the exact position indicated in figs. 102 and 103; that is, with its ascending node (or the first point of Aries) just setting in the west.

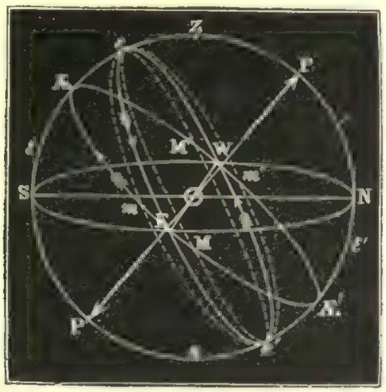


Fig. 102.—Showing the Moon's path round the Star-sphere when it has its greatest and least inclinations to the celestial equator.

(323.) Now let us suppose that the rising node of the moon's orbit is at W, the place of the vernal equinox. Then W M E M′ is the moon's orbit, *e* M and *e′* M′ are arcs of about 5° 9′; and we see that the range of the moon north and south of the equator exceeds the range of the ecliptic (that is, of the sun) by these equal arcs. In other words, the moon when at M is about 28° 36′ north of the equator, instead of being only about 23° 27′ north, as she would be if she moved on the ecliptic; while when at M she is about 28° 36′ south of the equator: she moves throughout the sidereal month as the sun moves throughout the sidereal year, passing alternately north and south of the equator, but with a greater range, due to the greater inclination of her orbit. Accordingly, she remains a longer time above the horizon when at any given stage of the northern half of her orbit, and she remains a shorter time above the horizon when at any given stage of the southern half of her orbit, than she would be if she moved on the ecliptic. She also passes higher than the sun above the horizon when at her greatest northerly range, attaining at this time (in our latitudes) a height of more than 66°, as at M, instead of less than 61°, as is the case with the sun, at *e*; and she is correspondingly nearer the horizon in southing when at her greatest southerly range from the equator, attaining, in fact, a southerly elevation of less than 10°, as at m, instead of more than 15°, as is the case with the sun, at *ε*.

(324.) Next let us suppose that the descending node of the moon's orbit is at W (fig. 102), the place of the vernal equinox; then W *m* E *m′* is the moon's orbit; *e m*

and $e\,m'$ are the arcs of about 5° 9′; and we see that the range of the moon north and south of the ecliptic is less than the range of the sun by these equal arcs. The moon when at m is about 18° 18′ north of the equator, instead of 23° 27′, and she is about 18° 18′ south of the equator when at m'. Thus she has a smaller range than the sun north and south of the equator. She does not attain a greater elevation above the southern horizon than about 56°, as at m; but, on the other hand, her least elevation when due south exceeds 20°, as at μ (the sun's greatest and least southerly elevations, as at e and ε, being respectively about 61° and about 15°).

(325.) Thirdly, let the rising node of the moon's orbit be near e, the place of the summer solstice (fig. 103); then e M e' M′ is the moon's orbit, which crosses the equator at two points, M and M′, in advance of the equinoctial points W and E.[1] We see that its greatest range from the ecliptic is attained nearly at the points e and e', and is therefore appreciably equal to the sun's range. The circumstances of the moon's motion must therefore resemble very closely those of the sun's, the chief difference resulting from the fact that the nodes of the moon's orbit on the equator are some twelve or thirteen degrees in advance of the equinoctial points.

(326.) Lastly, similar considerations apply when the descending node of the moon's orbit is near e, the moon's path being in this case $e\,m\,e'\,m'$, and its nodes on the equator some twelve or thirteen degrees behind the equinoctial points.

(327.) The moon's orbit passes through the complete cycle of changes (of which the above four cases are the *quarter* changes) in about 18·6 years, the lunar node moving on the whole backwards on the ecliptic. Thus, if such a cycle of years begin with the moon's orbit in the position W M E M′ (fig. 102), then in about a fourth of the cycle (that is, in about 4·65 years) the moon's orbit is in or near the position $e'\,m'\,e\,m$ (fig. 103), the node having moved backwards from W to near e', or one-quarter of a revolution. One-fourth of the cycle later—that is, about 9·3 years from the beginning of the cycle—the moon's orbit is in or near the position E m' W m (fig. 102), the node having moved still backwards from e' to near E. Yet another fourth of the cycle later or about 13·95 years from its commencement, the moon's orbit is in or near the position e M e' M′ (fig. 103), the rising node having shifted backwards from E to near e; and lastly, at the end of the complete cycle of 18·6 years, the moon's orbit is in or near its original position.

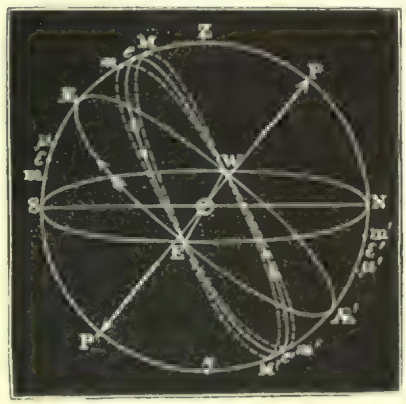


Fig. 103. Showing the Moon's path round the Star-sphere when it has its mean inclination of the celestial equator.

(328.) It will be manifest if we consider fig. 101 and the general relations which that figure illustrates, that when the moon is passing the sun (at the time of new moon) she may be close to the ecliptic so that her disc may overlap or even actually cover the disc of the sun, or on the other hand she may

[1] These points and the points m and m' are about 12½° from E and W, being determined by the relation that they are on the equator, and about 5° 9′ north and south of the ecliptic.

pass by the sun at a place where her course is at a considerable distance from the ecliptic so that no such partial or total concealment of his disc can occur. Furthermore it is obvious that exactly opposite the place of the sun the Earth's shadow must fall athwart the ecliptic, its centre travelling along the ecliptic *pari passu* with the motion of the sun's centre along the ecliptic, as shown in the twelve maps, figs. 86–97, the centre of the Earth's shadow being always 180° from the centre of the sun, or at exactly the opposite part of the sky. And the moon travelling round the star-sphere in the way we have considered, she may either, when passing the place where the Earth's shadow falls athwart the ecliptic, travel clear of that shadow or pass into it, so that more or less of her disc will be in shadow.

(329.) We see, then, how the study of the movements of the sun and moon was early associated with the attempt to predict eclipses of these bodies, an eclipse of the sun being an occasion when the moon in passing between the Earth and sun conceals a portion or the whole of his disc, while an eclipse of the moon is an occasion when the moon in passing the part of the ecliptic [1] opposite the sun falls partly or wholly into the Earth's shadow. Such occurrences will be more particularly considered in their proper place farther on. Here it is only necessary to notice that since the moon has throughout any year nearly the same crossing-places on the ecliptic month after month (fig. 101 shows how slightly a whole year changes the position of one of the crossing-places); and since the sun passes the two crossing-places at intervals of rather less than half a year (the centre of the Earth's shadow behaving in this respect precisely as the sun's centre does), it follows that at two periods in each year the sun and the Earth's shadow are for awhile so near one or other of the moon's two crossing-places that she cannot pass without an eclipse of the sun when she is new, or an eclipse of the moon when she is full. One or other kind of eclipse, or eclipses of both kinds, must occur while the sun and the earth's shadow are in or near this position. Thus there are two epochs in each year at or near which eclipses can occur; and they can occur at no other times during the year. These may be called *eclipse-months* or *eclipse-seasons*. The interval from the middle of one to the middle of the next would be exactly half a year but for the slow shifting of the crossing-places, already described, Art. 319. This causes the mean interval to be less than half a year by about ten days.[2]

[1] The Ecliptic is so called because an Eclipse (ἔκλειψις, *quitting* or *disappearance*) whether of the sun or moon can occur only when the moon is on or close to the ecliptic.

[2] The average interval may be more exactly determined as follows:—Since the descending or ascending node of the moon's path travels backwards round the signs once in 18·6 years, the sun passes either node of the moon's orbit 19·6 times in 18·6 years, and both nodes twice as often, giving for the mean interval required $\frac{186}{392}$ of a year, or about $173\frac{1}{3}$ days.

(330.) In the eclipses which they predicted and observed under varying conditions, the ancient astronomers recognised further evidence of the real relations of the apparent paths of the sun and moon around the Earth. The sun's almost uniform motion and scarcely perceptible change of size in his apparent yearly circuit showed that if he really moves round the Earth, it must be in a nearly circular orbit, and with nearly uniform velocity. The moon's monthly motion and slight change of apparent size showed the same in regard to her circuit. The lunar phases showed that her monthly orbit lies far within the sun's yearly path. Eclipses removed all possible doubt from this conclusion. On the geocentric hypothesis, which naturally approved itself in the first place to the minds of astronomers, it was now established that the sun goes round the Earth once in $365\frac{1}{4}$ days, while the moon goes round her at a much smaller distance once in $27\frac{1}{3}$ days, with an apparent freedom to change her path (not only with regard to the crossing-places on the ecliptic, but with regard also to the position of the point of nearest approach to the Earth) suggestive of instability as compared with the steadfast motions of the sun. Yet a law could be recognised in these changes.

(331.) Thus far we have considered observations relating to the moon's motions referred to the ecliptic or the track of the sun. We must consider next the moon's motion as referred to the star-sphere :—

In any given month the moon traverses appreciably a circuit around the stellar heavens resembling that traversed by the sun, but never actually coincident with the ecliptic. The time of circuit is not, like the sun's, appreciably constant. Yet the variation is hardly such as the ancient observers would have been apt to notice, and need not occupy our attention here. The actual mean period of circuit must have been early determined with great accuracy, as on it depended the prediction of eclipses of the sun and moon. This period is, of course, considerably less than that lunar month, the mean time in which the moon passes through her phases, which had earlier occupied the attention of astronomers. For, when the moon having passed from conjunction with the sun, has gone through her phases, and is again passing to conjunction with the sun, she has to attain to a position (in longitude) towards which the sun has been advancing all through the lunar month. In untechnical terms she has not only to complete the circuit of the star-sphere to the longitude from which she started, but to traverse, over and above that circuit, the arc over which the sun has passed in the month.

(332.) The mean time taken by the moon in traversing the circuit of the heavens, or the mean length of the *sidereal month*, is 27 d. 7 h. 43 m. $11\frac{2}{5}$ s. If we measure the moon's motion from the first point of Aries, which is itself

moving slowly in a direction contrary to that of the motion of the sun and moon, we get a slightly shorter month, for the same reason that the tropical year is less than the year of seasons ; but the difference only amounts to $8\frac{3}{5}$ seconds. But the sidereal month is the true period of the moon's circuit of the heavens, all other lunar periods being in a sense factitious.

The common lunar month is the period of what is called a *synodical revolution* of the moon, or the mean interval between successive conjunctions of the sun and moon : its length is 29 d. 12 h. 44 m. 3 s.

(333.) Observed throughout any circuit of the star-sphere the moon is found to travel with varying velocity, indicating eccentricity akin to that of the sun's apparent orbit, but greater. The ancient astronomers recognised this peculiarity, even as they had recognised the variation of the sun's rate of motion. They misinterpreted it also in the same way in consequence of their attempt to explain it by the simple eccentricity of the lunar orbit, without assuming any actual variation in the moon's rate of motion. The actual mean eccentricity of the moon's orbit is about $\frac{11}{200}$; that is to say, if 200 be taken as the moon's mean distance, her greatest and least distances would be represented respectively by 211 and 189 when her orbit is in its mean condition. But the eccentricity ranges from this value, or $\frac{55}{1000}$, to $\frac{66}{1000}$ at its greatest and to $\frac{44}{1000}$ at its least value. The ancients supposed it to be nearly twice as great, explaining the moon's varying motion as resulting entirely from eccentricity. They found that the position of the moon's perigee was not fixed on her orbit, any more than the position of her rising and descending nodes on the ecliptic had been found to be. But whereas the place of the node was found to be on the whole retrograding, so as to make the passage from node to node rather less than a sidereal lunar month, the place of the perigee was found to be on the whole advancing, so as to make the passage from perigee to perigee rather more than a sidereal month. The perigee thus advancing (on the whole) completes the circuit of the heavens in 8·85 years.

(334.) The following table of lunar months serves to show how they differ from the mean sidereal month :—

	Days
Common lunar month (*synodical revolution*)	29·53059
Mean interval from perigee to perigee (*anomalistic revolution*) . .	27·55460
Mean time of circuiting star-sphere (*sidereal revolution*)	27·32166
Mean time of circuiting the signs (*tropical revolution*)	27·32156
Mean interval from rising node to rising node (*nodical revolution*) . .	27·21222

(335.) Observations of the other five orbs which were seen to move upon the sphere of the fixed stars revealed other peculiarities of motion, suggesting a much greater degree of freedom than the moon showed, or (which is much the same thing) presenting even more difficult problems for solution

when astronomers attempted to recognise law and orderly sequence in these movements.

(336.) The planets Venus and Mercury could only be observed along those parts of the tracks of each which were at a certain distance from the sun. We must not exaggerate, as many have done, the observing acuteness of the ancient astronomers, who discovered that the planet Mercury, which they saw at certain times on the east of the sun as an evening star, is the same orb which they saw at other times on the west of the sun as a morning star. In countries like England, Germany, and France, indeed, astronomers might observe for centuries without noticing Mercury, which is seldom visible to the naked eye, and scarcely ever conspicuous. Copernicus never saw Mercury, though he anxiously looked for the planet whenever it was favourably placed. But in Egypt and Babylon, and in similar latitudes north and south of the equator, Mercury, for a full fortnight at each elongation, is not merely visible, but staringly conspicuous. I have repeatedly seen the planet, both as a morning and evening star in the Middle and Southern States of America, standing out more strongly than Jupiter is ever seen as a morning or evening star in England. In Egypt and Babylon, Mercury must have been even more obvious at his rapidly alternating appearance as an evening and morning star. Indeed the name given to the planet by the Greeks—*ὁ στίλβων* the sparkling (star)—shows that Mercury was regarded as strikingly conspicuous even in Greece, where the conditions for observing it were not as favourable as in Mesopotamia or Egypt. The Indians gave the planet a name having the same meaning.

(337.) Observing Mercury only when nearly at his greatest distance east and west of the sun, the ancient astronomers recognised the brilliant planet as in a sense accompanying the sun in his annual circuit round the ecliptic, being never more than some 26 or 27 degrees from the sun either on the east or on the west. Thus they would assign to Mercury a mean period of circuit equal to the sun's, or one year. The oscillation on either side of the sun seemed to occupy rather less than four months, or about 116 days, in which time the planet passes from its greatest elongation on the east of the sun to its greatest elongation on the west, and thence back to its greatest elongation on the east again. This would suggest that while travelling with the sun round the Earth, once in a year, Mercury travels round the sun in a period considerably less than 116 days.

(338.) For if M, fig. 104, is the position of Mercury when seen from the Earth, E, at its greatest distance east of the sun at S, and the planet is again seen similarly situated with respect to the sun after 116 days, when the sun is at S′, Mercury being at M′, it is evident that the planet has in that time not merely made one

circuit, which would have carried it only to M, but a circuit increased by an arc measuring the angle M E M′, equal to the angle S E S′ swept out by the sun in moving (apparently) round the arc S S′ in 116 days. In other words, Mercury appears to traverse in 116 days an arc corresponding to the motion of the sun in his apparent orbit during $365\frac{1}{4} + 116$, or $481\frac{1}{4}$ days. This gives the time of Mercury's apparent circuit round the sun as

$$116 \text{ days} \times \frac{365\frac{1}{4}}{481\frac{1}{4}}$$

or about 88 days.

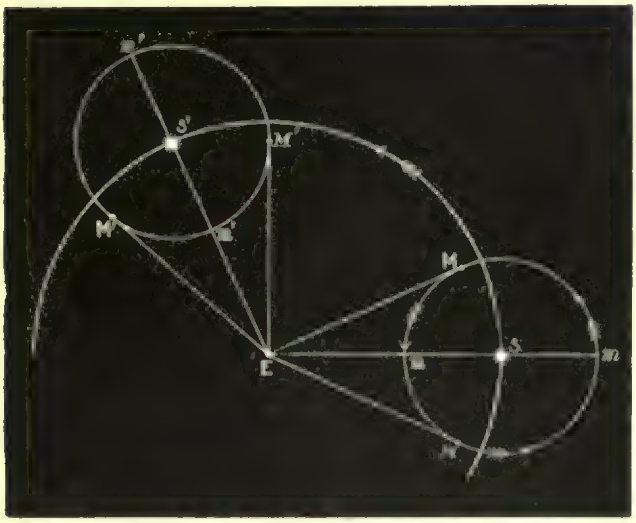


Fig. 104.—Illustrating the apparent motion of Mercury around the Earth.

(339.) It appears that the Egyptian astronomers did thus regard Mercury as travelling around the sun in an orbit large enough to have an apparent span of about 48° in a period of about 88 days, whilst the sun circled once in a year round the Earth. In this way, by giving to Mercury's supposed path round the sun its due inclination to the ecliptic, about 7°, and its proper eccentricity as indicated by the varying amount of elongation noticed at different times, the ancient observers were able fairly well to 'save appearances,' and to obtain rules for predicting the apparent movements of the planet. But if they had observed with anything like accuracy the actual eccentricity of the apparent orbit of Mercury round the sun, they must have discovered that uniform motion along an orbit of that eccentricity would not account for the observed variation in the rate of the planet's apparent motion round the sun.

(340.) Observations of Venus would point to the same general conclusions in this case as in the case of Mercury. The brighter planet ranges farther on either side of the sun, its mean range being no less than 46°; so that the apparent path of Venus round the sun has an angular span of about 2° more than a full right angle. The time in which the planet completes an apparent oscillation on either side of the sun—that is, from its greatest elongation on the east to its greatest elongation on the west, and thence back to its greatest elongation on the east again—amounts to nearly 584 days, giving for the period of revolution around the sun (by reasoning similar to that employed in Mercury's case)

$$584 \text{ days} \times \frac{365\frac{1}{4}}{949\frac{1}{4}}$$

or 224·7 days.

(341.) In the case of Venus, somewhat clearer evidence was given than

in the case of Mercury that the planet really has this subordinate movement around the sun while sharing the sun's apparent motion around the Earth.

For, if *s* S *s'* (fig. 105) represent part of the sun's course round the Earth at E, while M *M* m and V *V* v represent the epicycles of Mercury and Venus, it is clear, from the greater range of Venus on either side of the sun, that we must assign to her a wider course than to Mercury in the same degree that V *V* v in fig. 105 exceeds M *M* m. Hence, obviously, there is a much greater discrepancy between the arcs V v *V* and V v *V* than between the arcs M m *M* and M m *M*. This is actually observed, Venus taking a much shorter time--only about $141\frac{1}{2}$ days—in passing from her greatest elongation as an evening star, and on the east of the sun, as at V, to her greatest elongation on the west of the sun as a morning star, as at V, than in passing from the last-named position back to her greatest elongation as an evening star again. Moreover, the variations of distance as indicated in fig. 105, and the inferred variations in the amount of the illuminated portion of Venus turned earthwards—assuming her to be, like the moon, an opaque body deriving her light from the sun—correspond closely with what is actually observed as Venus passes through her varying apparent positions with respect to the sun.

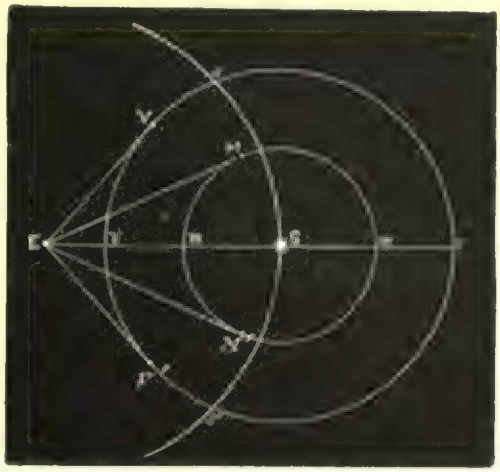


FIG. 105.—Illustrating the apparent motion of Venus around the Earth.

(342.) We have no direct evidence, however, that Egyptian or Babylonian astronomers either observed Venus and Mercury closely enough to recognise such peculiarities, or that if they did they drew the necessary inferences. All we know is, that if they had made such discoveries they would in all probability have kept them concealed among the mysteries of their religion, in which the planets, including the sun and moon, were deities. It is known, however, that, according to the ancient Egyptians, Mercury and Venus travelled round the sun, while he travelled round the earth ; and such a theory respecting the inferior planets really involves ideas respecting the superior planets also (as will presently appear) altogether inconsistent with the commonly accepted notion that ancient astronomers recognised no motion of the superior planets around the sun.[1]

[1] We have no means of judging what Egyptian or Babylonian astronomy was like, except the statements made by writers like Diodorus Siculus, Proclus, Macrobius, and others, who, while they evidently had but the vaguest information (derived through imperfect sources), were themselves so ignorant of astronomy that their account even of matters fully reported to them would have had no scientific value. Macrobius (*In. Somn. Scrip.*, Book I.) says the Egyptians held the opinion that Mercury and Venus move round the sun. But later this theory was attri-

(343.) The apparent path of Venus around the sun, like that of Mercury, was found not to lie in the plane of the ecliptic—the inclination being in her case, however, only about $3\frac{2}{5}$ degrees, or less than half that of Mercury. The orbit of Venus around the sun might also have been found to be eccentric, though not nearly so eccentric as the orbit of Mercury.

(344.) Turning to the outer planets—Mars, Jupiter, and Saturn—the ancient astronomers recognised peculiarities of motion even more striking

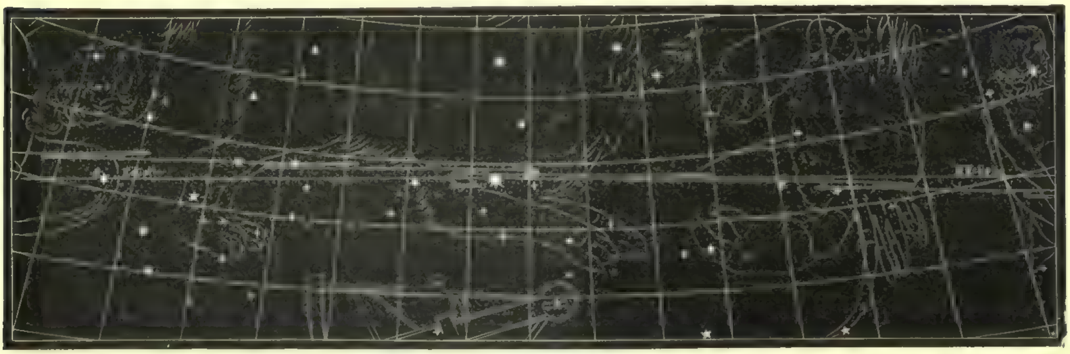


FIG. 106.—Five Synodical Loops (traversed in $5\frac{1}{6}$ years) of the path of Saturn 4,000 years ago. (From 'Saturn and its System.' 1865.)

than those of Mercury and Venus, though not at a first view so easily interpretable.

(345.) Taking first the most slowly-moving planet, Saturn, as the one which seems likely to be most easily dealt with, the astronomers of old times, like the astronomers of to-day, recognised in Saturn an orb which seemed to circuit the celestial sphere in a period of about twenty-nine years, making a series of small loops after the manner shown in fig. 106, where five such loops are shown along the portion of the ecliptic corresponding to the signs Gemini and Cancer of that time. The planet is supposed to pass on the whole from right to left, so that each short loop is traversed backwards, or from left to

buted to Apollonius of Perga. May we not well believe that the views vaguely attributed to Pythagoras, Philolaus, Heraclides, and Nicetas, were in reality such notions of Egyptian astronomy as the purposely veiled accounts of the Egyptian priests enabled Pythagoras to form during his travels in Egypt, and such as he had handed down to his followers, and as his followers had distributed—not improved in clearness—to others. When Pythagoras stated that all the heavenly bodies moved round the central fire (as Aristotle and Plutarch relate); when (as Cicero tells us) Heraclides and Nicetas supposed the Earth to revolve round the centre of the universe, and Philolaus taught that the Earth is one of the stars or planets, we may more reasonably believe that here we have traces of the advanced nature of ancient Egyptian and Babylonian astronomy, not understood by those who handed on these stories, than that Pythagoras and the rest only intended these sayings to be understood in some mystical or non-natural sense. What the Pyramid shows of the actual observing skill of Egyptian astronomers renders the former view antecedently probable. What we know of the ways of the later Egyptian priests strongly confirms it. On the whole, it is more probable that the Egyptians held the true heliocentric theory than even that they held Tycho Brahé's admirable compromise between that and the Ptolemaic theory.

right. Thus the planet advances between two loops till it reaches a certain turning-point called its station, then travels backwards till it reaches another station, and so forwards again to another loop which it traverses in a similar manner. Saturn traverses twenty-nine such loops in passing round the star-sphere, completing the circuit in twenty-nine years. It will be observed that his path is inclined to the ecliptic. In fig. 106 I have set the place of crossing to correspond with the position which it had some 4,000 years ago ; and the signs are marked in the position they then had. In fig. 107, five loops of Saturn, over a similar portion of his path, in the years 1860 to 1865, are similarly shown.

(346.) It will be observed that the loops vary in shape as the planet traverses different parts of his circuit of the heavens. The actual changes taking place during a complete circuit are discussed at length for Saturn in my 'Saturn and its System,' but can be sufficiently inferred from what is stated a little farther on with regard to Jupiter.

(347.) Now if it were merely observed that Saturn traverses such a course as is shown in figs. 106, 107, it can readily be understood that the

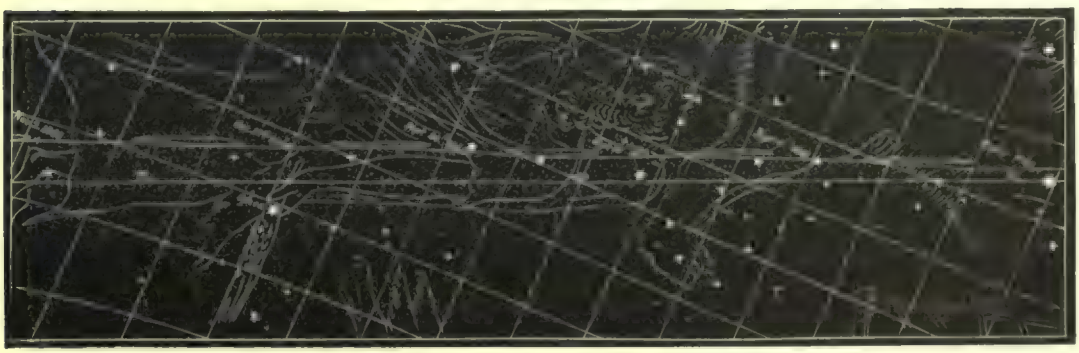


Fig. 107. Five Synodical Loops (traversed in $5\frac{1}{6}$ years) of the path of Saturn, 1860–1865. (From 'Saturn and its System.' 1865.)

observers would be led to recognise as the true path of the planet a looped course resulting from his motion, with more rapid course, around a small circle whose centre was advancing around the Earth. This is the usual statement made in books of astronomy. But the astronomers of old times must have been much more struck by two peculiarities affecting the motion of Saturn, and recognised in the case of each of the other superior planets. The planet is always lost to view (passing through conjunction with the sun) when in the midst of its long advancing arc, while it is exactly opposite the sun (that is on the meridian at midnight) when in the middle of its short retrograde arc. The astronomers of old times could hardly suppose that these coincidences were purely accidental ; and regarding them as not accidental, they

must have inferred that the motion of the planet was in some way regulated by the motion of the sun.

(348.) Having already noticed that Mercury and Venus moved precisely as if travelling round the sun each in a certain period, while he travelled once in a year round the Earth, the old astronomers can hardly have failed to inquire whether the outer planets too might not travel round the sun, each in its own proper period, while he travelled, with the motion already recognised, once in a year round the Earth. As it is certain that among the old astronomers of Egypt and Babylonia were many excellent mathematicians, and as no mathematician could fail to recognise the perfect agreement of the motions of the superior planets with the theory naturally suggested both by what had been observed in the case of the inferior planets and by the synchronism between the movements of the superior planets and the sun, it can hardly be doubted that the Egyptian astronomers had in reality anticipated the whole of what has been called the Tychonic theory of the Solar System, according to which all the planets move, each in its proper period, round the sun,[1] while the sun revolves in a year around the Earth. This need not prevent us from believing that later the Egyptians may have made the Earth also revolve around the sun, as in the true heliocentric theory. See note on p. 153.

(349.) Observation of the planet Jupiter led to similar results. His more brilliant orb was found to circle the star-sphere in a shorter time—twelve years instead of twenty-nine—the backward loops were found to be larger; but the method of circuit was seen to be precisely the same. The planet advanced through a long arc, reached its station, then retrograded over a short arc to the next station, thence advancing through a long arc as before. And always it was noticed that while the planet was invisible (being in conjunction with the sun) when in the midst of its long advancing arc, it was at its brightest, and exactly in opposition to the sun (or at its highest at midnight), when in the midst of its short retrograde arc.

(350.) In the case of Jupiter, as in Saturn's, the loops traced out by the planet were observed to change in shape. Fig. 108 shows one half the changes observed in Jupiter's loops—those, namely, by which a loop above

[1] I do not give the proof of the theorem that precisely the same motions result if the sun moves in a year round the earth, each planet moving in its period round the sun, as if all the planets, the Earth included, move in their proper period round the sun, or if each planet moves in a small circle once in a year around a centre which itself moves in a large circle around the Earth in the planet's proper period. For while such propositions, and their demonstration, have no interest for non-mathematicians, mathematicians would not be contented with so mere a sketch of the demonstration as could alone find a place in these pages. I may refer the student of such subjects to the later sections of my treatise on the *Geometry of the Cycloidal Curves*, a book written expressly for the study of the many problems relating to such curves which arise from the study of the planetary motions. But an outline of the proof is given farther on.

the mean track is changed through six forms into a similar loop below the mean track. It will be observed that the middle form of the seven shown is not looped, but simply twisted. The remaining half of the curves traversed by Jupiter in his complete circuit of the heavens change through forms similar to those shown in fig. 108, taken in order from below upwards and inverted right and left (or seen as from behind, or by reflection at a mirror). It is easy to recognise how these variations of form result from the motion of the Earth on a path inclined to the plane of the planet's path, and crossing that plane at points occupying varying positions with respect to the planet in each synodical revolution.

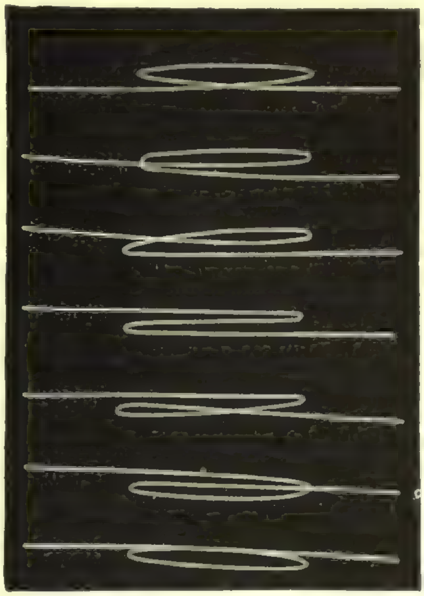


Fig. 108. - Showing the forms assumed by the Loops of a Planet's path.

(351.) The movements of Mars seemed at a first view somewhat different in character from those of Saturn and Jupiter; though on more careful inquiry it was seen that they only differ from those of the more remote planets in the same sense that the movements of Venus differ from those of Mercury. We have seen that, whereas Mercury passes through more than three apparent circuits of the sun in going with him once round the star-sphere, Venus does not even complete one circuit in a year, but takes 584 days in making an apparent circuit round the sun, while in that time making with him more than a circuit of the star-sphere, in the degree in which 584 exceeds $365\frac{1}{4}$. The case of Mars is similar. He makes a series of loops round the star-sphere, but he completes more than a circuit of the star-sphere between the tracing out of successive loops. He is out of sight, or so inconspicuous as not to be much noticed among the fixed stars, for a much longer time than Jupiter or Saturn. It is only when he is traversing the short backward arc of his loop that he is really conspicuous among the stars. In the middle of that retrograde motion he is at his brightest; and like the other superior planets, he is then directly opposite the sun, and shines with his greatest lustre when passing his meridional culmination at midnight. His appearance while advancing to the station preceding his retrograde motion, and after passing the station at the other end of his retrograde arc, is that of a conspicuous but not very striking red star. During his retrograde motion he shines brightly, but

not with equal brightness at each return to this favourable position. Sometimes he is almost as bright as Jupiter, from whose white orb, however, the ruddy hue of Mars always distinguishes him, as the dull yellow light of Saturn distinguishes him from both Mars and Jupiter at other times. Even when thus favourably situate, Mars is outshone by Jupiter. After completing a loop Mars advances, gradually diminishing in lustre, till he disappears from view, passing to conjunction with the sun, at a part of the zodiac rather more than half-way round from the middle of his retrograde arc (the place of opposition to the sun). Afterwards, continuing to advance, gaining on the sun, he draws onwards to the place of his next loop, which has the middle of its retrograde arc about one-seventh of the ecliptical circuit in advance of the middle of the preceding loop. So do the successive loops of Mars advance round the zodiac, but always a complete circuit of the zodiac is accomplished between each.

(352.) The relative dimensions of the loops or twistings traversed by the five planets known to the ancients can be inferred from fig. 109, which shows one loop of each path as actually mapped in to scale during the same biennial period a (few years ago). But it must be understood that, as figs. 106 and 107 show, the loop of Saturn goes through changes of form akin precisely to those described in the case of Jupiter; only there are twenty-nine changes instead of twelve: while the loops of Mars, Venus, and Mercury, though having always about the span (as compared with the span of the loops of Saturn and Jupiter) shown in fig. 109, vary greatly in form. The reason in the case of Mars and Venus is that the paths of these planets lie next to the Earth, on the outside and inside of her track: this proximity causes the inclination of the orbits of the two planets to the ecliptic to produce more conspicuous effects than the inclination of the more remote planets Saturn and Jupiter. In the case of Mercury, it is the large inclination of the planet's path to the ecliptic which causes the loop to undergo remarkable changes of shape.

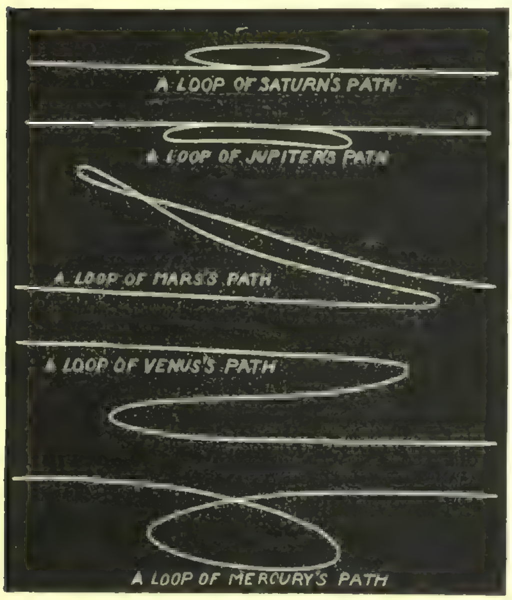


FIG. 109.—Forms of some of the Loops traversed by the Planets in the Star-sphere.

(353.) In figs. 110 and 111 the apparent paths of Saturn and Mars through a loop of either are shown on the same scale.

(354.) In fig. 112 the paths of Jupiter and Mars in 1884 are shown in the same zodiacal map.

(355.) It seems impossible to doubt that the keen observers and excellent mathematicians[1] who formed the school of Egyptian and Babylonian astronomers, from which the Greek school derived its chief principles, can have failed to be struck by the exact synchronism between the movements of the five planets and of the sun. It appears probable, indeed, as I have already pointed out, that these old astronomers even adopted the theory that the sun is the true centre of the motions of all the planets, our earth included. The mechanical and physical objections against such a theory could not probably be met by any arguments available in those times, any more indeed than they could be met by the astronomers of the Middle Ages for nearly a century after the time of Copernicus. But the obvious simplicity of the scheme would be apt to make it acceptable. Pure mathematics alone, however, suffice to show that all the movements of the planets can be explained as due to circling motions around the sun, in the periods which modern astronomy assigns to the different planets, the sun himself being supposed to circle around the Earth once in a year, at a distance unknown but certainly exceeding many times the distance of the moon, which was estimated by the old astronomers at about 230,000 miles, or not far from its true value.

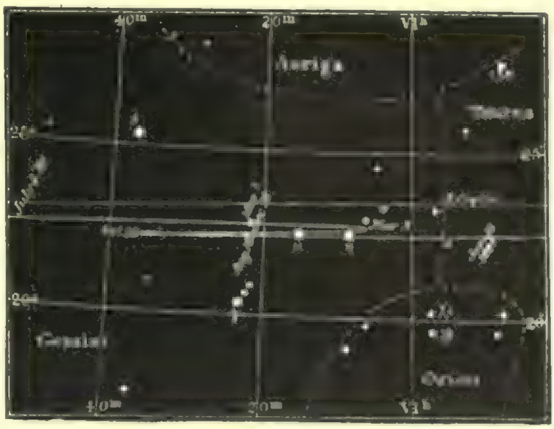


FIG. 110.—Path of Saturn from conjunction in June 1885 to conjunction in July 1886.

(356.) It is quite certain that the ancient Egyptian and Babylonian

[1] I may cite as one among the illustrations of the inability of the ancient Greek and Roman writers who dealt with Egyptian astronomy, to appreciate its value, that Diodorus Siculus speaks of Egyptian astronomy as very defective, since they could not predict solar eclipses accurately, whereas they could predict lunar eclipses well. He manifestly did not in the slightest degree appreciate this difference between the two problems. An eclipse of the moon is visible wherever the moon is above the horizon, and it is easy to calculate beforehand whether the moon will be above the horizon or not when in opposition to the sun. But an eclipse of the sun is only visible along the actual track of the shadow, and the track of the true umbra in total solar eclipses is very narrow compared with the span of the Earth's disc supposed to be seen from the sun.

Fig. 111.—Path of Mars during the opposition of 1886.

Fig. 112.—Paths of Mars and Jupiter during the opposition of these planets in 1884.

astronomers arranged the planets in order of distance corresponding with their periodic motions around the sun, and not around the Earth ; nor, so far as I can see, can any reason be assigned from the Ptolemaic System for arranging the five planets, the sun, and the moon, in the order which was actually adopted among all the civilised nations of the East long before Greek astronomy began.

(357.) This order was as follows :—

1. The Moon.
2. Mercury.
3. Venus.
4. The Sun.
5. Mars.
6. Jupiter.
7. Saturn.

It corresponds with the periods of motion of the sun and moon round the Earth, and of the other planets (for the sun and moon were called planets in ancient astronomy) round the sun. The week, most ancient of all the ancient time-periods, shows still, by the planets associated with its several days, how universal the order above presented must have been. For taking the simple explanation (unquestionably borrowed from very ancient lore) by Dion Cassius, we see how, by assigning to the twenty-four hours of each day the seven planets in the above order, constantly repeated, the first hours of successive days would be devoted to the planets in the following order :—

1. The Moon . .	Monday, *Lunæ dies, Lundi*, &c.
2. Mars . . .	Tuesday, Tuisco's day, *Martis dies, Mardi*, &c.
3. Mercury . .	Wednesday, Woden's day, *Mercurii dies, Mercredi*, &c.
4. Jupiter . .	Thursday, Thor's day, *Jovis dies, Jeudi*, &c.
5. Venus . .	Friday, Freya's day, *Veneris dies, Vendredi*, &c.
6. Saturn . .	Saturday, Sætor's [1] day, *Saturni dies, Samedi.*[2]
7. The Sun . .	Sunday, the day of the chief Deity, *Dies Dominica, Dimanche*, &c.

(358.) Satisfied that all the planets were for the special benefit of man, or ruled over the Earth as deities, the ancient astronomers strove with laudable zeal to ascertain the special influence and power of each planet. Regarding the sun as obviously the life of each year, the giver of harvests, vintage, &c., while the moon as obviously was the measurer of time for men, they strove to assign to each of the other planets some influence suggested by the planet's aspect. Hence, naturally, Saturn, gloomy-looking and slow-moving, was regarded as malefic : on his day rest was enjoined, for he seemed restful, and,

[1] Our Saxon forefathers erred in naming this day, much as the Romans erred in confounding their Hercules with the Greek Herakles. The Saxon Sator or Sætor was not the equivalent of the mythologic Saturn.

[2] The French, Italian, Spanish, and generally the Latin usage, presents Saturday as the Sabbath -Samedi, shortened from 'Sabbatis dies.' The Italian name is Sabbato, the Spanish Sabado, and so on. In Germany also the name Samstag indicates the original idea of rest naturally associated with the day dedicated to the most slowly moving and also gloomiest-looking of all the planets.

moreover, his day was held unfortunate like himself. The splendour of Jupiter as naturally suggested good-fortune and power. The ruddy light of Mars suggested thoughts of war and blood. Venus, most beautiful of all the planets, and shining with chief glory in the twilight, came naturally to be associated with thoughts of love; while Mercury, showing for a short time on either side of the sun, and seemingly shifty in movements, was associated as naturally with the idea of craft and subtlety. Such a disposition of the various planets was the best the old astronomers could devise: and, granted only that the planets really were able to exert special influence on the fortunes of men, such peculiarities of power and influence might most naturally be associated with those several bodies. At any rate the ideas thus conceived obtained in the course of time the most widespread influence, and this influence has been impressively long-lasting. To this day, in India, China, Japan, Persia, Egypt, nay, among the most civilised nations of Europe and America, particular days are supposed to possess specific qualities. Friday is still a day on which it is unfortunate to begin a long journey or any considerable undertaking; but few know that the superstition began when Friday, though sacred to the fortunate planet Venus, was regarded as an unsuitable day for beginning important business, because immediately following it came the day of the unlucky and slow-moving Saturn. Still more widely spread, and even respected, is the idea that there must be general rest from work on the Sabbath, a day really representing (though now shifted to the sun's day through the command of Constantine) the day anciently devoted to Saturn, and most naturally regarded by worshippers of the heavenly bodies as a day which should be devoted to rest and saturnine gloom.

(359.) In reality, of course, the ideas of the ancient astrologers respecting the special influences of the planets on the fortunes of men, though natural enough, had no more foundation than the fancies of the alchemists, according to which the planets rule specially over seven metals, in the order neatly indicated by Chaucer:—

> SOL gold is; and LUNA silver we threpe;
> MARS iron; quicksilver MERCURY we clepe;
> SATURNUS lead; and JUPITER is tin;
> And VENUS copper, by my faderkin.

(360.) We must not forget, however, that it required a great advance in scientific research to enable men to see that these ideas have no real foundation. The essential fact on which all such conceptions respecting the heavenly bodies depend is the seemingly surpassing importance—the size, mass, and apparently manifest stability—of the Earth from which the celestial motions are

observed. So long as men had not learned enough to enable them to recognise the possibility that the Earth might merely seem large and massive because near, and might be at rest only in relation to creatures bound, as it were, by its attraction to its surface, and so unable to recognise such motion as it might really have, they could not be expected to adopt any other idea than that the heavenly bodies were specially created for the benefit of the Earth, and probably for the special control of the various creatures existing upon its surface.

(361.) The astronomy of the ancients, as we find it presented in the chief treatise of Ptolemy, shows us the Earth as the centre of the movements of all the heavenly bodies ; but so far as the five planets Mercury, Venus, Mars, Jupiter and Saturn are concerned, the epicyclic motions assigned to 'save appearances' were purely artificial and improbable.

(362.) A single example will suffice to show this, and (as I think) to indicate further that the Greek astronomers had very imperfect ideas respecting the epicyclic theory of ancient Babylonian and Egyptian astronomers—the true contents of 'the golden'—i.e. solar—'vases of the Egyptians.'

Let E, fig. 113, be the Earth as the centre of the Solar System, A B C part of a circular track round E, along which the centre of Saturn's motion is carried once in the Saturnian period of nearly $29\frac{1}{2}$ years; $a\,s_1$, $s_2\,b$, and $c\,s_3$ positions successively assumed by a smaller circle in which Saturn himself is supposed to travel, in the direction shown by the arrows. Then it is easily seen that by the selection of suitable dimensions for the circles A B C, $a\,s_1$, &c., and suitable velocities for the motion of the centre of Saturn's smaller circuit along the circle A B C, and of the planet's own motion in that smaller circuit, Saturn will move along such an epicyclic path as $s_1\,s_2\,s_3$, corresponding to its apparent motion as seen from E. Such effects as really accrue from the eccentricities of the orbit of the Earth and Saturn round the sun, and from the inclination of Saturn's path to the plane of the ecliptic, can also be readily accounted for by giving due eccentricities to the circles A B C and $s_1\,a$, and assigning a suitable inclination of the plane of the smaller circle to that of the larger. By doing the like for all the planets, we obtain the epicyclic system of Ptolemy—remembering only that the motion along the larger circle, as A B C, was supposed to be obtained from the movement of a spherical shell, such as is shown between the arcs $a\,s_2\,c$ and $s_1\,b\,s_3$, around the central earth E.

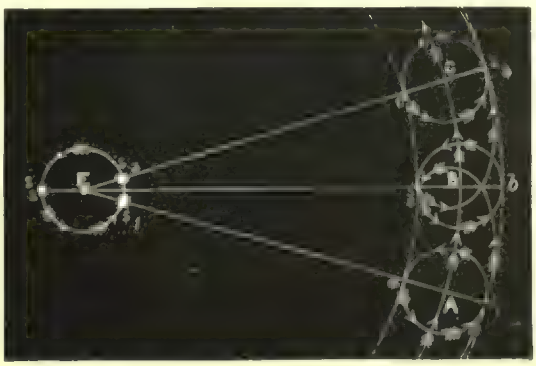


Fig. 113.—Illustrating the Ptolemaic interpretation of the looped path of Saturn.

(363.) The weak point about this Ptolemaic System resides in its failure

to give any account whatever of the characteristic relations between the apparent motions of the planets and the sun.

We have, for instance, in the case of Saturn, to suppose the sun circling around E in such a path as $S_1 S_3 S_2 S_1 S_3$, fig. 113, in exactly the same time that the planet occupies in its circuit of the small circle $a s_1$, and the motion of the sun so adjusted to that of the planet, that when the planet is, as at s_1 or s_3, at its greatest distance from the Earth, the sun lies as at S_1 or S_3 in exact conjunction with the planet; while when the planet is, as at s_2, at its nearest to the central Earth, the sun is, as at S_2, in exact opposition to the planet.

(364.) Why the sun and planet, though moving on entirely independent orbits, should have movements thus exactly synchronising, the believers in the Ptolemaic System were unable to suggest. The various periods observed in the movements of the heavenly bodies, as the day, month, year, and the synodical periods of the planets, were obstinately asynchronous: they could not possibly be reconciled together or shown to be in any way commensurable;[1] here, where one would least expect synchronism, the most perfect synchronism was found to exist.

(365.) But we can obtain precisely the same motions for Saturn (as for any other planet) without using the imaginary circle A B C along which an imaginary centre is supposed to travel. The apparent motion of the sun around the Earth can be used instead, and the epicyclic motion obtained by simple circuit around the sun. Thus, if we suppose, in fig. 113, that $S_1 s_1$ is a radius of a large circle in which Saturn moves slowly (making one circuit in 29½ years) around the moving sun, it will be seen that when the sun has passed over the arc $S_1 S_3 S_2$ in rather more than half a year, $S_2 s_2$ will be the position of this slowly revolving radius, and s_2 the position of Saturn; while when the sun has passed on over the arc $S_2 S_1 S_3$, $S_3 s_3$ will be the position of the slowly revolving radius, and s_3 the position of the planet. It is easily seen[2] that by this epicyclic motion the planet will traverse the path $s_1 s_2 s_3$ with respect to the Earth, precisely as by the other: only, whereas the former motion is artificial and fanciful, involving a synchronism with the sun's motion for which there seems no adequate reason, the motion according to the second system is compounded of the observed apparent motion of the sun round the Earth and of a motion of the planet around the sun akin to what had been already recognised as affording the only natural explanation of the apparent motions of Mercury and Venus. In whatever time and at whatever distance a planet is supposed to revolve around the sun, himself revolving in his apparent yearly course around the Earth, the apparent synchronism of the movements of the planet and the sun must necessarily result.

(366.) This being so, I have, for my own part, little doubt that the epicyclic system of the ancient Egyptian and Babylonian astronomers was of

[1] With what zeal the more ancient astronomers sought for signs of commensurability is shown by their recognition of the approach to commensurability shown among the periods collected together in the celebrated Saros.

[2] The proof of the general proposition will be found in Prop. II. Sect. V. of my *Geometry of the Cycloidal Curves.*

PLATE VI.

Fig. 1.—Mercury.

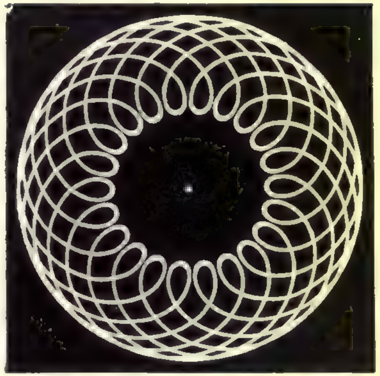


Fig. 2.—Venus.

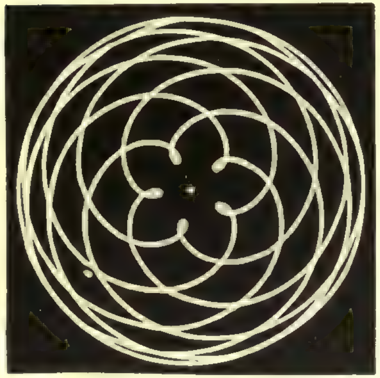


Fig. 3.—Mars.

Fig. 4.—Juno.

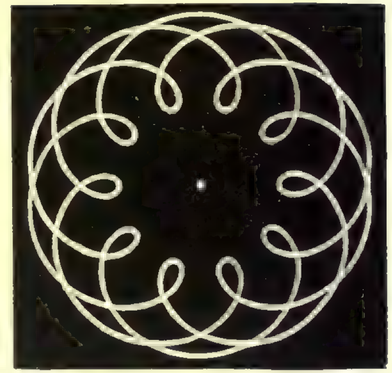


Fig. 5.—Jupiter.

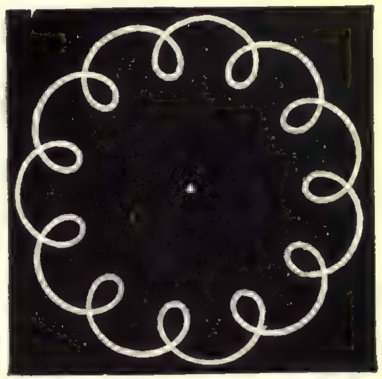


Fig. 6. Saturn.

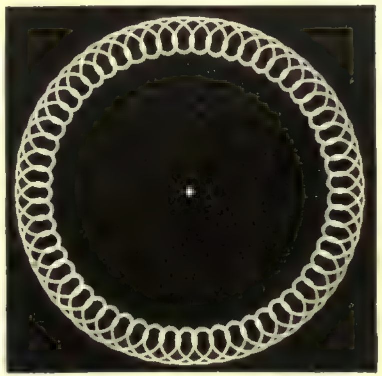
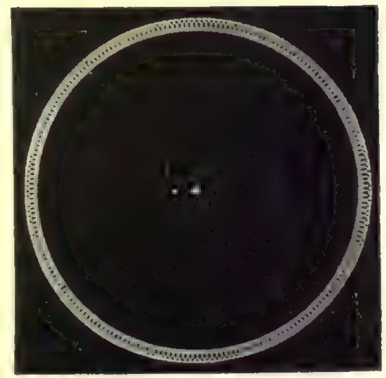


Fig. 7.—Uranus.

Fig. 8.—Neptune



EPICYCLIC GEOCENTRIC PATHS OF THE PLANETS.

On the supposition of uniform circular motions with the following exact ratios between the periods of the planets and the Earth.

Mercury's period to Earth's as	7 to 29.	Jupiter's period to Earth's as	12 to 1.
Venus's ,, ,,	8 to 13.	Saturn's ,, ,,	59 to 2.
Mars's ,, ,,	2 to 1.	Uranus's ,, ,,	85 to 1.
Juno's ,, ,,	13 to 3.	Neptune's ,, ,,	167 to 1.

the kind last described. To outsiders they probably disclosed only the general law of their system,—viz. that the planetary movements are epicyclic,—and the Greek astronomers could get no more out of this than the artificial as well as cumbrous Ptolemaic System. But there is strong evidence to show that among their sacred secrets the older astronomers preserved the only natural epicyclic system, afterwards independently rediscovered by Tycho Brahé.

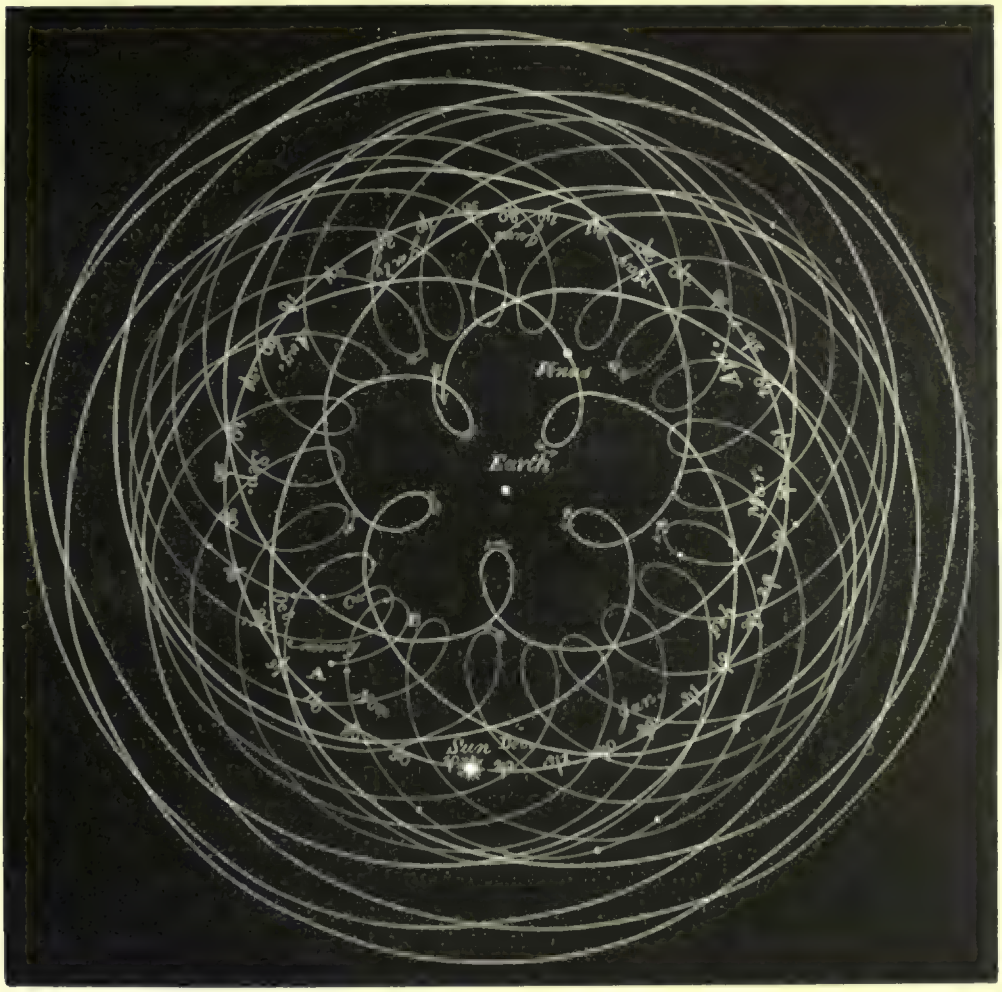


Fig. 114.—Showing the paths of Venus and Mercury, relatively to the Earth, supposed to be at rest.

(367.) Whatever epicyclic system be adopted, the real relative motions of the planets with respect to the Earth must be accounted for. These, whether deduced from observation or calculated from the known motions of the several planets, are as follows :—

The movements of Mercury and Venus are shown in fig. 114. Twenty-three synodical revolutions of Mercury, and five of Venus, are shown as drawn by astronomers two centuries ago.

(368.) Fig. 115 represents the geocentric movements of Saturn and Jupiter, in sufficient number to complete the circuit of the heavens. One geocentric revolution of Mars is also shown. These curves are taken from an old drawing, attributed to the elder Cassini. The loops of Saturn beginning near the top somewhat to the right are dated in the original 1708, 1709, &c., to 1737. Those of Jupiter beginning on the left are dated from 1708 to 1720. The path of Mars ranges from 1708 to 1710.

(369.) I have thought it well to give a further illustration of the geocentric movements of Mars, showing the motion of the planet, relatively to the Earth, from 1875 to 1892, in Plate V. The student will find it well worth his while to study figs. 114 and 115, and the looped circuits of Mars in Plate V., with some care. He will learn more, from such study, respecting the actual problems which astronomers had to deal with in endeavouring to establish the true theory of the Solar System, than from any verbal accounts of the planetary movements—even when such accounts are accurate, which is seldom, indeed, the case. In particular, the figure in Plate V. should be carefully studied, as it is the only one of the five sets of looped circuits in which the eccentricities and inclinations are taken into account. In the figure of Plate V. they are not only taken into account, but their effects are exactly presented. To an eye placed at E, the path of Mars during the nine synodical revolutions illustrated would appear precisely as they actually are, if he is supposed to pass from point to point along the looped course, in intervals of ten days, in the direction shown by the arrows; passing however, above and below the plane of the Earth's orbit, supposed to be represented by the paper, attaining his greatest distance above and below that plane where the marks ↥ and ↧ are set (the upright of each mark indicating the exact range of his northerly and southerly departures from the plane of the ecliptic), and crossing that plane ascendingly or descendingly at the points marked respectively ☊ and ☋. Every point marked by a dot on the orbit of Mars has been separately determined, either by calculation or construction, as the case required.[1]

(370.) In dealing with all these pictures of planetary movements as viewed from the Earth, it is necessary to notice that the part of the loop for each planet which comes nearest to the Earth indicates where the planet, if exterior, is in opposition, if inferior, is in inferior conjunction with the Earth. Hence, in the cases pictured in fig. 114 the sun is at the part of his path nearest to Venus or Mercury at the moment when the planet is at the innermost part of a loop shown in the figure; and he is again in the part nearest to the planet when the planet is at the part of its path most distant from the Earth. But in the cases illustrated in fig. 115, and in Plate V., different conditions hold. The sun is at the part of his path most distant from the planet, or opposite to it in the figure, when the planet is traversing the portion of his loop nearest to the Earth; while, on the other hand, the sun is on the same side as the

[1] The student will find a number of attractive geometrical problems suggested by this figure; as, for instance, the determination of the locus of the points marked M where Mars is in perihelion, or of the points marked M′ where he is in aphelion; the loci, again, of the points marked ♂'s ♈ and ♂'s ♎, where the planet passes his vernal and autumnal equinoxes respectively. The geometrical investigation of the figure of the curves traversed by Mars will be found a somewhat difficult task, since these are not produced like true epicyclics by uniform motion in a circle whose centre travels uniformly round another circle, but by variable motion in an ellipse whose focus moves also with variable motion in another ellipse. The distances and positions of the planet, however, as given in our *Ephemerides*, depend in reality on the solution of this problem, with others of much greater difficulty and complexity.

planet, and at its nearest to the planet's path, at the time when the planet is at the part of its path most distant from the Earth.

(371.) Since the planets' apparent paths form loops on the star-sphere, the curves shown in figs. 114, 115, and Plate V. do not lie on the same plane; for if they did the tracks as projected on the celestial sphere would lie on the same great circle. The actual range of each planet from the plane of the ecliptic (which is supposed always to be that of the figure) is shown in the pictures of the planetary orbits. (Plates IX. and X.)

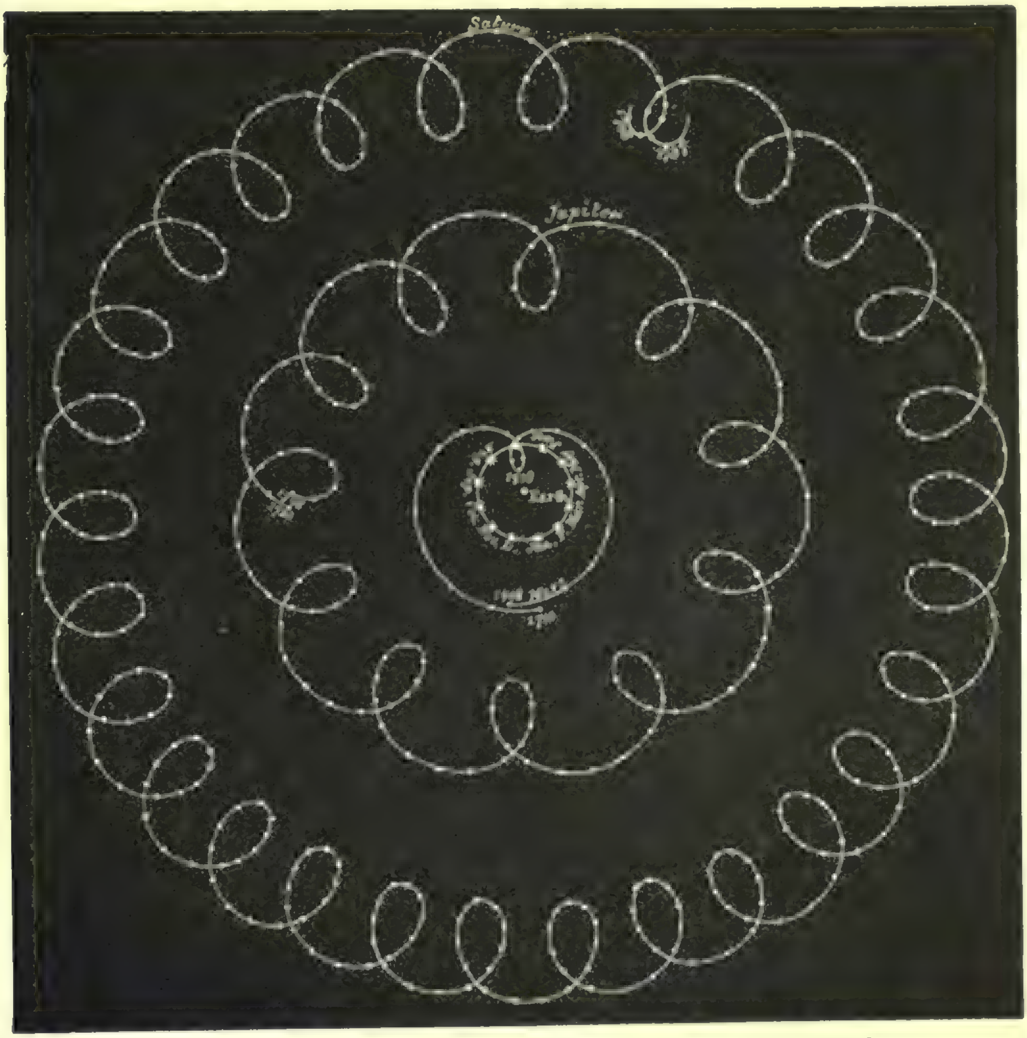


Fig. 115.—Showing the paths of Saturn, Jupiter, and Mars around the Earth, supposed to be at rest.

(372.) The ancient astronomers, though they fully recognised that the paths of the planets as viewed from the Earth are not re-entering, seem to have entertained the idea that originally the curves must have been perfect. The re-entering paths which the planets would trace around the Earth if their periods of circuit were commensurable with hers (or with the sun's supposed motion around her) are shown in the eight figures of Plate VI.

(373.) These eight curves are all actual epicyclics, traced out by the mechanical combination of two circling motions.[1] If the planetary paths were circular around the sun as centre, and all in one plane, their motions with reference to the Earth would be true epicyclics, even though none of the periods of revolution were commensurable with the Earth's ; but they would not be re-entering curves. Since, as a matter of fact, all the orbits, including the Earth's, are more or less eccentric, and all inclined somewhat to the plane of the Earth's motion, the ancients could not get even such comparative simplicity as would come from combining in a single diagram such curves as are shown in figs. 114, 115, and Plate V. (substituting non-re-entering for re-entering curves). Extra circling motions had to be suggested for each planet, besides oscillating movements of the planes of circling, to correspond with the diverse inclinations of the planets. We cannot wonder if the result seemed overwhelming in its complexity, with its

> Centric and eccentric scribbled o'er
> Cycle and epicycle, orb in orb.

[1] These pictures, taken from my *Geometry of the Cycloids*, are due to the mechanical and geometrical skill of my friend Mr. Henry Perigal, who kindly lent me these blocks, and many other charming mechanical cyclics for that treatise. I may remark, however, respecting the third figure of Plate VI., representing the geocentric orbit of Mars, on the assumption that the period of Mars is to that of the Earth as 2 to 1, that a better result would have been obtained if the ratio selected had been either 15 to 8, or 32 to 17.

CHAPTER IV.

THE TRUE MECHANISM OF THE SOLAR SYSTEM.

(374.) Whatever views the earlier astronomers of Egypt and Babylon may have formed respecting the motions of the planets, it is certain that for more than a thousand years before the time of Copernicus (born 1472 or 3, died 1543) the epicyclic system of Ptolemy had prevailed as the true system of the universe. Artificial and fanciful though it was, with movements taking place about non-material centres, with oscillations of cyclic planes about imaginary axes, and with continually growing complexity among the devices necessary to 'save appearances' as observations became more numerous and more exact, it was easier to understand than that Egyptian system which set the sun as the centre of the planetary motions, and (probably) the Earth as the centre round which the sun travelled. Nor was it likely to be overthrown by any mere change in the assumed position of the chief centre of the celestial motions.

(375.) The idea commonly entertained that Copernicus, by setting the sun in the centre of the Solar System, swept away at once the cyclics and epicyclics of Ptolemy, introducing simplicity where before there had been ever-growing complexity, is entirely erroneous. When as yet observations were comparatively rough and imperfect, an astronomer would have had some chance of establishing a system of the universe which placed the sun at the centre; for the more obvious peculiarities of planetary motion, the alternating advance and retrogradation of the planets, with the relative mean range of the arcs of either sort, and the observed positions of the stations, could be at once explained by setting the sun in the middle of the system, and the planets, including our earth, in motion around him, all circling in one direction. But the very complexity which had resulted from the erroneous assumption that our earth is fixed at the centre of the Solar System prevented the astronomers from placing the sun at the centre with any chance of 'saving appearances' by a simple system of movements. In dealing with a system of circling movements, the centre of motion may be supposed set anywhere within the

system, by adding to the whole system, or taking away from it, a properly selected circling motion, and so reducing to rest some selected point. But only one motion can thus be added to or taken from the several members of the system in making such a change. And in the case of our solar system, as interpreted by Ptolemy, each orb had many movements. Copernicus could assume the sun at rest by assigning to the whole system a circling motion equal to that which the sun had been supposed to have around the Earth, but in the opposite direction; and it was an attractive feature of his theory that by the same change which thus took away from the sun its imagined motion around the Earth, Copernicus was taking away from every one of the planets the same circling motion, and giving that motion only to one orb, our earth, which had before been supposed to be at rest. But all the planets required still a number of cyclics, eccentrics, and oscillations, to account for the apparent movements still left unexplained.

(376.) In the chapters on the several planets will be found full descriptions of the actual apparent movements of advance and retrogradation shown by planets which in reality are all the time travelling the same way around the sun. Here it will be convenient to give the following comparatively simple explanation of the principles on which this alternation of motion depends :—

Suppose E e' E$'$ e (fig. 116) the Earth's path round the sun at S; P$'$ P P$''$ the path of a planet outside the Earth; and further, that the Earth travels faster than the planet. Then we might determine all the circumstances of the planet's apparent motion by taking the Earth and planet round S, according to their respective rates of motion. But it will be convenient to consider, instead, the varying effects corresponding to the relative situations of the Earth and planet. The reader will see at once that the following proof, while much simpler, is not a whit less complete than that obtained by following the motions of both bodies.

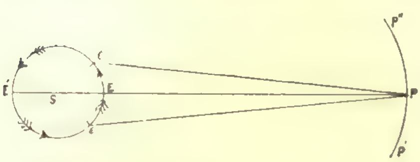


FIG. 116.—Illustrating the motion of a planet, seen from the Earth as she moves round the sun.

(377.) Consider first the case where the Earth is as at E, the planet as at P, S E P being a straight line: since the Earth is moving faster away from this line than the planet is, it is clear the planet, as seen from the Earth, must appear to lag behind, as though moving towards P$'$. Suppose the Earth as at E$'$, the planet as at P$'$; then, as the Earth and planet are moving in opposite directions from the line E$'$ S P, it is obvious that the planet will seem carried forward, or as though moving towards P$''$, with the sum of the effects due to its own and to the Earth's motion respectively. Hence, at some intermediate positions (placed, it is clear, somewhat as e and e') the planet situated as at P would seem to be at rest.

(378.) Now, both bodies moving at the rates supposed, it is obvious that their relative situations will change through all the arrangements just considered, as the

Earth, by her more rapid motion, comes again and again into conjunction with the advancing planet (i.e. to a position such as that first considered in the preceding paragraph). Hence, obviously, we have, starting from conjunction, the following successive characteristics of the planet's motion:—

A relatively slow retrogression, gradually diminishing till the planet seems momentarily at rest; then advance, gradually increasing till it attains its maximum, when it is much more rapid than the former retrogradation; thence, diminution in the rate of advance until the planet again seems momentarily at rest; and finally, a gradually increasing retrogradation up to that observed at first. And obviously, the duration of the advancing motion is considerably greater than that of the retrogradation, precisely as the arc c' E$'$ e is greater than the arc e E e'. Clearly, it only requires that the path of the planet should not be in the same plane as the Earth's, for this alternation of advance and recession to produce an apparently looped path; while, according to the position of the planet with respect to the points in which its path intersects the plane of the Earth's path, the shape of the loops will vary through all the forms described in the preceding chapter.

(379.) Again, it is clear that, so far as planets external to the Earth are concerned, the relation of the loops in size and number can be at once accounted for. Planets very far from the Earth would obviously exhibit a relatively small loop and many loops in their complete circuit of the heavens. On the other hand, planets not so far from the Earth would exhibit larger loops and fewer in the circuit, or even but one loop in more than one circuit.

(380.) As for planets within the orbit of the Earth, a very simple device will at once exhibit their relations without any new discussion. To an interior planet, the Earth is, of course, an exterior planet, and therefore she describes in appearance just such loops as exterior planets have been shown to describe. But, if one conceives an indefinitely long line passing always through the centres of the Earth and an interior planet, it is obvious that, whatever path one end of the line describes on the heavens as Earth and planet travel on their orbits, a similar path will the other end describe on the opposite region of the heavens. In other words, an interior planet describes an apparent path precisely similar to that which the Earth, as seen from that planet, would appear to describe,—or, as already shown, a series of loops resembling those described by an exterior planet.

(381.) Copernicus was not able to account for all the peculiarities of planetary motion. In fact, when once he considered minor peculiarities, he found precisely the same difficulties which had perplexed the Ptolemaists. He had got rid of the grand difficulty of the Ptolemaic theory—the conception of a set of moving points around which, instead of around real centres, the planets were required to move. But all the minor difficulties yet remained.

(382.) In dealing with the work of Kepler I have occasion to consider farther on the device by which Copernicus, introducing subordinate circles or cyclics, endeavoured to explain alike the eccentricities of the planetary orbits and the varying rates at which the planets move. The explanations of Copernicus, barely sufficient even with the astronomy of his own time, were presently shown to be entirely inconsistent with the planetary

motions as more carefully observed by Tycho Brahé and others. But it may be well to mention that the greater part of the treatise 'De Revolutionibus Orbium Cœlestium,' in which Copernicus presented his theory, is taken up with these cyclic, eccentric, and kindred devices for saving appearances. The real Copernican theory was very different from that which is now commonly called the Copernican theory. It was indeed in the main of little real value; and even for that great change which, by setting the sun at the centre, assigned their true relative positions to the Earth and sun, Copernicus gave no sufficient reason. He gave more reasons than one; but all his reasons were invalid.

(383.) But while the real Copernican theory thus presented features unlike those usually attributed to it, and altogether inconsistent with facts, there were valuable details for which Copernicus seldom receives due credit. He not only set the Earth in motion round the sun, leaving the sun at rest in the centre of the system, but he assigned to the Earth, heretofore regarded as immovable, a number of other movements by which the apparent movements of the heavenly bodies might be explained. By setting the Earth rotating in a sidereal day on its axis he explained the motion of the sphere of the fixed stars. By an ingenious combination of conical and anticonical movements he set the Earth moving so as to have her axis constantly in the same position (that is, always directed to the same points on the star-sphere), and was thus able to explain the seasons. By making the anticonical movement slightly greater than was necessary to correct the conical movement, he accounted for the precession of the equinoxes; the effect of this excess being to give just that slow reeling motion to the Earth by which the celestial equator is caused to shift with respect to the ecliptic so as slowly to carry the equinoctial points backwards among the signs.

(384.) These ideas were, it is true, no more than ideas. Our earth is not affected by a conical movement, which, if uncorrected, could keep its axis always inclined at the same angle to a line from the sun; nor is there any anticonical movement by which such conical movement is not only corrected, but so far overcorrected as to account for the precession of the equinoxes. But in suggesting terrestrial movements as a substitute for the celestial revolutions, gyrations, and nutations, heretofore believed in, Copernicus undoubtedly did much to divert astronomy from the false course on which it had so long been moving, and every step on which was removing it farther from the truth.

(385.) But if Copernicus is worthy of admiration (not such as he usually receives, yet considerable) for suggesting a part of the true system of the universe, though on erroneous grounds, Tycho Brahé deserves much higher praise for leading to the establishment of that true system by pointing out the erroneous nature of the reasoning employed by Copernicus, and by making

the series of observations through which the Ptolemaic System and the real Copernican System were alike overthrown.

(386.) Many of Tycho Brahé's objections against the views of Copernicus were doubtless unsound. He argued that if the Earth were really rotating towards the east a body thrown from the Earth would travel towards the west: 'birds which flew from their nest would be carried miles away from it before they again alighted.' His objections against Copernicus' explanation of precession were scarcely more valid. But the answers made by the earlier Copernicans to these objections, based on erroneous ideas about physical laws, were as invalid as the objections themselves. It was argued that the air, being borne along by friction, carried with it bodies flung from the Earth which would otherwise have been flung violently westwards; and other preposterous and futile answers were given to the unsound arguments of the Danish astronomer.

(387.) Other arguments used by Tycho Brahé were natural enough, and not unsound in themselves, yet unsatisfactory, as based on assumptions since shown to be mistaken. Thus he argued that if our earth really circles in an immense orbit around the sun, then the stars ought to be seen in appreciably different relative positions in different months. It was known in the time of Copernicus that the moon travels at a distance of about 30 terrestrial diameters from the Earth, and that the sun is many times farther away. On the Copernican theory, the orbit of Saturn was seen to be about $9\frac{1}{2}$ times farther from the sun than the Earth's orbit; and Tycho Brahé, like other astronomers of his time, and like his predecessor Copernicus, believed the sphere of the fixed stars to be just beyond the orbit of Saturn. Now the motion of the Earth in an immense orbit, several millions of miles at least in diameter, was so great absolutely, on the Copernican theory, that it seemed unreasonable to suppose that it would not appreciably affect the relative positions of the orbs on the enclosing star-sphere. In other words, it seemed impossible that a span of several millions of miles could be little more than a point compared with the diameter of the sphere of the fixed stars. Considering relative dimensions, which Tycho perceived to be chiefly in question—If the sphere of the fixed stars really comes just outside the orbit of Saturn, it has but ten or twelve times the diameter of the orbit assigned by Copernicus to the Earth; and a circling motion around an orbit having a diameter equal to one twelfth of an enclosing sphere (having the same centre) would in the most obvious manner affect the relative positions of stars and the relative dimensions of star-groups on the surface of that enclosing sphere. Tycho Brahé pointed out that the stars did not show a displacement amounting to so much as 2 minutes of arc, or $\frac{1}{2700}$th of a right angle, during the course of the year;

whence (mistakenly supposing the apparent discs of the stars to indicate their real size) he inferred that if the Copernican theory were true each star must be a sphere equal in diameter to twice the Earth's distance from the sun.[1]

(388.) To this difficulty the Copernicans in the days of Tycho Brahé had no valid answer. Copernicus himself had, indeed, given in the sixth proposition of his 'De Revolutionibus Orbium Cœlestium' what he took to be a proof that the sphere of the stars lies at an immense distance; but certainly

FIG. 117.—System of Copernicus.

he had no idea of the real vastness of the distances separating the stars from us. Nor was he aware that the differences of the stellar distances are enormous—many times exceeding, indeed, the distances of the nearer stars. He set them all definitely in a crystalline sphere (that is, spherical shell); and

[1] Tycho Brahé supposed the sun to travel at a distance from the Earth equal to 1,150 terrestrial radii, or to about 20 times the moon's distance. This distance, amounting to about 4½ millions of miles, is of course far short of the truth; but neither Ptclemaist nor Copernican in Tycho Brahé's time assigned a greater distance to the sun.

it may be remarked in passing that, though in his picture of the universe he assigns no spheres definitely to the planets, it is clear that he regarded each planet, our earth included, as carried round the sun by the motion of its own crystal sphere.

(389.) Tycho Brahé recognised the force of the argument based by Copernicus on the simplicity of his system. He saw also the objection against the Ptolemaic theory that most of the motions took place around imaginary

FIG. 118.—System of Tycho Brahé. The dotted lines show how the Ptolemaic System gave the same apparent motions in the case of any given planet, but in a more artificial manner.

centres. He advanced a new theory, or what at least was a new theory in his day, and independently thought out by him, which was opposed neither by the mechanical and optical objections justly urged by him against the theory of Copernicus, nor by the even more obviously striking objections against the Ptolemaic theory. His theory was :—(1) That the stars all move round the Earth, as in the Ptolemaic System ; (2) that all the planets except the Earth, move round the sun as in the Copernican System ; (3) that the sun

and the orbits in which the planets are carried round him are carried around the Earth.

(390.) It may be well to compare the three systems together directly, assuming for convenience of comparison that the real planetary paths round the sun are circles uniformly described, each in its proper period, by the several planets. It is in this way that the comparison is usually made; and though in reality the relative validity of the different systems is seriously affected by this method of comparison, yet it is at least initially convenient.

(391.) 1. In the Copernican System, we have the sun at the centre, the Earth and all the other planets travelling around him at different distances, the sun being central within the sphere of the fixed stars, which surrounds all the planetary orbits or spheres (fig. 117).

(392.) 2. To conceive the system of Tycho Brahé, imagine all the moving bodies inside the sphere of the fixed stars in the Copernican universe to be carried round in such a way that the sun, S, describes a circle around the Earth, E, equal to that which the Earth describes around the sun in the Copernican theory, in the same time but in an opposite direction. Thus will the Earth be reduced to rest, but all the other motions will take place relatively to her precisely as they did before the change. The only other change necessary to transform the Copernican to the Tychonic System is the shifting of the sphere of the fixed stars so that its centre shall be at the centre of the earth, E, as now fixed by the imagined change. We have then the system pictured in fig. 118.

(393.) 3. The Ptolemaic System gives precisely the same relative movements as the Tychonic System, but in a less natural, in fact in a mechanically unthinkable manner. The Earth, as in the Tychonic System, is at the centre; each planet revolves in a year in a circle (equal to the Earth's real orbit about the sun [1]) around a point which travels in a circle around the central Earth, this circle being equal to the real orbit of the planet, and described in the same time. Fig. 118 serves to illustrate the effect of this in the case of the planet Jupiter. E C is equal to S J, the distance of Jupiter from the sun, and *c* C *c′* is part of a circle round S as centre. Along this circle the point C travels in Jupiter's period, while round C the planet itself travels in a year in the orbit J *j j′*, equal to the orbit of the Earth round the sun. It is obvious that the motion of the radius E C round E, in company with the motion of C J round C, must give to J precisely the same motion around E as the synchronous motion of S J (equal to E C) around S, and of E S equal to C J around E. One pair of motions gives the diagonal of a parallelogram having adjacent sides E C and C J; the other gives the diagonal of a parallelogram having adjacent sides E S (parallel and equal to C J) and S J (parallel and equal to E C). But the latter parallelogram is the same as the former, and E J, the line from the central Earth to the moving planet, is the same on either system.

(394.) Clearly, the Tychonic System has an immense advantage over the Ptolemaic

[1] This equality is not essential to the Ptolemaic System, regarded merely as a device for saving appearances. It suffices that the radius of the travelling orbit, or *epicyclic*, should bear to that of the fixed orbit, or *deferent*, the same proportion that the radius of the Earth's orbit bears to the radius of the orbit of the planet. But making all the epicyclics equal simplifies the Ptolemaic System.

System in setting the real sun S instead of the imaginary point C as the centre of one of the motions (the real earth E being the centre of the other). In the time of Tycho Brahé the Tychonic System had an equal superiority over the Copernican, in getting rid of mechanical difficulties entirely insuperable according to the mechanical ideas which prevailed in Tycho Brahé's time and were held by all Copernicans, including Copernicus himself.

(395.) If any doubt could remain as to the high position which should be assigned to Tycho Brahé in astronomy, it should be removed when we consider that to him we owe the observations by which Kepler was able to recognise the real nature of the movements taking place within the Solar System—whether round the sun as sole centre, or round the sun as himself revolving once a year round the Earth.

(396.) Here again we have to strive to remove strange misconceptions of the work which Tycho Brahé and Kepler had to accomplish. The idea is commonly entertained that the orbit of the planet Mars, to which, for obvious reasons, both Tycho Brahé and Kepler directed chief attention, has an elliptic figure similar to that shown in the figures commonly given in explanation of the way of drawing ellipses, and of the Second Law of Kepler, presently to be considered. Had such been the case, there would have been variations of velocity and of distance which would have made both the observational work of Tycho Brahé and the work of comparison undertaken by Kepler comparatively easy.

(397.) It may be well for the student who wishes to appreciate the work of these two great astronomers at its right value to examine carefully fig. 119, which represents the true forms, dimensions, and relations of the orbits of the Earth and Mars, and the real motions of the two planets in these orbits in ten-daily periods. M is the perihelion, M′ the aphelion of the orbit of Mars, C its centre; and the path is appreciably circular in shape, notwithstanding the measurable, and indeed obvious, amount of the eccentricity S C. E is the perihelion, E′ the aphelion, and *c* the centre of the less eccentric orbit of the Earth; and here, though S *c* is a measurable distance, or the eccentricity obvious, the ellipticity of shape is less than that of Mars, and quite inappreciable on the scale of fig. 119.

(398.) It will be observed, however, that though the motion over any small arc in the neighbourhood of the perihelion, M, of Mars is not very strikingly different from the motion in the neighbourhood of the aphelion, M′, the difference is recognisable; while when the motion is considered for periods of forty or fifty days on either side of M or of M′, the difference becomes considerable. Taking the observations of Tycho Brahé during several successive oppositions of Mars, and remembering that these were made from the Earth, E, moving in an orbit itself not exactly determined, and with velocities varying according to a law (as yet not ascertained) round the sun S, it will be seen that Kepler had before him a task of considerable difficulty when he strove to determine the exact nature of the path of Mars.

(399.) It may be interesting to consider one among the nineteen ways in which Kepler (adopting devices suggested originally by the ancient astronomers, but developed

by Copernicus) strove to explain the observed motions of Mars, which we may regard as represented by the relations shown in fig. 119, since these, though exact enough for such a drawing, are necessarily but a rough presentation of the true motions.

(400.) Let us consider what Kepler had really to explain.

Suppose A B A′, fig. 120, to represent half of the real orbit of Mars around an eccentric point S, C being the centre of the orbit, appreciably circular *in shape.* (For the ellipticity of the half-ellipse A B A′, or the amount by which C B is less than

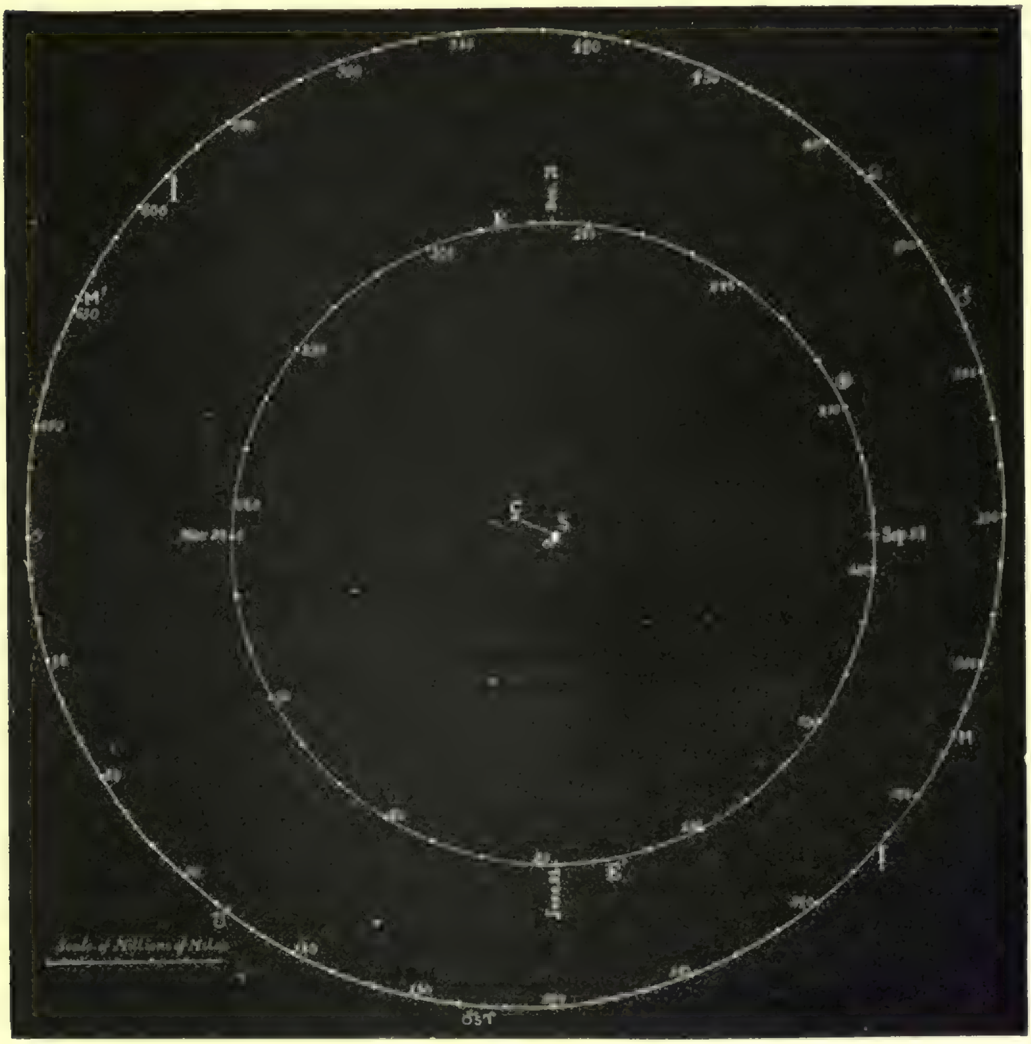


FIG. 119.—Orbits of Mars and the Earth, showing the exact eccentricity of each orbit, and the motion of each planet in ten-yearly periods. Perihelion of the Earth at E ; of Mars, at M.

C A, is determined by noticing that B S—not drawn in the figure—is equal to C A ; and it is obvious that if C S is small compared with C A, as it is in the orbits of all the primary planets, a circular arc described with centre B, and distance B S, will meet B C produced in a point very close to C.)[1]

[1] In the case of Mars, C S is about one-eleventh part of C A ; and Mercury has a larger eccentricity. If C S were exactly one-eleventh of C A, C B would bear to C A the same proportion that the square root of 120 bears to the square root of 121, or nearly the same that 240 bears to 241.

(401.) Now it had long been known that Mars moves most rapidly at A, and least rapidly at A′; and that when he has passed through a fourth of his periodic motion from A he has arrived at a point *b*, in advance of B, the quarter of his orbit, by an arc B *b*, nearly equal to the eccentricity C S. Here, then, were the primary peculiarities

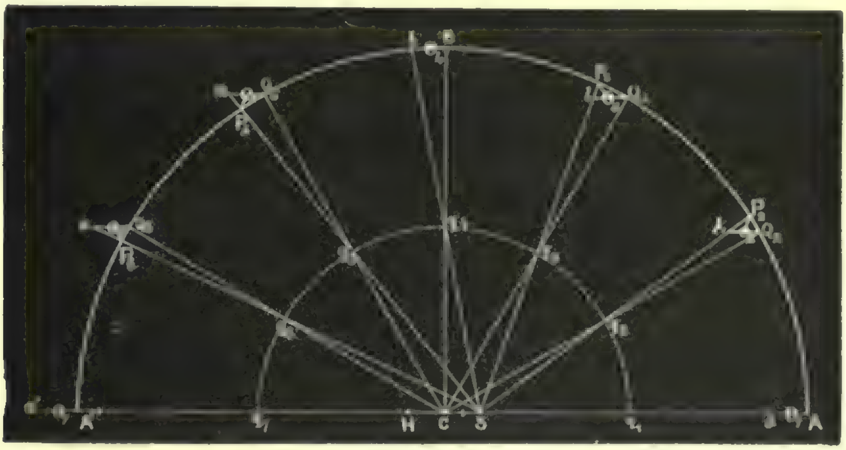


Fig. 120. — Illustrating Kepler's study of the motion of Mars.

to be explained by the use of some devices of epicyclics and eccentrics with uniform circular motion, if such might be.

(402.) Now if we strike out a circle around *c* (fig. 121) as centre, of which $O_1\,O_4\,O_7$ is the half, and set Mars travelling on a small circle whose centre moves uniformly on $O_1\,O_4\,O_7$, it is evident that, by making Mars start from some point A on the prolongation of $c\,O_1$ and travel twice round in his small circle, while its centre

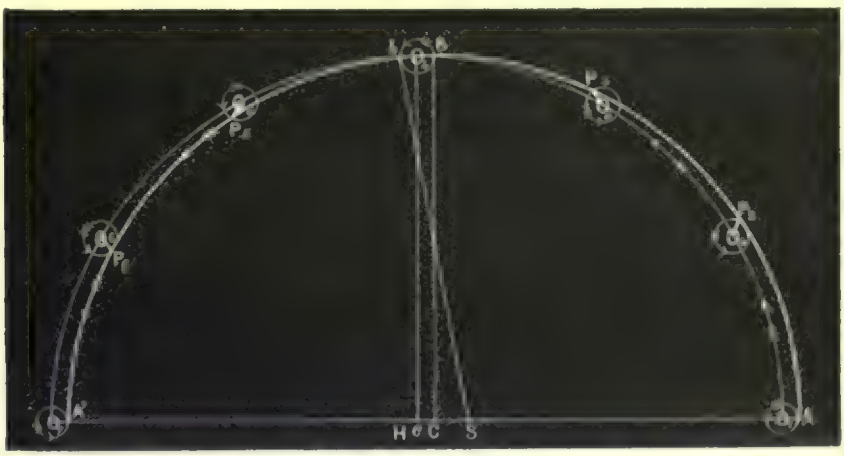


Fig. 121.

travels once round the large one, we shall have Mars at a point, as *b*, in advance of O_4 (that is, $b\,O_4$ will be parallel to *c* A) after a quarter of a revolution in the large circle, and therefore half a revolution in the small one. Also, another quarter-revolution in the large circle will bring Mars, after another half-revolution in the small circle, to A′, a point on $c\,O_7$. The rest of the circuit on the large circle will bring

Mars to A after motion on a half-orbit exactly similar to and symmetrical with the half-orbit A b A′. Moreover, it is clear that C, the middle point of A A′, will lie one radius of the small circle from c, towards A. Hence, if we draw the perpendicular C B we obtain a line touching the small circle at B, $b\,O_4$ B being a diameter of the small circle, parallel to A′ A. Therefore, obviously, if we are to reproduce the observed motions of Mars so far as they have been hitherto considered, we must have b B in fig. 120 equal to b B in fig. 121, or, in other words, the radius of our small circle equal to half the eccentricity C S as shown in fig. 120. This eccentricity we repeat, therefore, in fig. 121. By using an epicyclic of this size, round which Mars circles twice while the centre of the epicyclic goes once round the larger circle, we obtain the orbit A $P_2\,P_3\,b\,P_5\,P_6$ A′, the points thus lettered being traversed while the centre of the small circle is traversing the points $O_1\,O_2\,O_3\,O_4\,O_5\,O_6\,O_7$. This was the path obtained by Copernicus (see Book V. of his 'De Revolutionibus'). He called c S the eccentricity, because this was the eccentricity of the circular path traversed by the centre of the small circle; but C S was none the less the actual eccentricity of the orbit obtained for the planet. (Copernicus applied the method to all the planets, the Earth included.) The path which thus gave the planet very nearly its right place at the quarter-periods of the orbital circuit was not an ellipse, nor was it described according to the law subsequently discovered by Kepler. But with the observations available to Copernicus, the agreement, even in the case of Mars, was sufficiently near to 'save appearances.' We shall see this presently when we consider the first two laws of Kepler. Noting it, we are compelled to recognise, first, the ingenuity of Copernicus; next (and I think pre-eminently), the observing skill of Tycho Brahé, who, without the telescope, could make observations of Mars sufficiently exact to show that this epicyclic solution of the problem of the planet's motions must be rejected; and thirdly, the amazing patience of Kepler, who, though brought so near a solution by the ingenious device of Copernicus, and thus led to believe that by minor epicyclic and eccentric contrivances he could explain the motions of Mars, tried these one after the other till he had tested no fewer than nineteen before at last arriving at the conclusion that he must give up the mere combination of uniform motions in eccentric circles and epicyclics, and try some entirely new arrangement.

(403.) It was natural that Kepler should think of the ellipse—a curve which, unlike the circle, may be said to have inherent eccentricity. If we take two points, H and S (fig. 120), equally distant from C, and A B A′ is a curve such that the sum of the distances of each point from H and S is constant: then is A B A′ half of an ellipse, having A A′ as its greater axis, C B as half its minor axis, and H C or C S as its eccentricity.

(404.) So soon as the ellipse is thought of for a planetary orbit we see at once that the idea of uniform motion is no longer probable in itself. For the arc of an ellipse is a curve of constantly varying curvature, whose length, measured from any given point, does not increase uniformly with any simple law of angular motion around any centre or series of centres. Kepler had already learned that the velocity at A and the velocity at A′ (fig. 120) are appreciably as $R + 2r$ and $R - 2r$ (where R and r are the radii of the large circle and the epicyclic respectively); that is, taking C H equal to C S, these velocities are as H A to H A′ or as S A′ to S A, or inversely as the planet's distance from the sun at these points where the motion takes place at right angles to the line joining the sun and planet. Hence equal arcs near A and A′ are described in times proportional to the distances S A and S A′.

(405.) Kepler immediately inferred, though quite erroneously, that the times of describing equal arcs in all parts of the orbit are proportional to the distance from S. It would follow from this that if any arc, as *a h* (fig. 122) of a planet's path is divided into equal small arcs *a b*, *b c*, *c d*, *d e*, &c., and we draw the lines S *a*, S *b*, S *c*, S *d*, &c. (properly these lines should be drawn to the middle of the arcs *ab*, *bc*, &c.; but, the arcs being very small, this is a point of no importance), the time of traversing the arc *a h* is proportional to the sum of the lines S *a*, S *b*, S *c*, &c., and therefore to the mean value of these lines. In Kepler's time the determination of this mean value in the case of an elliptic arc was a practically insoluble problem. He was led to suppose, and again erroneously, that the sum of the lines S *a*, S *b*, S *c*, &c. would be always proportional to the area *a* S *h*. The idea is evidently incorrect (since *a b*, *b c*, *c d*, &c. are obviously not inclined at a constant angle to the respective lines S*a*, S*b*, S*c*, &c., which would be necessary to make the areas of the small angles *a* S *b*, *b* S *c*, &c. proportional to S *a*, S *b*, &c., and their sum, the area *a* S *h*, therefore proportional to the sum of the lines S *a*, S *b*, &c.); so that one is disposed to marvel the more at Kepler's success in mastering the problems he had attacked. By a piece of singular good fortune, the error he had made in assuming the times along equal small arcs to be proportional to the distance, was exactly corrected by the error he presently made in assuming that the sum of lines, as S *a*, S *b*, &c., drawn to equal small arcs *a b*, *b c*, &c., is always proportional to the area *a* S *h* between the extreme lines S *a*, S *h*, and any arc *a h* divided into equal small arcs.

FIG. 122.—Illustrating Kepler's discovery of his Second Law.

(406.) Thus was astronomy led first, by testing and rejecting a number of combinations of eccentrics and epicyclics, to—

KEPLER'S FIRST LAW.—*Each planet moves in an ellipse round the sun, situate in one focus of the ellipse.*

(407.) And next, through the correction of one serious error by another, which fortunately was equally serious, astronomy was led to—

KEPLER'S SECOND LAW.—*Each planet moves with such velocity in its elliptical orbit, that the time of traversing an arc is always proportional to the area enclosed between the arc and two straight lines drawn from the sun to each extremity of the arc.*

(408.) The student of Kepler's work is by no means to imagine that because he thus made mistakes, working through error up to truth, his work was not as deserving of applause as it has commonly been held to be. Doubtless he often justified the caution which the more philosophic Tycho Brahé had addressed to him in the beginning of his career—to inquire into causes from their effect, instead of entering into fanciful speculations. But to this, the great fault of Kepler's intellect, science owes almost as much as to the work he based on sound methods of research; for in his day science

needed one set of ill-founded speculations to shake men's faith in a set of speculations equally ill-founded which had been long adopted as sound theories. Moreover, Kepler was never so in love with even his wildest speculations as to accept them without giving them the most thorough investigation of which in his day they admitted. Where he could not apply proper tests he often continued to hold and promulgate absurdly incorrect ideas. But where he could, he either showed that his speculations corresponded with observed facts, or, failing that, he abandoned them.

(409.) Thus it was with Kepler's Second Law. Not only was it a mere speculation as he first thought of it, but it was a speculation suggested by utterly erroneous reasoning. But he put it to the test—found that the planets actually do move in accordance with this law, which, so far as the best observations in his day were concerned, 'saved appearances' perfectly. Hence he properly published the law as sound; and would doubtless have continued to do so even though it had been shown him that the planets do not traverse equal small arcs in times proportional to the distance from the sun, and that the sum of lines to a number of adjacent equal small arcs along any arc of a planet's orbit is not proportional to the area between that arc and the bounding-lines from the sun to either end of it.

Let us consider the actual nature of Kepler's Second Law, and some of the simpler results following from it.

(410.) We shall presently have examples of such eccentric and obviously elliptic orbits as are commonly pictured in illustrations of Kepler's first two laws. Here we may preferably limit our attention to an orbit of small eccentricity, such as the half-orbit of Mars pictured in fig. 120. S is the sun, A B A′ one half of the planet's orbit, A C A′ the major axis, C the centre, C B at right angles to A A′ the half minor axis. The planet, starting from A, with its greatest velocity passes in one-twelfth of its periodic time over an arc A P_2 such that A S P_2 is one-twelfth of the area of the complete path, and one-sixth of the half-orbit A B A′. In the next twelfth of its period the planet passes to P_3, such that P_3 S P_2 is equal to P_2 S A; in the next to b; then to P_5, P_6, and A′, the sectors b S P_3, P_5Sb, P_6 S P_5, and A′ S P_6 being all equal to P_2 S A,—that is, to one-twelfth part of the ellipse, very nearly circular in shape, traversed by the planet around the focus S.

(411.) We can now show how nearly the orbit obtained by Copernicus and Kepler (shown in fig. 121) corresponds with the true path A B A′. Regard this path as circular, which is very nearly true, and from C, its centre, draw the equidistant radii C Q_2, C Q_3, C B, C Q_5, and C Q_6, dividing the arc A B A′ into six equal parts. Hence the sector P_2 S A is equal to the sector Q_2 C A; and therefore if C Q_2 and S P_2 intersect in q_2, the triangle C q_2 S is equal to $Q_2 q_2 P_2$. Now if we draw $Q_2 k$ parallel to C S, and neglect the triangle k P_2 Q_2 as small compared with Q_2 q_2 P_2, we see that we have the triangles C q_2 S and k q_2 Q_2 equal if q_2 is the bisection of C Q_2. And we get a similar result in the case of the other equal triangles C q_3 S, P_3 q_3 Q_3, &c. We get the inter-

sections of every such pair of lines as C Q_2, S P_2, &c. at the points q_2, q_3, q_4, &c. on the circumference of a circle round C as centre, and having the radius C q_1 equal to half C A. Also, we notice that $k\,Q_2$, $l\,Q_3$ b B, $m\,Q_5$, $n\,Q_6$, &c. are each equal to C S; take, further, a A equal to a' A′ equal to C S: then bisect each of these parallels in O_1, O_2, O_3, &c. to O_7, joining O_2P_2, O_3P_3, &c., and we obviously have the points O_1, O_2, O_3 in fig. 120 precisely agreeing in position with the points similarly lettered in fig. 121, while O_1A, O_2P_2, O_3P_3, &c. correspond almost as exactly both in length and position with the lines similarly lettered in the second figure. The angle $k\,P_2\,Q_2$, for example, is appreciably a right angle, or P_2 a point on a circle having $k\,O_2\,Q_2$ as diameter; and the angle $P_2\,O_2\,Q_2$ at the centre is double the angle $P_2\,k\,Q_2$ at the circumference of this circle—that is, double the angle P_2 C A. Hence the curve AP_2 bA′, of fig. 121, is as near an approach to the true ellipse A P_2 P_3 A′ of fig. 120 as we obtain by that first approximation to the true elliptic motion which consists in neglecting the area of the small triangles $k\,P_2\,Q_2$, $l\,P_3\,Q_3$, &c.[1]

(412.) Encouraged by the success he had thus obtained, Kepler sought further to ascertain if any law connected the periods and distances of the planets. He had the table of planetary distances and periods, which is given presently, to consider. From this table he had to ascertain if any numerical or geometrical relation connects the distances and periods. It seems singular that Kepler should have been so long perplexed by a problem which at a first view seems so simple. But we must remember :—First, that he was personally apter to think of fanciful than of simple relations; and secondly, that in his day, when as yet logarithms were unknown, the work of calculation for testing any theory relating to numbers was by no means so easy as it is in our time. Thus, even when the true solution had so far suggested itself

[1] In fig. 121, call the angle O_2 C O_1 ϕ (it corresponds with the mean anomaly in elliptic motion); put $c\,O_1 = a_1$, and C S $= e\,a$,—so that, as usual, we may have the ratio of C S to C A represented by e, the eccentricity. Then if we take C as the origin, C A and C B as axes of x and y, we obviously have:

$$x = a\left(\cos\phi + \frac{e}{2}\cos 2\phi - \frac{e}{2}\right) \quad (1)$$

$$y = a\left(\sin\phi + \frac{e}{2}\sin 2\phi\right) \quad (2)$$

If, retaining the direction of the axes unchanged, we shift the origin to S, we have:

$$x = a\left(\cos\phi + \frac{e}{2}\cos 2\phi - \frac{3e}{2}\right) \quad (3)$$

$$y = a\left(\sin\phi + \frac{e}{2}\sin 2\phi\right) \quad (4)$$

and this is the approximate formula for elliptic motion in modern astronomy when e is small.

It is obvious that the values of x and y' given in (1) and (2) cannot correspond, when ϕ is eliminated, with the values of the co-ordinates of an ellipse around C as centre. Yet if e be small, so that powers above e^2 may be neglected, these values may be shown to approximate to those of elliptic co-ordinates.

Thus, we have:-

$$\frac{x^2}{a^2} = \cos^2\phi + \frac{e^2}{4}\cos^2 2\phi + \frac{e^2}{4}$$
$$+ e\cos\phi\cos 2\phi - e\cos\phi - \frac{e^2}{2}\cos 2\phi$$

$$\frac{y^2}{a^2} = \sin^2\phi + \frac{e^2}{4}\sin^2 2\phi$$
$$+ e\sin\phi\sin 2\phi;$$

whence

$$\frac{y^2}{a^2(1-e^2)} = \sin^2\phi + \frac{e^2}{4}\sin^2 2\phi$$
$$+ e\sin\phi\sin 2\phi + e^2\sin^2\phi$$
$$+ \text{neglectible terms};$$

wherefore

$$\frac{x^2}{a^2} + \frac{y^2}{a^2(1-e^2)} = 1 + \frac{e^2}{2} + e\cos\phi$$
$$- e\cos\phi - e^2 + \frac{e^2}{2}$$
$$= 1.$$

the equation to an ellipse having C as centre and axes C A ($= a$) and C B $= a\sqrt{1-e^2}$.

to him that he was prepared to look for the relations he required among the powers in the table of distances and periods, he was engaged two months and a half in ascertaining and proving the law actually existing and—one would have supposed—obvious.

(413.) It is clear from the first two columns of figures in the table given a few lines farther on that the numbers representing the periods vary in greater degree than the numbers representing the distances. Yet, that the former do not vary so greatly as the squares of the latter, is obvious on a moment's trial; for the square of Jupiter's distance is greater than 25 and that of Saturn's is greater than 81, whereas the periods of these planets are respectively but as 11·87 and 29·47. It seems a natural thought to try a power midway between the first and the second—that is, the power 3/2. To do this, all that Kepler had to do was to cube the numbers in the first column of the table and to take the square roots of the resulting numbers. Had he done this he would have obtained the series of numbers in the second column, and his Third Law would have been established: all this could have been done, without the aid of logarithms, in the course of a quarter of an hour or less.[1] What Kepler actually did, or how he was kept for so long a time from the solution of his problem, it is not easy to imagine. But at length he dealt with the task in the simple manner above indicated. He thus obtained—

(414.) KEPLER'S THIRD LAW.—*The cubes of the mean distances of the planets are proportional to the squares of the periodic times.*

(415.) The following table shows how this law corresponds with the actual relations between the planetary distances and periods as known to Kepler:—

Planets	Distance. Earth's as 1	Period. Earth's as 1	Distance cubed	Period squared
Mercury	0·387	0·241	0·058	0·058
Venus	0·723	0·615	0·378	0·378
Earth	1·000	1·000	1·000	1·000
Mars	1·524	1·881	3·540	3·538
Jupiter	5·203	11·867	140·852	140·826
Saturn	9·539	29·457	867·937	867·715

(416.) The agreement of the sets of numbers in the two last columns is practically exact; for it is as close as the actual agreement between the numbers

[1] The simplicity of the task which Kepler had before him, after he had decided to look for the relation needed in the powers of the numbers representing the distances and periods, will be seen at once if we consider that he had no occasion to deal with the whole table of distances and periods until he had recognised a law in the case of any one planet besides the Earth; and then he had only to test that law. Suppose, for instance, he had considered Mars. Then absolutely the only thing he had to do in order to ascertain whether powers were in question was to inquire what power of 1·524 gives the number 1·881. The first power is too low; the second is too high; but the power 3/2 is just right,—as is at once seen by noting the equality of the square of 1·524 to the cube of 1·881. Then the rest of the table had to be tried, giving corresponding results.

representing the distances and periods with the true proportions (which could not be represented save by an interminable array of numbers).

(417.) Kepler had thus far succeeded in ascertaining the laws according to which the planets move round the sun, so far as observation went in his time. The motion of the moon still remained unexplained. Of course it was not to be expected that the moon, which travels round the Earth, should show any correspondence with Kepler's third law connecting the periods and distances of bodies travelling round the sun. But the moon in her motion round the Earth did not obey either of the other two laws. The alternating, but on the whole advancing, movement of the moon's perigee, to say nothing of the retrogression of her nodes, sufficed to indicate a degree of divergence from the laws of elliptic motion and uniform description of areas, which convinced Kepler that here some other causes were at work modifying those unknown causes to which the laws he had recognised were due. As he recognised the moon's attractive influence in raising the tides—though so indistinctly that he imagined the moon acted upon some subterranean terrestrial spirits or humours which retired into the bowels of the Earth when the moon's influence passed away—Kepler recognised, but in a similarly vague way, that as the moon influences the Earth, so the Earth must exert influence on the moon, and even far beyond her. He also suggested that the sun's attractive influence may cause the irregularities of the moon's motion. He formed a fanciful theory of the existence of what he called a 'friendly pole' and an 'unfriendly pole' possessed by the sun, and acting alternately on the several planets so as to draw them towards their perihelia and repel them towards their aphelia.

(418.) Although many, in fact most, of the ideas suggested by Kepler in regard to the causes to which the planetary motions are due were incorrect, he undoubtedly was the first to suggest the action of attraction. Owing to his imperfect knowledge of mechanical principles, he imagined that besides some attractive force exerted by the sun on the planets, and by the Earth on the moon, there must also be some force—he went even so far as to suggest that it was something akin to soul or spirit[1]—by which the planets and the moon were kept travelling onwards; since otherwise, he supposed, they would

[1] Some assert that he spoke of it as an animal force; but the word thus translated 'animal' is akin rather to 'spiritual' as we now use the two words. We speak in these days of the animal body, forgetful of the fact that with the Latins *anima* and *animus* were definitely limited to that which is not the body, but the air, wind, breath—in fine, the spirit. In old times—as Greek and Latin names for 'soul' and 'spirit' show the 'breath' was the soul, which passed away into the upper air at death. The 'shadow' was yet a third self, whose true home was in the realm beneath the earth, towards which all shadows naturally tend. What fate might have attended a student of optics who explained shadows, or a student of chemistry and physiology who analysed the breath and explained respiration—one shudders to think. It would have been warm.

presently cease to advance, and be drawn inwards towards the centre of attraction. Kepler's ideas, however, about the attractive influence of the sun and moon were closely akin to those with which Newton started; in fact, we know from a letter of Newton's to Halley that Kepler's ideas really guided Newton towards that pathway along which he subsequently advanced to his own magnificent discoveries. Kepler points out (in his 'Astronomia Nova') that attraction alone would cause the planets to fall to the sun, and the moon to the Earth. He showed that the centre of gravity of the Earth and moon lies at a point whose distance from each is inversely proportional to its mass—which is right: but he supposed the centre of equal attraction, the neutral point between the two bodies, to lie at this centre of gravity—which is altogether wrong.

(419.) In the meantime, Galileo was investigating those mechanical questions, which began now to be much more definitely involved in astronomical researches than they had been when only the Copernican theory was under discussion. Then science had asked how the Earth could rotate, revolve, and gyrate, after the ways suggested by Copernicus, without certain consequences which were not observed to take place. Nor could Copernicus answer the difficulties thus suggested. But now, not only were these still unsolved problems at issue, but the whole question of the planetary movements was seen to depend on physical and mechanical considerations. As a matter of fact, until men perceived that the planets are actually guided on their paths by attractive influences depending on the mass (and therefore, in some degree, assuming no very wide variations of density, on the size) of the attracting orb, there was nothing to show that the sun, and not the Earth, is the centre of the planetary system, even after Kepler had established his three laws. The idea of Tycho Brahé could be applied quite as readily to the ellipses of Kepler as to the epicyclics of Copernicus. And though certainly a greater simplicity could be recognised in a system which set the Earth as one of the planets travelling round the sun, obeying as she did—when thus viewed—the third law of Kepler as well as his other two laws, yet, *per contra*, something might still be said in favour of an arrangement by which the Earth, which certainly is the centre of the moon's motion, could be regarded as the centre of the sun's motion also.

(420.) Galileo, equally by his mechanical investigations and by his telescopic discoveries, began the work of proving from physical considerations that the earth, along with the other planets, must move round the sun as centre.

(421.) Little need be said at this point respecting Galileo's telescopic work. The discovery of the globular forms of the planets, of the opacity of

the inferior planets as shown by their phases, with the inference that the superior planets are also opaque though Galileo's largest telescope power showed no perceptive variation of illumination when they are near quadratures, taught men to regard the planets as akin to the Earth. The observations of Vendelinus on the moon's position in regard to the sun when she is half-full, had shown that the sun lies much farther from the Earth than had been supposed. This method, first suggested by Aristarchus of Samos, is not susceptible of great accuracy,[1] and no reliance would now be placed on Vendelinus's result even as tending to show that the sun's distance is not less than 13,752 radii of the Earth, or roughly, 55,000,000 miles, though this is indeed far short of the truth. But in the time of Galileo this result was regarded as at least so far trustworthy that the dimensions of the planets, deduced from their apparent size in the telescope and their distances as inferred from the sun's, could be regarded as demonstrably comparable with the Earth's dimensions, in the case of the planets Mars, Venus, and Mercury, and far exceeding hers in the case of Jupiter and Saturn. The fact was thus established that the Earth is one of a family of globes, all absolutely large, yet all small compared with the sun, whose globe, judged by Vendelinus's estimate, was seen to exceed the Earth's not far from three hundred thousand times. Further, the discovery of the family of Jupiter travelling round that planet as he advanced on his orbit round the sun, and in their subordinate system fulfilling the third law of Kepler (among themselves only, of course,) as perfectly as the law is fulfilled in the planetary system, must be considered as at least highly suggestive of the physical laws to which the laws of planetary motion discovered by Kepler are to be ascribed.

(422.) It was, however, the recognition of the first two laws of motion which just at this time constituted Galileo's most important work. Axiomatic to us, these two laws were not even accepted as sound by the early students of physics. Men saw, or believed they saw, that a body set in motion comes presently to rest, unless its motion is maintained in some way. What they failed to perceive was that a body set in motion is brought to rest by external influences, and dces not come to rest of itself. Galileo recognised

Fig. 123.—Illustrating the Sun's great distance beyond the Moon's orbit.

[1] This method, already referred to in Art. 315, is further illustrated in fig. 123, E being the Earth, S the sun, M′ (where a line from S touches the moon's orbit round the Earth) the place where the moon, if spherical and smooth, would be exactly half full. The nearer M′ is observed to be to M (a point so situate that m E M is a right angle) the farther must we assume the sun to be.

that as external resistances are diminished moving bodies lose less and less of their motion. An irregular mass set moving on a rough horizontal surface is brought to rest quickly. A globe of equal weight set rolling on the same surface with the same velocity travels farther before it is brought to rest. If the surface is smooth the rolling globe travels farther still. A perfectly polished ball of ivory or some hard metal set rolling on a perfectly horizontal and very smooth plane travels very much farther. So also will a suitably shaped body set travelling on a horizontal surface of ice. Recognising that even where such a body travels farthest there is some friction, and taking into account what till his day had been generally neglected, the resistance of the air—very effective against rapid motion—Galileo was able to pass onwards in imagination to the case of a body not resisted at all either by friction against a solid surface, or by resistance akin to that of the air, and to deduce in this case the conclusion that a moving body absolutely uninfluenced by resistances either direct or frictional, would continue to move onwards for ever with unchanging velocity. He considered many other cases, however—as the motion of pendulums of different lengths, differently suspended, and swung with greater or less arcs of motion; the motion of falling bodies, of projectiles and so forth—finding always reason to believe that every change of velocity and of direction of motion is due to force exerted from without, and not to any inherent quality in the moving body tending to bring it to rest.

(423.) Galileo was thus finally enabled[1] to enunciate—

THE FIRST LAW OF MOTION.—*A body at rest will remain at rest, and a body in motion will continue to move in a straight line, with unvarying velocity, unless acted upon by some force.*

(424.) Simple as it is, this law effected a revolution in men's ideas about motion. The difficulty which had so perplexed Kepler as to compel him to call in 'celestial intelligences' to keep the planets on their way, disappeared at once and for ever. His celestial intelligences were not proved to be unnecessary: but it was at least shown that if they exist they have no work to do

[1] The first law of motion has been attributed to Kepler, because he expressed the opinion that the natural motion of bodies is uniform and rectilinear; but it is evident that he did not mean this to be understood as the first law of motion is understood. He believed that causes existing within a body tend to reduce its motion, in such sort that, however natural uniform rectilinear motion may be held to be, bodies must inevitably lose that motion unless it is in some way maintained. Galileo himself, whose genius for mechanical inquiries was far keener than Kepler's, worked round the first law for many years before he recognised its truth. His ideas on the subject in 1604 were quite erroneous. So were those in his first dialogue on the Copernican System in 1630. It was not until 1638 that, in his *Dialogues on Mechanics*, written somewhat earlier, he indicated the true nature of the motion of a body advancing independently of any impressed forces. He pictures a body moving on a plane horizontal surface infinitely extended, all obstacles to the body's motion being removed: *Jam constat*, he says, *ex his quæ fusius alibi dicta sunt, illius motum equabilem et perpetuum futurum esse.*' 'It is proved, by those things which have been said elsewhere more at large, that its (the body's) motion will be uniform and perpetual.'

in the particular way he had suggested. The difficulties raised earlier by Tycho Brahé were also seen to be in a fair way of being overcome, though something more was still needed for that purpose.

(425.) In the experiments by which he had established the first law of motion, Galileo had repeated occasion to compare the action of forces of different kinds on bodies at rest and in motion. Until his time, it had been generally taken for granted that a force produces a different effect on a moving body and on the same body when at rest. A number of phenomena seem to suggest that this is so, though the suggestion is in reality deceptive. If a ball at rest is struck in a particular direction, the full effect of the blow is recognised ; but if the same ball when advancing very rapidly is struck in the same direction, and with the same force, the blow seems much less effective : the ball may indeed seem to travel onwards with scarcely any appreciable change. A multitude of similar cases might be cited. There are others where a difference in the velocity of a moving body seems to cause particular forces to produce effects actually differing in kind. For instance, a cannon-ball placed on the surface of water sinks immediately through the water ; the same ball, when fired from a cannon, and striking the surface of the water is sharply deflected, rising from the water instead of sinking through it. Galileo succeeded in showing that in reality every force exerted on a body produces its full effect moment by moment, whether the body is at rest or in motion. It would require a volume to deal with all the experiments which he made, and with all the results (his own, his predecessors', and his contemporaries') which he investigated. His experiments on falling bodies and projectiles sufficed, however, for the complete illustration, and practically for the demonstration of the law. At first sight it seems incredible that when a falling body or projectile is rushing swiftly through the air, the action of terrestrial gravity on the body at any moment, is producing precisely the same effect as when acting on the same body just left free to fall. It seems equally incredible that a projectile moving in a boldly parabolic sweep is acted upon in its ascent and descent (apart always from difference of atmospheric resistance) in precisely the same way as a body ascending to the same height in a vertical line and thence vertically descending. Galileo showed, however, that the motion of projectiles and falling bodies corresponds exactly (when due correction is made for atmospheric resistance) with the same action of gravity at each instant on the body when rising and when falling, when moving in the vertical or on a parabolic curve, when its velocity is great or when it is small.

(426.) Galileo thus established—

THE SECOND LAW OF MOTION.—*A force acting on a body, at rest or in*

motion, produces always an effect proportional to the magnitude of the force, and taking place in the direction in which the force is exerted.

(427.) The importance of this law, not only in the interpretation of observed movements, but also in the suggestion of the nature of forces at work in producing them can hardly be overrated. It enabled astronomers at once to get rid of all the mechanical difficulties suggested by Tycho Brahé. It did more: it enabled them to regard many of Tycho Brahé's objections as in reality arguments in favour of the true theory. He had argued that a body leaving the earth's surface would be immediately left behind or to the west, if the earth were rotating towards the east; but now science could point out that when the actual motion of the body as it left the earth's surface was considered, the observed motion afterwards was precisely such as would take place whether the earth were rotating or at rest. There was no force acting to deprive the body of the motion which it had had in company with the earth's surface before leaving that surface—either of its own will, as when a bird flies from the earth, or projected upwards as a projectile. Later, indeed, it came to be recognised that bodies projected to or falling from great heights, or thrown to great distances, will be deflected in accordance with the second law, because of the slight differences of rotational rates due to differences of distance from the earth's centre; and observations of such deflections as these have been among the most effective demonstrations of the reality of the earth's rotation. But at first the great interest and value of the two laws of motion established by Galileo, resided in their explanation of more obvious phenomena, which had before seemed inconsistent with the theory that the earth has movements of rotation, revolution, and gyration. Quite early, also, the second law showed its value by pointing to the position of the attractive centre which could alone account for the circumstance recognised in Kepler's second law; while soon after it showed how the first law of Kepler proclaims the nature of the force residing at that centre—Kepler's third law serving as a general test by which both discoveries could be confirmed and established.

(428.) Although the third law of motion was not fully established until much later, by Newton, this is the proper place in which to consider it.

In the laws hitherto considered, nothing has been said about the quantity of matter in motion, or acted upon by forces impressed from without. In considering the action of such a force as terrestrial gravity on bodies near the earth, or of the sun on the planets, we are not at once led to consider the masses of the bodies acted upon, when inquiring into the amount of displacement which may be produced, or of velocity which may be generated by the attractions of those much larger bodies. If the earth, for instance, produces a certain effect on a grain or ounce of matter belonging to a mass moving

near her surface, she produces the same effect on a neighbouring grain or ounce of the same mass, and the like with all parts of the mass, in such sort that, apart from atmospheric resistance, precisely the same movements and changes of movement are produced on a large mass as on a part of it, or as would be produced on a small mass subjected to the earth's influence under the same conditions. In like manner the same effect is produced on the planet Jupiter by the sun as would be produced on the minutest mass,—even a grain or a peppercorn,—which might be set moving round the sun at the same distance and with the same velocity.

(429.) But in dealing with the relations of a system of bodies, and indeed in the exact treatment even of such cases as the action of the sun on the planets and of planets on their satellites, we have to consider the masses not merely of the bodies acting but of the bodies acted upon. In dealing with celestial problems, in fact, we have to take actions and reactions into account as carefully as in ordinary cases of terrestrial mechanism, though their nature may not seem so obvious as where we have actual strains or tensions. The law of such actions and reactions recognised, but not altogether distinctly, by Galileo, and established by the experimental researches and reasoning of Newton, may be best expressed in Newton's own way, though not (since he enunciated it in Latin) in his own words :—

(430.) Third Law of Motion.—*To every action there is always opposed an equal reaction, or, in other words, the mutual actions of two bodies upon each other are always equal and in opposite directions.*

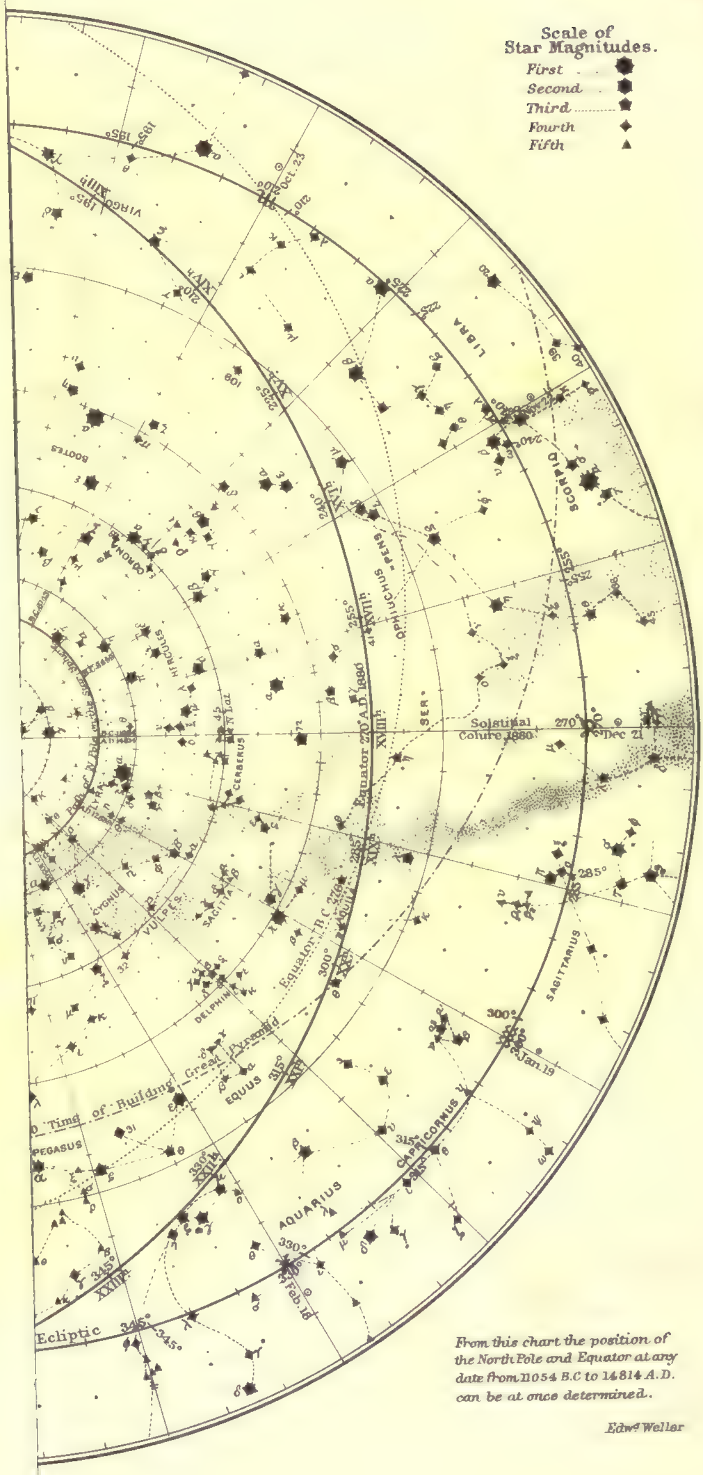
(431.) The application of this law to cases of pressure or strain is sufficiently obvious. As Newton quaintly puts it : 'If you press a stone with your finger the finger is also pressed by the stone : and if a horse draws a stone tied to a rope the horse (if I may so say) will be equally drawn back towards the stone ; for the stretched rope, in one and the same endeavour to relax or unstretch itself, draws the horse as much towards the stone, as it draws the stone towards the horse.' So again if a body strike upon another, and by its action impart motion to the other, or change motion already existing, the former will undergo a precisely equal change in its own motion in the opposite direction. The change may appear wholly different in character, but the total quantity of motion will be precisely the same in amount and opposite in direction to that imparted. If a small elastic ball strikes the surface of the earth and rebounds with almost undiminished velocity, nothing can seem much more unlike than the seeming immovability of the earth, and the rapid upward motion communicated by the earth's reaction to the ball : yet in reality the earth's imperceptible downward motion and the ball's upward rush represent precisely equal amounts of motion in opposite directions.

In the case of the mutual attractions of the heavenly bodies, we do not seem at first to have an example of action and reaction. Especially does evidence of reaction seem wanting in the case of an orb like the sun governing by its attraction the movements of a family of bodies like the planets. Indeed, there is a most perplexing problem, possibly never to be solved by man, in the question, How is the influence we call gravitation, which evidently exists, exerted without contact or connecting attachments ? But so far as the third law of motion is concerned, it is fulfilled as perfectly in this case as in the others. We cannot deal with problems in celestial dynamics, any more than with problems relating to terrestrial mechanism, without taking the equality of action and reaction fully into account. If the sun pulls a planet with a certain amount of force, the planet pulls the sun with precisely equal force in the opposite direction. The planet responds much more obviously to the pull exerted between the two bodies, because the planet is much smaller than the sun : but the total pull exerted by the smallest planet on the sun's large mass, is not less in the minutest degree than the pull of the sun on the planet's small mass. And so with every case of the mutual interaction of two orbs in space through the attraction of gravity.

(432.) When Kepler had established the laws of planetary motion and Galileo and Newton had established the fundamental laws of motion under the action of force, the way was cleared for that important advance by which physical astronomy replaced the mere study of the movements of the heavenly bodies as actually observed. As Kepler had shown that the planets move in ellipses around the sun, situate in a focus, men like Huyghens, Wren, Hooke, Newton, Halley, and others, inquired under what force or forces bodies would move in such orbits. As the second law of Kepler showed the planets sweeping out unvarying areas around the sun in equal times, philosophers asked what this might show respecting the position of the centre of force by which the planets are ruled. While lastly, the third law, indicating as it did the various mean velocities with which the planets move at their various distances, suggested a means of determining the law according to which the central attracting force varies at varying distances.

(433.) Though not actually the first-fruit, the recognition of the true nature of terrestrial gravity was the most important among the first-fruits of the just views respecting motion embodied in these laws. The study of the action of terrestrial gravity by Galileo may be regarded as the veritable beginning of those researches into the influence of attraction on bodies moving through the celestial spaces on which modern physical astronomy mainly depends. The laws of Kepler had shown what these motions are ; but the study of the laws and effects of the interaction of those bodies first indicated the proximate causes of the motions as actually observed.

VII.

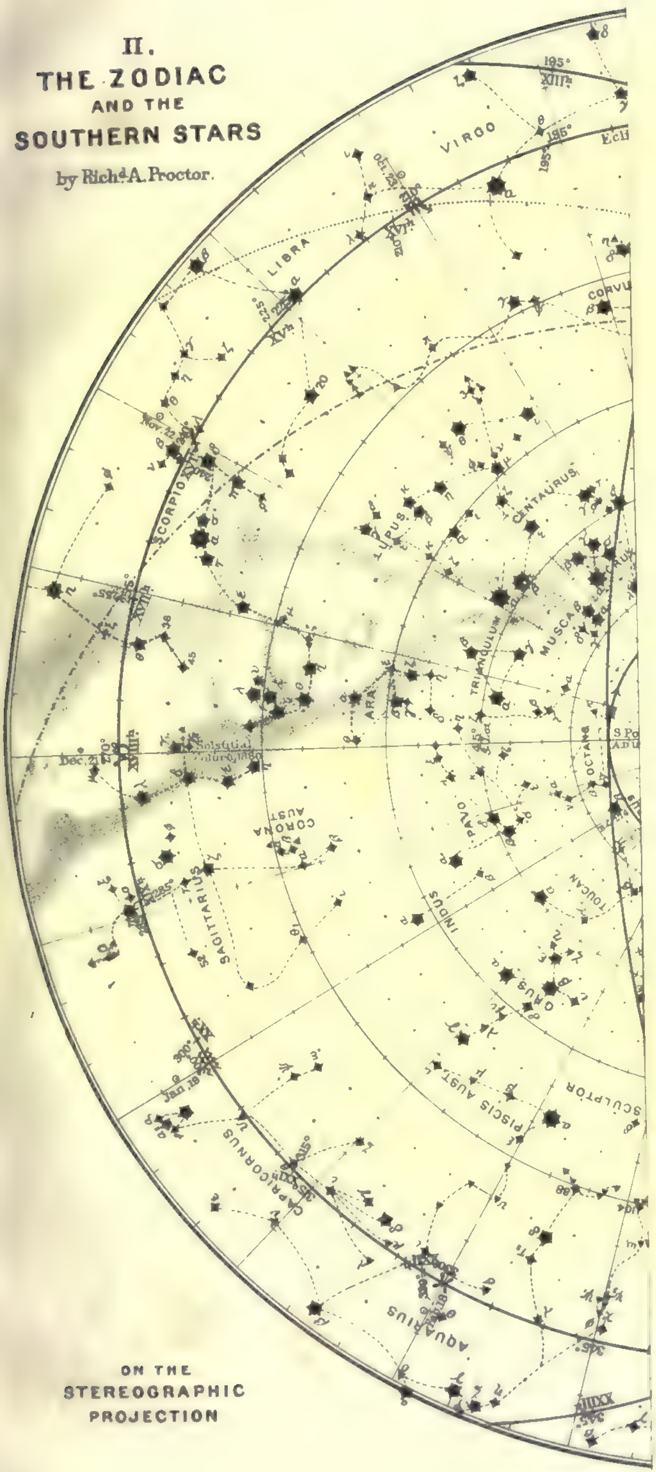


From this chart the position of the North Pole and Equator at any date from 11054 B.C to 14814 A.D. can be at once determined.

Edwd Weller

II.
THE ZODIAC AND THE SOUTHERN STARS

by Richd A. Proctor.



ON THE STEREOGRAPHIC PROJECTION

(434.) Nothing in Galileo's researches suggests that his ideas respecting the action of terrestrial gravity differed from those which from time immemorial had been held by philosophers and students of nature. We find repeated evidence in the literature of many centuries preceding his time that terrestrial gravity was supposed to be a force residing in the Earth's centre. Many such passages have been cited by persons unacquainted with the true nature of Newton's work, to show that Newton had been anticipated by Shakespeare, by Dante, by Chaucer, and by yet earlier writers. The fact that Galileo not only understood precisely how the action of gravity, as actually observed, takes place, but analysed the laws according to which falling bodies and projectiles move, would suffice to show that Newton had been anticipated--if his work on dealing with gravity had been such as many imagine and as the oft-told story of the falling apple has suggested.

(435.) Galileo recognised gravity as a force tending vertically downwards towards the Earth's centre (considering the Earth, as he did, to be a sphere). He set himself to ascertain whether the force is uniform or otherwise, an inquiry to which he applied the two first laws of mot on.

Consider, first, uniform motion :—

(436.) A body M, moving under the action of no forces, will move onwards uniformly in a straight line, as M M′, traversing equal spaces M A, A B, B C, &c., in equal

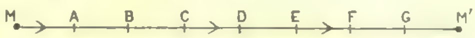


FIG. 124.—Illustrating uniform motion.

times. If the body, though moving in this line, does not move uniformly, we must infer that it is acted upon by some external force or forces—tending towards M′ if the spaces successively described increase, towards M if they diminish. If the body leaves the line M M′ on one side or the other, we must infer that it is exposed to the action of some external force or forces tending towards that direction in which the body leaves the line M M′.

(437.) Galileo recognised, as his predecessors had done, that a body let fall from rest, and moving in the vertical unless disturbed, is continually accelerated as by a force tending towards the Earth's centre ; while a body projected vertically upwards is retarded and presently brought to rest. Of course, the mere fact that the body in the former case begins to fall, and in the latter ceases to ascend, would suffice to show that there is a force tending towards the Earth's centre. It was Galileo's task to show the laws according to which this force works.

(438.) He ascertained by a series of ingenious experiments which need not here detain us, that a body let fall from rest acquires in the first second a velocity of 32·2 feet per second ; in the next a velocity of 64·4 feet per second ; at the end of the third second its velocity is 96·6 feet per second ; and so on, the velocity at the end of n seconds being n times 32·2 feet per second. Also he found that the body traverses in the first second 16·1 feet ; in the second 48·3 ; in the third 80·5 ; in the fourth, 112·7, &c. ; the spaces traversed in successive seconds being as the numbers 1, 3, 5, 7, &c. Thus the spaces described in 1 second, 2 seconds, 3 seconds, 4 seconds and so on,

from rest, are as the numbers 1, 4, 9, 16, &c. (obtained by adding to 1 the number 3, giving 4, to this the number 5 giving 9, and so on). In other words the spaces traversed from rest at the ends of the first, second, third, and fourth seconds, &c., are as the squares of the numbers 1, 2, 3, 4, &c.; that is, they are as the square of the time. A body projected vertically ascends, under gravity, according to the same law: that is to say, in the last second of its ascent it traverses 16·1 feet; in the last but one 3 times 16·1 feet; in the last but two, 5 times 16·1 feet, and so on.

(439.) The discussion of these results was not altogether simple in Galileo's time, when as yet the methods for properly dealing with continually varying quantities had not been recognised. A second is a finite period, and the distances traversed in successive seconds by a falling body are definitely as Galileo recognised them: but in any given second the body does not move uniformly; nor does it move uniformly in the tenth, or hundredth, or millionth of a second. Its velocity is varying continuously; and though by dividing up the time into exceedingly minute portions and regarding the velocity in each as uniform, we may approximate to the total distances traversed in longer periods of time, the result is only an approximation so long as the minute portions are finite. In Galileo's day mathematicians had no very clear ideas as to general rules for dealing with the very small divisions of space or time required even for such approximation. In modern mathematics we obtain at once in such cases the exact result which would follow from the subdivision of quantities belonging to space or time into parts infinitely minute. But though the idea of infinitesimal subdivision had presented itself to mathematicians of ancient Greece, and probably much earlier to the Chaldæan, Egyptian, and Indian mathematicians of old times, no definite general rules were available in Galileo's day for dealing with continuously varying quantities.

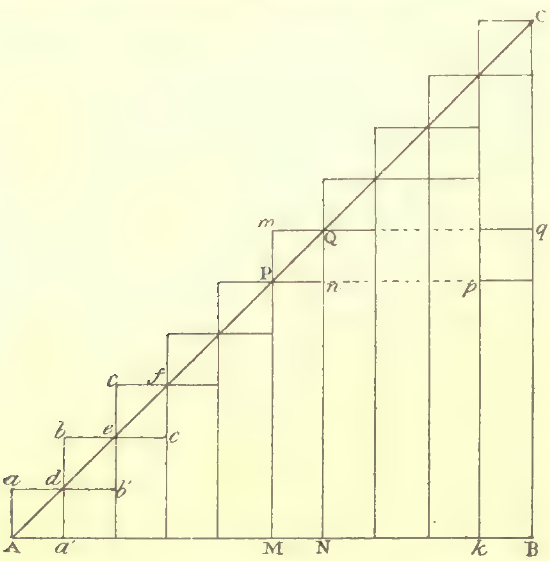


FIG. 125.—Illustrating Newton's way of measuring the action of a uniform force.

He solved this particular problem, finding that the force which produced the observed effects must be uniform —a uniform force being defined as one which generates equal velocities in equal times. But it will be convenient to give here Newton's solution of the general problem of determining the spaces traversed in given times by a body moving under the influence of a uniform force. It is only an indirect solution of the problem presented by the facts which Galileo had observed: but it is none the less a full and complete solution. Indeed, it suggests an easy direct solution:—

(440.) Represent the progress of time by uniform motion along the line A B (fig. 125), so that the small equal portions of time into which we propose to divide the time represented by A B will be represented by small equal divisions A a' . . . , M N . . . , to k B, along A B. Now a uniform force is one which all the time produces the same effects, so that in equal portions of time, large or small, it generates equal velocities. If then our body or particle

starts from rest when the time A B is beginning, and if we represent by such vertical lines as $a'd$, M P, N Q, B C, &c., the velocities acquired at the end of the times represented by A a', A M, A N, A B, &c., respectively, these lines $d\,a'$, P M, &c. will be proportional respectively to A a', A M, &c., and therefore will all lie in a straight line A D P Q C. Complete the figure as shown. It is now clear that in any small part of the time A B, as M N, the moving mass or particle supposed to have started from rest at the beginning of this time will have a mean velocity greater than that represented by M P, and less than that represented by N Q; for the velocity is continually increasing during this interval, and while M P represents the velocity at the beginning of the interval, N Q represents the velocity at the end. Hence we may represent the distance traversed during the interval M N with this velocity, by a space which while greater than the rectangle P N is less than the rectangle m N (for the distance traversed in a given time with a given uniform velocity is always proportional to the number obtained by multiplying together the numbers representing the distance and time; while a rectangle's area is also always proportional to the product of the numbers representing its length and breath). Therefore the whole space traversed in the time A B will be represented by a space greater than the area included between A B, B C, and the inner zigzag $a'd\,b'e$ &c., but less than the area included between the same lines and the outer zigzag A $a\,d\,b\,e$ &c. The difference between these areas is obviously the rectangle C k, for we can imagine each such rectangle as $m\,n$, forming part of the difference, to be slid parallel to A B into such a position as $p\,q$, there making up a component part of the rectangle C k. And this rectangle C k, having a finite height B C, and a base which is one of the small equal divisions of A B, may be made as small as we please; since we may take the divisions of A B as small as we please without at all modifying the validity of the reasoning just followed. (Thus our rectangles may be as thin as the lines P M, Q N, or a millionth part of this, or in fine indefinitely thin.) Hence since the zigzags on either side of A C will thus approach it indefinitely, we see that the area A B C represents the space traversed in the time represented by A B, by a body starting from rest, under the influence of a force capable of generating the velocity B C in the time A B.[1]

(441.) If now we call the uniform force f (it is generally represented by g in the case of gravity); the time, t; the velocity generated in this time, v; and the space traversed in this time, s; we have, first, since the force is uniform—

$$v = f\,t = \mathrm{B\,C}, \text{ according to our method of representing velocities;} \qquad (1)$$

and then, since triangle A B C = $\frac{1}{2}$ A B . B C,

$$s = \tfrac{1}{2}\,t\,.\,f\,t = \tfrac{1}{2}\,f\,t^2 \qquad (2)$$

$$\text{and } t = \sqrt{\frac{2\,s}{f}}.$$

(442.) Or we have the following laws for bodies moving under the action of a uniform force—

(1) *The velocity acquired in a given time is proportional to the time.*

(2) *The space traversed in a given time is proportional to the square of the time.*

[1] It may be noticed that the areas thus described by the body in the successive small intervals of time represented by A a', &c. are represented by triangles A da', &c., having A d, A e, A f, &c. as hypothenuses, whose areas are obviously as 1, 3, 5, &c. Hence is suggested a simple direct way of dealing with the evidence obtained by Galileo.

(4) *The time in which a given space is traversed is proportional to the square root of the space.*

NOTE.—In stating all such laws, we ought, strictly, to speak of the numbers representing time, space, velocity, and so on, the same units of time and space being used throughout. But this, though correct, is cumbrous; and the more convenient method followed above, though less correct, can lead to no mistakes if properly explained at the outset.

(443.) From law 2 it is obvious that the observed fact that a body falling from rest acquires a velocity of 32·2 ft. per second, indicates 32·2 as the true value (approximate) of terrestrial gravity—the foot being the unit of space, and the second the unit of time.

(444.) Galileo recognised, even in the comparatively simple case of a body moving vertically under gravity, the operation of the second law of motion; for it might well have been supposed, before that law was established, that a body moving upwards and downwards would not have the same downward velocity communicated to it (whether added to motion already downwards, or abstracted from upward motion) as a body originally at rest. But it was in dealing with the motion of projectiles that close observation and keen reasoning were most needed to see and show that gravity produces precisely the same effect in a given interval of time, not only on a body rising or falling vertically, but on a body so projected as to traverse a curvilinear course. Here, as we are not dealing with dynamics, except as they bear on the physics of astronomy, we need only illustrate by the accompanying diagram, fig. 126. A missile is supposed to be projected from P with a velocity such that the vertical portion of the velocity alone would carry the body up to a, where gravity would reduce that part of the motion to rest, so that the body, if it only had that motion, would descend from rest at a to P; while the horizontal velocity is such, that if the body had no other velocity, and was not affected by gravity (or moved on a perfectly smooth horizontal surface), it would move uniformly along the horizontal line, P P′, in the time supposed to be occupied in rising to a and returning to P under the action of gravity. From A along a P we take ab to represent 16·1 feet; then, $bc = 3\,ab$; $cd = 5\,ab$; $de = 7\,ab$; and $f\mathrm{P} = 9\,a\,\mathrm{P}$; next (supposing 6 seconds to be the precise time of ascent and of descent) we divide P P′ into twelve equal parts in f, e, d, &c. We next complete the series of vertical and horizontal parallels shown in the figure, intersecting in the points F, E, D, &c. Then the second law of motion shows that the body, which in the first second

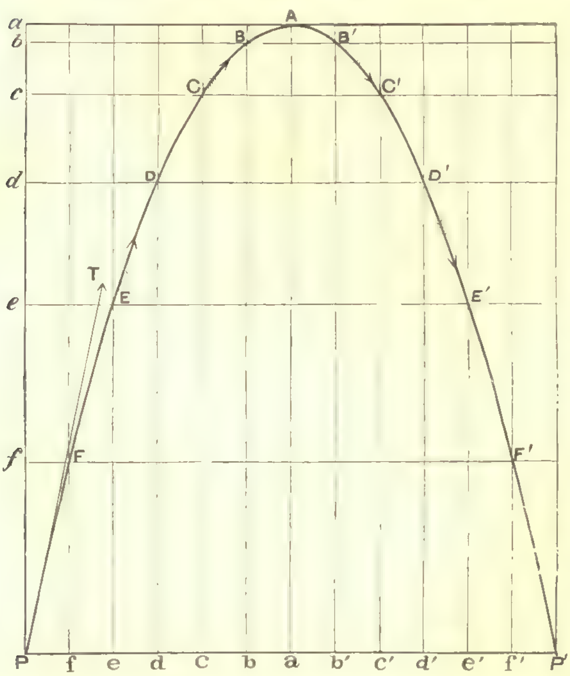


FIG. 126.—Illustrating the parabolic motion of a projectile.

after projection would move from P to f with the horizontal part of its motion, and from P to f with the vertical part of its motion, will be found at F at the end of that second; similarly it will be at E at the end of the next second, at D at the end of the third second, and so on, traversing the curved path P D A D′P′ during the twelve seconds of its flight. Geometry shows that this curve is a parabola.[1]

(445.) Just ideas respecting the deflection of a moving body under the action of thwart forces are important towards the correct appreciation of planetary motions and perturbations, as of other motions and perturbations recognised in dealing with problems in physical astronomy. I here take, however, only the simplest case, that of a force acting at right angles to the direction in which a body is moving, the force being supposed impulsive—that is, all acting in an instant of time—an idea often practically convenient, though never absolutely exact even in cases where a moving body really is deflected by a sharp and sudden stroke.

(446.) Suppose a body moving uniformly from P towards A to receive at P an impulse which acting alone would carry it from P to a in the same time. Then we

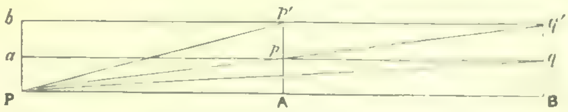


FIG. 127.—Illustrating the deflection of a moving body by a thwart force.

know that the actual course of the body will be along the line P p, so that the deflection of the body is represented by the angle p P A. If now we suppose the velocity of the body to have been twice as great along P A, so that it would have traversed the distance P B in the time in which the effects of the impulse alone would have carried it to a, then the actual course of the body will be along P q, and its deflection will only amount to the angle q P B. If, on the other hand, the original velocity remaining unchanged, the impulsive force had been doubled, so that under its influence alone the body would have been carried to b, the actual course of the body would have been along Pp', its deflection being p'P A. Lastly, if both the velocity and the impulse had been doubled, the actual course of the body would have been along P q', and the deflection q'P B—or the same as in the first case. Regarding the distances P A, A B, P a, P b, as very small —practically evanescent—which corresponds to the study of momentary movements and the supposed action of uniform forces or of forces not acting continuously but by

[1] Here no account has been taken of the resistance of the air; Galileo had to deal with this as well as he could, applying to the observed results such corrections as he judged suitable. For projectiles moving with small velocity in still air the corrections are slight: but for bodies projected with great velocity the effects of resistance are considerable, greatly reducing the range of flight (in common practice often reducing it to a mere fraction of its amount as calculated for a vacuum) and impairing the symmetry of the curve traversed, the descent along the part corresponding to A D′ P′ being much more nearly vertical, and occupying a longer time than the ascent along the part corresponding to P D A.

It is to be observed, further, that in reality the parabolic path assigned by Galileo to a projectile moving under the action of gravity *in vacuo*, is only an approximation to the true path. P A P′ is in reality the apogeal end of a long ellipse traversed under the action of the earth's mass, which (for the purpose of such a problem) may be supposed collected at its centre, which lies at the remoter focus of this long ellipse. I shall have occasion presently to consider the flight of a projectile as thus viewed. Here I need only mention that where the range of flight is great, the force of gravity, tending as it does to the Earth's centre, cannot be regarded (in close approximations) as acting in such parallel directions as are represented in fig. 126 by the lines F f, E e, D d, &c.

rapidly succeeding and exceedingly minute impulses—we see that the following laws hold in regard to deflections—

(447.) *The deflection of a body acted upon by a force at right angles to its course is*

1. *Directly proportional to the force acting on the body; and*
2. *Inversely proportional to the body's velocity.*

(448.) These laws need only be modified for other thwart forces, by resolving each such force into two parts, one acting at right angles to the body's course, the other in the direction of that course. To the former part of the force the above laws apply; the latter tends to increase or diminish the body's velocity in the direction it had had before the force acted. The effect of each part can thus be readily determined. The difficulty arising from the continuous action of forces uniformly or continuously varying, has required for its mastery the development of methods of mathematical analysis by which the finite effects of quantities or changes infinitely small but infinite in number, can be determined. These methods, though they necessarily seem approximate (since we can never deal directly with either the infinitely great or the infinitely small), lead to absolutely definite results.

(449.) Considering the moon's movements in the light of the fundamental laws of motion, Newton was led to suspect that terrestrial gravity, already known to act apparently unchanged to the summits of the highest mountains, may extend its action to the moon.

(450.) It is to be observed that already astronomers and physicists had begun to recognise the probability that the planets are retained in their orbits by a force residing in the sun, and diminishing outwards as the square of the distance increases: so that when the idea was suggested that the earth rules the moon, the influence of terrestrial gravity at the moon's distance would be inferred at the outset by this law of variation with distance.

(451.) If there is any truth in the story of the apple (I refer to Newton's not Eve's), Newton certainly did not inquire *why* the apple fell to the earth. It is not impossible that on some occasion, when he was pondering over the motions of the celestial bodies,—and perhaps thinking of those inviting speculations by which Borelli, Kepler, and others had been led to regard the celestial motions as due to attraction,—the fall of an apple may have suggested to Newton that terrestrial gravity afforded a clue which, rightly followed up, might lead to an explanation of the mystery. If the attraction of the sun rules the planets, the attraction of the Earth must rule the moon. *What if the very force which drew the apple to the ground be that which keeps the distant moon from passing away into space on a tangent to her actual orbit!* [1]

[1] Newton illustrates the idea of the moon's motion around the Earth and of any planet's motion around the sun by the motion of projectiles. 'For,' he says, 'a stone projected is by the pressure

(452.) Whether the idea was suggested in this particular way or otherwise, it is certain that in 1665, at the age of only twenty-three years, Newton was engaged in the inquiry whether the Earth may not retain the moon in its orbit by the very same inherent virtue or attractive energy whereby she draws bodies to her surface when they are left unsupported.

(453.) This question depends on the law according to which the attractive force diminishes with distance. Assuming it to be identical in quality with the force by which the sun retains the several planets in their orbits, Newton had, in the observed motions of the planets, the means of determining the law very readily, at least for circular orbits—which at this stage of the inquiry might conveniently be considered. The following reasoning, based on the considerations touched on in Art. 446, and illustrated by fig. 127, may be

of its own weight forced out of the rectilinear path, which by the projection alone it should have pursued, and made to describe a curve line in the air; and through that crooked way is at last brought down to the ground; and the greater the velocity is with which it is projected, the farther it goes before it falls to the earth. We may, therefore, suppose the velocity to be so increased, that it would describe an arc of 1, 2, 5, 10, 100, 1000 miles before it arrived at the earth, till at last, exceeding the limits of the earth, it should pass quite by without touching it.

'Let A F B, fig. 128, represent the surface of the Earth, C its centre, V D, V E, V F the

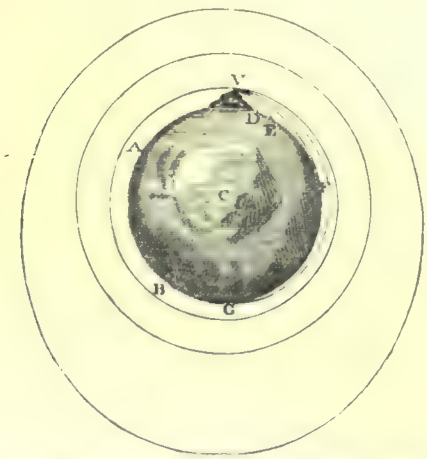


Fig. 128.—Illustrating the Moon's motion round the Sun. (Facsimile from an early edition of the *Principia.*)

curve lines which a body would describe if projected in an horizontal direction from the top of a high mountain successively with more and more velocity; and because the celestial motions are scarcely retarded by the little or no resistance of the spaces in which they are performed, to keep up the parity of cases, let us suppose either that there is no air about the Earth, or at least that it is endowed with little or no power of resisting; and for the same reason that the body projected with a less velocity describes the lesser arc V D, and with a great velocity the greater arc V E, and, augmenting the velocity, it goes farther and farther to F and G, if the velocity was still more and more augmented, it would reach at last quite beyond the circumference of the Earth, and return to the mountain from which it was projected.

'And since the areas which by this motion it describes by a radius drawn to the centre of the Earth are (by prop. 1, book 1, *Princip. Math.*) proportional to the times in which they are described, its velocity, when it returns to the mountain, will be no less than it was at first; and, attaining' [or rather retaining] 'the same velocity, it will describe the same curve over and over, by the same law.

'But if we now imagine bodies to be projected in the directions of lines parallel to the horizon from greater heights, as of 5, 10, 100, 1000, or more miles, or rather as many semi-diameters of the Earth, those bodies, according to their different force of gravity in different heights, will describe arcs either concentric with the Earth, or variously eccentric, and go on revolving through the heavens in those trajectories, just as the planets do in their orbs.'

This explanation and Newton's picture (fig. 128) may be regarded as now common property, and scarcely need referring to their author: but since quite a discussion arose lately as to whether one writer on astronomy (Sir Rob. Ball) was entitled to borrow them from another (Professor Sim. Newcomb) it will be as well to remove all dispute by referring the explanation to its real author, Sir Isaac Newton—whom both had forgotten.

regarded as quite sufficient to suggest the law of diminution according to the inverse square of the distance :—

(454.) Let us call the distance of a planet (the Earth, suppose), unity or 1, its period 1, its velocity 1. Let the distance of a planet farther from the sun be called D; then the third law of Kepler tells us that its period will be the square root of $D \times D \times D$, or will be $D\sqrt{D}$. But regarding the orbits as circles round the sun as centre, the circumference of the larger orbit will exceed that of the smaller in the proportion of D to 1; hence, if the velocity of the outer planet were equal to that of the inner, the period of the outer planet would be D. But it is greater, being $D\sqrt{D}$ (that is, it is greater in the proportion of $\sqrt{D}$ to 1); hence the velocity of the outer planet must be less, in the proportion of 1 to $\sqrt{D}$. Now the sun's energy causes the direction of the Earth's motion to be changed through four right angles in the time 1; that of the outer planet being similarly deflected in the time $D\sqrt{D}$; and we have seen that the deflection of a moving body by a force acting at right angles to its direction is inversely proportional to its velocity; so that the outer planet, moving $\sqrt{D}$ times more slowly, ought to be deflected $\sqrt{D}$ times *more* quickly if the sun influenced it as much as he does the nearer one. Since the outer planet, instead of being deflected $\sqrt{D}$ times more quickly, is deflected $D\sqrt{D}$ times *less* quickly the influence of the sun on the outer planet must be less than on the earth $(\sqrt{D}) \times (D\sqrt{D})$ times—that is, $(D)^2$ times less. In other words, the attraction of the sun as thus determined, or rather suggested, diminishes inversely as the square of the distance.

(455.) Newton had therefore only to determine whether the force continually deflecting the moon from the tangent to her path is equal in amount to the force of terrestrial gravity reduced in accordance with this law of inverse squares, in order to obtain at least a first test of the correctness of the theory which had suggested itself to his mind. Let us consider how this was to be done; and in order that the account may agree as closely as possible with the actual history of the discovery, let us employ the elements actually used by Newton at this stage of his labours.

(456.) Newton adopted for the moon's distance in terms of the Earth's radius a value very closely corresponding to that now in use. We may, for our present purpose,

Fig. 129.—Illustrating the Earth's action on the Moon.

regard this estimate as placing the moon at a distance equal to sixty terrestrial radii. Thus the attraction of the Earth is reduced at the moon's distance in the proportion of the square of sixty, or 3,600, to unity. Now, let us suppose the moon's orbit circular, and let $m\,m'$, fig. 129, be the arc traversed by the moon in a second round the Earth at E ($m\,m'$ is of course much larger in proportion than the arc really traversed by the moon in a second); then when at m the moon's course was such that if the Earth had not attracted her, she would have been carried along the tangent line $m\,t$; and if t be the place she would have reached in a second, then $m\,t$ is equal to $m\,m'$, and E t will pass almost exactly through the point m'. Thus $t\,m'$, which represents the amount of

fall towards the Earth in one second, may be regarded as lying on the line t E.[1] Now m'E is equal to m E, and therefore $t\,m'$ represents the difference between the two sides m E and t E of the right-angled triangle m E t. Newton adopted the measure of the earth in vogue at the time, according to which a degree of arc on the equator was supposed equal in length to 60 miles, or the Earth's equatorial circumference equal to 21,600 miles. This gave for the circumference of the moon's orbit 1,296,000 miles, and for the moon's motion in one second rather less than half a mile. Thus $t\,m$ and m E are known, for m E is equal to thirty terrestrial diameters; and it is easy thence to determine t E.[2] Now Newton found, that with the estimate he had adopted for the Earth's dimensions, t E exceeded m E by an amount which, increased 3,600-fold, only gave about 14 feet, instead of $16\frac{1}{10}$ feet, the actual fall in a second at the Earth's surface.

(457.) This discordance appeared to Newton to be too great to admit of being reconciled in any way with the theory he had conceived. If the deflection of the moon's path had given a result *greater* than the actual value of gravity, he could have explained the discrepancy as due to the circumstance that the moon's own mass adds to the attraction between the Earth and herself. And a slightly *less* value might be explained as due to the sun's perturbing action, drawing the moon on the whole somewhat from the Earth. But so great a deficiency was inexplicable. Newton therefore laid aside the investigation.

(458.) Fourteen years later Newton's attention was again attracted to the subject, by a remark in a letter addressed to him by Dr. Hooke, to the effect that a body attracted by a force varying inversely as the square of the distance, would travel in an elliptic orbit, having the centre of force in one of the *foci*. It is to be noted that Hooke gave no proof of the truth of his remark; nor was there anything in his letter to show that he had established the relation. He was not, indeed, endowed with such mathematical abilities as would have been needed (in his day) to master the problem in question. Newton, however, grappled with it at once, and before long the idea suggested by Hooke had been mathematically demonstrated by Newton. Yet, even in ascribing the idea to Hooke's suggestion at this epoch, we must not forget that Newton, in the very circumstance that he had discussed the moon's motion as possibly ruled by the Earth's attraction, had implicitly entertained the idea now (perhaps) first explicitly enunciated by Hooke: for the moon does not move in a circle around the Earth, but in an ellipse.

(459.) In studying this particular problem. Newton's attention was

[1] In the account ordinarily given, $t\,m'$ is taken as lying parallel to m E. This is *also* approximately true. As a matter of fact the point m' lies a little outside t E (that is, on the side away from m) and a little within the parallel to m E through t. But the angle t E m is exceedingly minute. (This angle, as drawn, represents the moon's motion for about half a day instead of a single second of time.)

[2] By Euc. 1. 47 the square on t E is equal to the squares on $t\,m$ and m E.

naturally drawn again to the long-abandoned theory that the Earth's attraction governs the moon's motions. But he was still unable to remove the discrepancy which had foiled him in 1665.

(460.) At length, however, in 1684, news reached him that Picard[1] had measured a meridianal arc with great care, and with instrumental appliances superior to any which had been hitherto employed. The new estimate of the earth's dimensions differed considerably from the estimate employed by Newton before. Instead of a degree of arc at the equator being but 60 miles in length, it now appeared that there are rather more than 69 miles in each degree.

(461.) The effect of this change will be at once apparent. The Earth's attractive energy at the moon's distance remains unaffected, simply because the proportion of the moon's distance to the Earth's diameter had alone been in question. Newton, therefore, still estimated the Earth's attraction at the moon's distance as less than her attraction at her own surface, in the proportion of 1 to 3,600. But now all the real dimensions, as well of the Earth as of the moon's orbit, were enlarged linearly in the proportion of $69\frac{1}{2}$ to 60. Therefore the fall of the moon per second towards the Earth, increased in the proportion of 3,600 to 1, was enlarged from rather less than 14 feet to rather more than 16 feet—agreeing, therefore, quite as closely as could be expected with the observed fall of $16\frac{1}{10}$ feet per second in a body acted upon by gravity and starting from rest.

(462.) It is said that as Newton found his figures tending to the desired end, he was so agitated that he was compelled to ask a friend to complete the calculations. But the work was of so exceedingly simple a character that this must be regarded as altogether improbable, if not impossible.

(463.) Newton now returned to the investigation of the laws of planetary motion, starting from the beginning and reasoning upon the whole subject in the light of the facts and laws now known and established.

(464.) In the first place, since the planets do not move in straight lines, but are constantly changing the direction of their motions, we see from the first law of motion that some force must be constantly acting upon them. We have already seen this in the case of the moon travelling round the earth as at E (fig. 129) in the curved path *m m′*. Precisely the same reasoning applies (and the same figure serves), in considering the motion of a planet around the sun. A body moving in direction *m t*, fig. 129, would continue to travel in that straight line unless a force acted upon it. If we find that instead of so moving the body moves on a curved path as *m m′* deflected from *m t*, we know by the second law of motion that the force deflecting the body must have been directed towards that side of *m t* on which *m m′* lies. In the case of the planets whose deflection is constantly towards the sun,[2] it was clear that there must reside some attractive force in or near the sun drawing them towards him.

[1] Picard died at Paris in 1682, two years before the news of his labours had reached the ears of Newton.

[2] I may correct here a very common error, arising from the inexact way in which these matters are often dealt with. It is quite commonly supposed that a planet moving in such a path as A B C, fig. 130, is under the influence of two balancing forces, one toward S, called the *centripetal force*, and the other from S, called the *centrifugal force*. If we consider the motion of a planet over any part of its path, without any supposed know-

(465.) Next, Newton showed that Kepler's second law indicates the sun as the centre of force deflecting the planets constantly from the tangent to their paths. To prove this, he adopted the device of imagining the continuous action of the sun changed into a succession of impulses following each other in very rapid succession. Suppose a planet at A (fig. 130) is travelling at the moment in the direction A T, and suddenly receives an impulse from the sun S which would bring the planet to M in the same time in which, if undisturbed, it would travel to T. Then we know that A B, the diagonal of a parallelogram having sides A T and A M, will be the actual course of the planet. But the triangle A B S has an area equal to that of the triangle A T S, because they are of equal height on the base A S. So that the impulse exerted towards S has not affected the area swept out by the planet around S. In the next equal small interval of time, again, the planet would move on to *t*, such that B *t* is equal to A B, thus sweeping out the surface B S *t* equal to B S A. But, if suddenly pulled towards S in such a way that it would traverse a distance B *m*, in the same time which would carry it undisturbed to *t*, its actual course will be B C, the diagonal of the parallelogram having *t* B and B *m* as sides. And we see that the triangle B S C is equal to the triangle B S *t*; or *again*, the area swept out round S is unaffected by the impulse towards S. It is obvious that if the impulse were not towards S the area would be affected. If M lay on the same side of A S as T, T B, parallel to A M, would not be parallel to A S, but would give a triangle (A B S) greater than A T S; and the reverse if M was on the other side of A S. Since then, we observe that each planet constantly sweeps out equal areas around the sun, we know that the force acting on the planet must be constantly directed towards the sun. The validity of this reasoning is not at all affected by substituting for the constant action of the sun a succession of very swiftly succeeding small impulses. For just as a curved line may be represented by a great number of very small straight lines as closely as we please, and theoretically so closely that no faculties we possess would enable us to distinguish the line thus made up from a true curve, so it is clear that a multitude of swiftly succeeding small impulses would produce effects so closely corre-

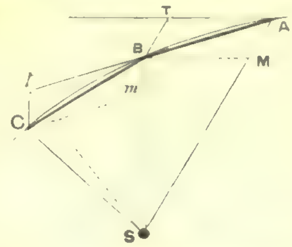


FIG. 130. Showing how the Sun is proved to be the centre of attractive force by which the planets are governed.

ledge of the force acting on the planet, and judging only of the direction in which such force acts by the way in which the planet behaves, we see at once that the force must be towards the centre. The planet at A is moving towards T; it is found, after a short interval, at B, instead of T, where, if not acted on by any force, it would have been. It has then been deflected in the direction T B, or towards the side of A T on which S lies. There is, then, no centrifugal force acting on the planet, but always a force towards S. The expression 'centrifugal force,' used by Newton correctly according to his own explanation of the word force —which made *inertia* a force—would only be correctly applied, according to the modern use of the word 'force,' if Galileo's first notion, that bodies not acted on by any force would move uniformly in a circle, were correct. So soon as we recognise that a body not acted on by a force will move uniformly in a straight line, we see that a body which leaves the straight course must be acted on by a force directed towards that side on which the body is deflected from that former course. We need, in fact, consider only this:—If a body at A, fig. 130, moving so that it would reach T in a certain interval of time, receives an extraneous impulse whose action causes the body to be found at B at the end of this interval, then, drawing A M parallel to T B, A M represents this impulse in direction and magnitude on the same scale on which A T would represent an impulse competent to give the body its motion in direction A T. We know, then, from the observed course of the body that no force which would have to be represented by a line from A on S A produced has acted at all upon it. In other words, from the observed motion of a planet at every point of its orbit, we know that no centrifugal force acts on the planet.

sponding to those of an absolutely continuous force, that no faculties we possess would enable us to distinguish one set of effects from the other.

(466.) Having shown that the planets must be under the action of a force residing in the sun, and the moons of a planet under the action of a force residing in that planet, Newton next inquired according to what law such a force must act to explain the actual shape of the paths followed by the planets (as shown by Kepler's first law), and to explain the relations indicated in Kepler's third law.

(467.) He had already shown that if the planets' paths be regarded as circles described at the planets' mean distances, then, if the sun's attractive force diminishes as the square of the distance, the third law of Kepler would be fulfilled.

(468.) It is possible that these inquiries, and the further much more difficult inquiry whether a planet not moving in a circle round the sun so attracting would move in an ellipse, might have remained matters of interest only to a few mathematicians, but for the momentous discovery now made by Newton. Halley, Wren, and Hooke had accompanied Newton (perhaps even anticipated him, though this is not certainly known) thus far. Halley and Wren had also tried to prove that a planet would travel in an ellipse round the sun if his attraction diminishes inversely as the square of the distance; but they had both failed. Hooke (as we have seen) alleged that he had solved this problem. He said he would keep back his solution till others, failing, acknowledged the difficulty of the problem; but doubtless Hooke had really failed. Newton solved the problem, but laid aside the solution till Halley, reopening the question, recalled his attention to the subject.

(469.) Several points had at once to be dealt with, however, before this general theory of the mechanism of the heavens could be regarded as established.

First, Newton had to show that the elliptic paths traversed by the planets accord with the law of attraction, and moreover that Kepler's third law, true for circular paths, would be true also for elliptical paths.

(470.) The full reasoning in dealing with these problems is not such as can be presented in an absolutely popular form. A problem which had altogether foiled Wren, Halley, and Hooke, and which even Newton was not able to solve in a simple way, cannot, of course, be so simplified as to be presented without details such as mathematicians only could understand.

(471.) Yet the line of argument followed in the 'Principia' was in reality not very complex.

Let A P Q A′, fig. 131,[1] be an ellipse traversed by a planet around the sun at S, or by one of the moons of Jupiter or Saturn around its primary. The eccentricity is, of course, greatly exaggerated. Suppose P Q a small portion of the planet's path described in the time t, P T the tangent at P showing the course in which the body would have travelled during the time t but for the attraction of the orb at S. Join S P, S Q and draw Q T, Q N parallel to S P, P T; also draw Q L perpendicular to S P.

(472.) Then we know that while the planet is (uniformly) sweeping out the small area S P Q around S, it is drawn towards S through the distance Q T or N P (P Q being

[1] Fig. 131 illustrates a method of constructing ellipses much favoured in popular works. Two pins are stuck in at the foci S and S′, and a tied string S P S′ kept stretched by a pencil at P is supposed to guide the pencil point along the ellipse A B A′ B′. So it will, if the pins are indefinitely thin, and upright, the string inexpansible, and the pencil upright and indefinitely fine-pointed, the string remaining close to the paper: conditions which never hold. The method is not only clumsy, but untrustworthy—except, of course, for very simple cases.

so small an arc that it may be regarded as a straight line). The time then is proportional to the area P S Q; or to S P, Q L: and the space traversed under the action of the attracting force exerted by the body at S is P N. But we know (Art. 442) that whatever the space traversed by a body in a given time under a uniform force (and of course the force acting at S may be regarded as uniform on the body moving over the very short arc P Q), the force is proportional to the space directly and to the square of the time inversely. Hence the force exerted by the attracting orb at S on the body at P is proportional to $NP \div (SP)^2(QL)^2$. But by a well-known property of the ellipse $NP \div (QL)^2$ is constant,[1]

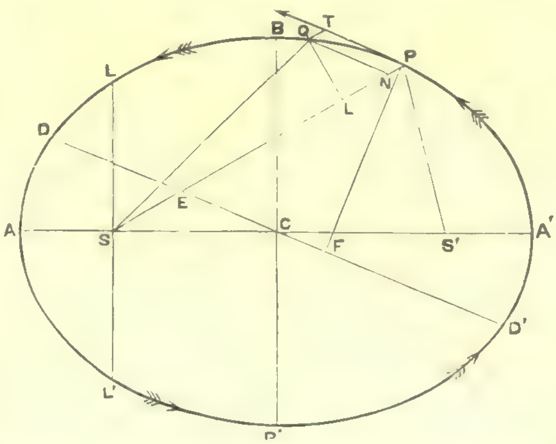


Fig. 131.—Illustrating the demonstration of Elliptic Motion.

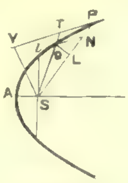


Fig. 132.

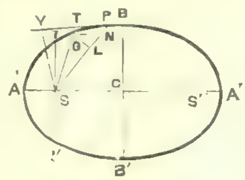


Fig. 133.

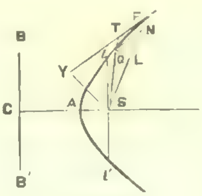


Fig. 134.

[1] This may be shown thus: Complete fig. 131 by drawing the axes A C A′, B C B′, and the diameter D C D′ parallel to the tangent at P; also draw P F perpendicular to D D′, and let E be the point of intersection of S P and D C. Then

$$\frac{NP}{(QL)^2} = \frac{NP}{(QN)^2} \cdot \frac{(QN)^2}{(QL)^2}.$$

But $(QN)^2 \div NP$ is ultimately the chord of curvature through $S = 2(CD)^2 \div AC$; and

$$(QN)^2 : (QL)^2 :: (EP)^2 : (PF)^2 :: (AC)^2 : (PF)^2 :: (CD)^2 : (BC)^2;$$

(for $PE = AC$ by an elementary property of the ellipse; and $AC.BC = PF.DC$ by another). Hence ultimately (that is, when Q is indefinitely near to P),—

$$\frac{NP}{QL^2} = \frac{AC}{2(CD)^2} \cdot \frac{(CD)^2}{(BC)^2} = \frac{AC}{2(BC)^2}$$

$$\frac{1}{2SL} = \frac{1}{\textit{latus rectum}}$$

The most logical way, however, of dealing with the problem actually attacked by Wren, Hooke, Halley, and Newton, is to inquire what will be the orbit traversed under a force varying as the inverse square of the distance. If we had dealt with the problem thus we should have represented the force at S by $C \div (SP)^2$, where C is some constant; and with the necessary portion of the construction already used, we should have found—

$$\frac{C}{(SP)^2} = \frac{NP}{(SP)^2(QL)^2}$$

or

$$\frac{NP}{(QL)^2} = C, \text{ a constant.}$$

Now it can easily be shown for the hyperbola and parabola, as it has just been shown for the ellipse, that, P Q, figs. 132, 133, and 134, being any very small arc of a conic section of which S is a focus, to which lines P S, Q S, are drawn (in the case of the hyperbola it must be the focus lying within the concavity of the branch on which P and Q lie), then—

$$\frac{NP}{(QL)^2} = \frac{1}{2SL} = \frac{1}{LSL'} = \frac{1}{\textit{latus rectum}}$$

Hence, whatever the force at S, provided only that it varies as the square of the distance, a body projected in any direction P T, and with any velocity, will describe a conic section around S as focus. The nature of the conic section may be determined according to the conditions given in any case. Thus, if the velocity be given at any point, and the attractive force of the central mass, it can readily be shown that if this velocity is equal to that with which a body

wherever P may be taken (P Q being very small). Hence the force is proportional to $\frac{1}{(S\,P)^2}$, or varies inversely as the square of the distance.

(473.) The third law of Kepler follows immediately from the value of the force thus deduced for the case of a particular orbit. For we see that the area P S Q is swept out round the centre of force at S in the time in which a body would move from rest at P to N under the action of a force which we may represent by $C \div (S\,P)^2$. Now the square of this time would be proportional (by Art. 472) to N P. $(S\,P)^2$. Hence since the time of periodic circuit exceeds the time of traversing P Q as the whole area of the ellipse exceeds the area P S Q, it follows that the square of the time of periodic circuit

$$\begin{aligned} &\propto N\,P\,(S\,P)^2\left[\frac{A\,C.\,B\,C}{P\,S\,Q}\right]^2 \\ &\propto (A\,C)^2(B\,C)^2.\frac{N\,P}{(Q\,L)^2}. \\ &\propto (A\,C)^3 \text{ since } \frac{N\,P}{(Q\,L)^2} = \frac{A\,C}{2\,(B\,C)^2} \text{ (See preceding note).} \end{aligned}$$

In other words *the squares of the periods of orbits described around the same central mass attracting inversely as the square of the distance vary as the cubes of the semi-axes or of the mean distances.*

(474.) This law would be strictly true if the planets were infinitely minute compared with the sun: but the masses of the planets, though very small, bear yet definite relations to the sun; and, as a matter of fact, instead of considering each planet as swayed by the sun's mass, we must regard each as though swayed by the sum of its own mass and the sun's, supposed to be gathered at the sun's centre. This at least is a sufficient rule as regards the period of a planet and the dimensions of its orbit with respect to the sun; though of course to determine the actual orbit round the common centre of gravity we should have to take into account the actual disposal of the masses forming this sum. So that, in effect, to obtain the exact law for the periods and mean distances of the planets we have to regard them not as bodies circling round the same centre, but as so many different bodies revolving round centres slightly differing in attractive energy; Jupiter, for instance, round a centre equal in mass to Jupiter and the sun; Saturn round a centre equal in mass to Saturn and the sun; and so on. The result of this consideration is that, instead of finding the fraction $\frac{(\text{mean distance})^3}{(\text{period})^2}$ *constant* for the solar system, we find that this fraction calculated for the different planets (1) Mercury, (2) Venus, (3) Earth, and so on, gives results respectively proportional to—(1) the sun's mass added to Mercury's, (2) the sun's mass added to Venus's, (3) the sun's mass added to the earth's, and so on.[1]

would describe a circle at half the distance of that point from the central mass, the body will traverse a parabola: if the velocity is less than this, the body will move in an ellipse; if greater, in a hyperbola.

[1] The law thus interpreted is applicable to all cases where different bodies revolve around a common centre. But it also admits of being generalised for different bodies travelling round different centres. Thus extended, it runs as follows:—

If a body of mass m revolves round a centre of mass M in time P, and at a mean distance D, and another body of mass m' revolves round another centre of mass M' in time P', and at a mean distance D', then

$$\frac{D^3}{P^2\,(M+m)} = \frac{D'^3}{P'^2\,(M'+m')}$$

or

$$\frac{D^3}{P^2} : \frac{D'^3}{P'^2} :: (M+m) : (M'+m').$$

This general law—almost as simple, be it ob-

(475.) The next point was the inquiry into the peculiarities of the moon's motions. These had hitherto proved inexplicable, but might confidently be expected to find their interpretation in the disturbing influence exerted by the sun and (in much less degree) by the planets, on the motion of the moon round the Earth.

The study of the moon's motions in full would require a volume many times larger than the present. Here only so much can be presented as will indicate the nature of the forces at work, and in a general way how they operate.[1]

(476.) The matter which immediately follows should be carefully studied, because the considerations here involved, and the very proportions of the forces deduced, apply practically unchanged to the study of the tides and of the precession of the equinoxes, while the general method employed illustrates the processes by which all disturbing forces are to be dealt with geometrically. The analytical study of perturbations cannot even be touched in such a work as the present; it is, however, but the development of processes of calculation applied to such relations as are here presented geometrically.

(477.) Let E, M, and S, represent the Earth, moon, and sun—the moon on the first quadrant of her path from 'new moon,' where she crossed the line E S.

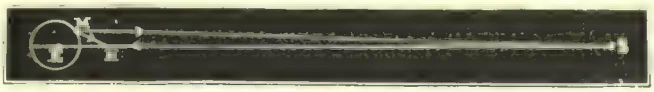


FIG. 135.—Illustrating the Sun's absolute action on the Moon (S M), and on the Earth (S H); and his relative or perturbing actions on the Moon (M H).

Suppose we represent the pull of the sun on the moon by the line S M. Then in order to determine the sun's *perturbing* action on the moon, we must take into account the sun's pull on the Earth in the direction E S, noting that we have not to consider here the relative masses of the Earth and moon, since the whole question is

served, as Kepler's third law—is extremely important. It may be regarded as the fundamental law of the celestial motions. It presents the influence of gravity as a bond associating the motions of all the orbs in the universe, whether of double suns round each other, or of primary planets round suns, or of secondary planets round their primaries. It is a law absolutely universal (so far as is known), and strictly exact, excepting in so far as *perturbations* come into operation to affect it; and as perturbations have very little effect on *mean* periods of revolution, the exactness of the law is scarcely affected in this way. It is an impressive thought that we can by means of such a law associate the motions of bodies which to ordinary apprehension have nothing in common that, for instance, such a relation as this can be affirmed:—

$$\frac{\left[\begin{array}{c}\text{Moon's mean}\\ \text{distance}\\ \text{from Earth.}\end{array}\right]^3}{\left[\begin{array}{c}\text{Moon's}\\ \text{mean}\\ \text{period.}\end{array}\right]^2\left[\begin{array}{c}\text{Sum of}\\ \text{moon's mass}\\ \text{and Earth's.}\end{array}\right]} = \frac{\left[\begin{array}{c}\text{Mean distance be-}\\ \text{tween components}\\ \text{of a Centauri.}\end{array}\right]^3}{\left[\begin{array}{c}\text{Their period}\\ \text{of revolu-}\\ \text{tion.}\end{array}\right]^2\left[\begin{array}{c}\text{Sum of}\\ \text{their}\\ \text{masses.}\end{array}\right]}$$

It will be observed how the law enables us at once to compare the sums of the masses, when we know the mean distances and periods. For it may be written

$$\frac{M + m}{M' + m'} = \frac{D^3 P'^2}{D'^3 P^2}$$

Also where m and m' are both small compared with M and M′ respectively, the law becomes simplified into

$$\frac{M}{M'} = \frac{D^3 P'^2}{D'^3 P^2}$$

[1] In the first edition of my treatise on the Moon, the lunar perturbations were somewhat fully dealt with, and I think more clearly for the general reader than elsewhere: for in Airy's masterly treatise on *Gravitation* the illustrations are not sufficiently numerous, and do not approach nearly enough to the actual proportions of the orbits, &c., supposed to be presented; and the discussion of the moon's motions in Herschel's *Outlines of Astronomy* assumes more knowledge than most readers even of astronomical works can be expected to possess. But even my comparatively simple and fully illustrated account was 'caviare to the general.' I was by some accused of unfairness in introducing such difficult matter into a work which readers had expected to find wholly popular. In later editions I excised all the best part of that matter.

of motions actually generated, and the sun does not tend to give more motion to the Earth by his pull than he would if her mass were the same as the moon's. To the greater mass he gives a proportionately greater pull, and so tends to communicate the same velocity. A line such as H S, somewhat less than M S, will represent the sun's pull on the Earth; and M H will represent on the same scale the sun's perturbing action on the moon. In the figure, H S is marked with an arrow pointing towards the earth. This is done to show that in considering the *balance* of force acting on the moon relatively to the Earth, we have to regard the moon as *pulled* by a force M S towards the sun, thrust by a force E H from the sun, and therefore *perturbed* by a force M H acting towards H.[1]

(478.) Where the disturbing force is relatively so distant as in the case of the sun perturbing the moon, the sun or moon disturbing the sea (causing the tides), or acting on the protuberant portions of the earth's equatorial regions and so disturbing the Earth's axial pose (causing the precession of the equinoxes), there is a very simple and convenient way of determining the position of the line M H. Although in what follows, the reasoning seems to relate only to the sun perturbing the moon, it may be regarded as applying in all respects save scale to the other cases also.

(479.) Let E, M, and S in fig. 136 represent the Earth, moon, and sun, situated as in fig. 135, except that a space of about ten yards intervening, on the scale of fig. 136, between the moon's orbit m M m' and the sun's globe S (shown on the same scale as the moon's orbit) is omitted for want of space. (The lines E s and s S are supposed to be parts of the magnified representation of the line E S of fig. 135; while M s' and s' S are parts of the line M S).

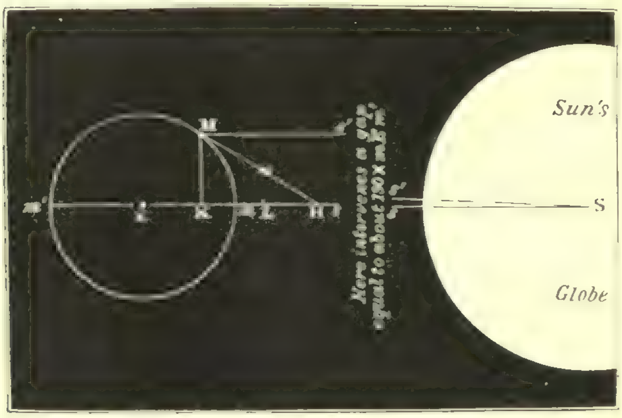


FIG. 136.—The same as fig. 135, much enlarged.

(480.) Now, taking into account the smallness of E m compared with E S, we see that M K square to E m cuts off K S appreciably equal to M S. To represent properly the sun's force on E, where S M (both figs.) represents his force on M, we must take a point H, such that H K is twice E K. For, to make H S less than M S or K S by so much only as E K, would obviously only make the deduced force (L S) less than K S as the distance K S is less than E S; and we must have twice this reduction to correspond to a force (H S) less than K S as the square of the distance K S is less than the square of the distance E S. Hence we must take K L equal to L H equal to E K, or E H equal to three times E K to get the required line M H, representing the perturbing force of the sun on S.[2]

[1] The reader may conceive fig. 135 to represent a small model, in which M is pulled towards S by an elastic string with tension represented by M S, E pulled towards S by a force represented by H S; then by the triangle of forces we find at once that M is pulled (relatively to E) by a force represented by M H.

[2] Of course this involves the assumption that the ratios E S to K S, K S to L S, L S to H S are all equal, which is not exactly correct: but where the equal lines E K, K L, and L H are all very small compared with E S, these ratios are

(481.) If we carry out this method for different points of the orbit $m\,M\,m'$, we get such a result as is pictured in fig. 137. The arrowed lines indicate the directions and varying magnitudes of the perturbing influences exerted by the sun on the moon in her motion, assumed for the moment to take place in a circular orbit round the Earth as centre. It will be understood of course that in fig. 137, as later in figs. 138 and 139, the sun's relative motion by which the line E S of figs. 135, 136, and therefore the line M_1M_3 of figs. 137, 138, and 139, are constantly undergoing displacement, circuiting round E as a centre once in a year, is not taken into account. It could not be, in fact, without introducing confusion. But in what follows the reader will remember that the arcs $M_1\,M_2$, $M_2\,M_3$, &c. must all be supposed increased by the mean arc apparently traversed by the sun in a quarter of a lunar month.

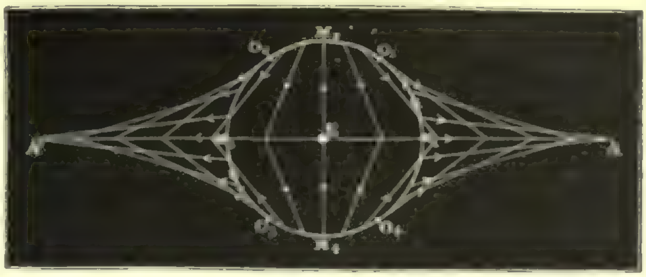


FIG. 137.—The Sun's perturbing action on the Moon in different parts of her orbit.

(482.) Just here, however, we must consider what these perturbing forces actually amount to. In particular we must compare them with the Earth's influence on the moon; for on this comparison their perturbing effect necessarily depends. In fig. 136 M H represents the sun's perturbing force on the same scale on which M S, including the omitted 10 yards or so, represents the sun's direct force on the moon. We may, however, in considering the much larger direct forces, regard E S as representing the sun's direct force on a unit of mass of the Earth or of the moon: for such differences as are represented by M H, K H, &c. are unimportant in this aspect, though all-important in studying the varying perturbing action of the sun. Now E S representing the sun's action on a unit of the Earth's mass at E, the Earth's action on the moon would be represented by a line easily deduced. Since the Earth is nearer the moon, there must be an increase according to the law of the inverse squares of the distance, or as the square of E S exceeds the square of E M; while, since the sun's mass is 330,500 times greater than the Earth's, there must be a decrease in this degree. Hence a line representing on the scale of fig. 136 the Earth's direct pull on the moon will be

$$= (E\,S)\,\frac{(388\cdot5)^2}{330{,}500}.$$
$$= (E\,S) \times (\cdot457)\text{[1]} = \tfrac{16}{35}\,E\,S \text{ approximately;}$$

whence we see that the Earth's pull on the moon is less than half the sun's pull on either the Earth or moon. The sun's pull on the moon (that is, on a unit of the moon's mass) is 2·190 (or about $2\frac{3}{16}$) times as great as the Earth's. It is then with a

so nearly equal that we are very near the truth in regarding them as actually equal, E m is less than $\frac{1}{386}$th of the distance E S; and the error we introduce by taking the above ratios as equal corresponds to that arising from the supposition that 386:385::385:384::384:383; or that $1\frac{1}{385} = 1\frac{1}{384} = 1\frac{1}{383}$. It will be noticed that on the assumption of equal ratios, we have

$$E\,S : K\,S :: (E\,S)^3 : (H\,S)^3$$

and

$$E\,S : K\,S :: (E\,S)^2 : (L\,S)^2 :: (K\,S)^2 : (H\,S)^2;$$

relations which should be all carefully noted, since corresponding relations appear in the theory of the tides and in dealing with the precession of the equinoxes.

[1] The real proportion is ·45663; but this degree of accuracy would be quite out of place in the above inquiry.

force represented by a line equal to about $\frac{16}{35}$ of E S in fig. 135 that the disturbing forces represented by lines such as M H are to be compared. Remembering that E S is equal to 388·4 times E M, we see that the Earth's force on the moon will be represented by a line equal to about 177·39 times E M. On the scale of figs. 136 to 140, the Earth's action on the moon would be represented by a line about $13\frac{3}{4}$ feet in length.

(483.) It will be convenient to resolve the disturbing forces obtained as in fig. 137 into (1) that part of each which acts radially on the moon, drawing her either from or to the Earth as the case may be, and (2) that part which acts tangentially, accelerating or retarding her in her course.

Figs. 138 and 139 show how this is done. In the former the perturbing force M H becomes resolved into the force M B acting radially outwards and M C acting

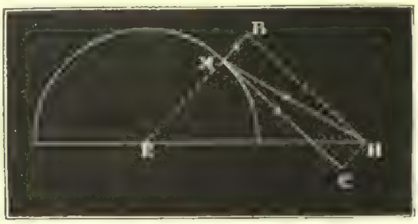


Fig. 138.—Resolving the perturbing action into its radial and tangential parts.

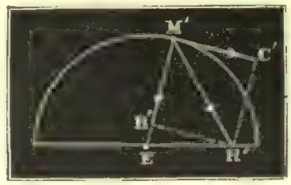


Fig. 139.—Another example of the same process.

tangentially to retard. In fig. 139 the force M′H′ is resolved into M′B′ acting radially inwards, and M′C′ acting tangentially—and as in the other case retardative.

(484.) We thus resolve the full perturbing forces shown in fig. 137, into the series of radial forces shown in fig. 140 and the series of tangential forces shown in fig. 141.

From fig. 140, we see that the radial perturbing forces act outwards round the greater part of the moon's orbit, and that the forces acting outwards also exceed in absolute amount those acting inwards. M_1A and M_3A', for example, are each double of either E M_2 or E M_4. Hence the sun's perturbing action on the moon tends slightly to diminish the Earth's mean pull on her satellite. We observe also that the radial forces tend on the whole to draw away the moon from the Earth when near M_1 and M_3, or where the moon is new or full—or in *syzygy*, as it is called for simplicity's sake—and to draw the moon inwards at M_2 and M_4. It might be supposed that under this influence, renewed at each circuit, the moon's orbit would become elongated in direction M_3M_1, and narrowed in direction M_2M_4; just as the common idea is that the waters of the ocean, exposed to precisely similar disturbing forces of lunar attraction, will tend to rise on the side towards the moon and on the side opposite. If either problem were a statical one, such effects would of course result; but, as a matter of fact, each problem being essentially dynamical, we find a reverse effect.[1] The moon's orbit is lengthened along

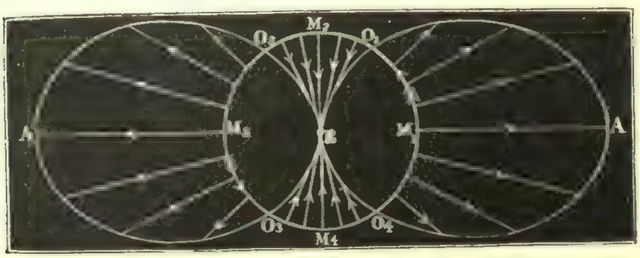


Fig. 140.—Radial parts of the Sun's perturbing action on the Moon.

[1] The difference between the apparently obvious effect of perturbing attraction, in these cases, and real effects, may be compared with the difference between the apparently obvious effect of

M_2M_4 and narrowed along M_1M_3; the moon's action on the ocean tends to cause low water under the moon and opposite, as will presently appear.

(485.) The effect of the radial perturbing forces in diminishing the Earth's pull on the moon, is to increase the moon's period of revolution around the Earth. If the Earth's mean distance from the sun were constant, the period of revolution would also remain constant, so far, at least as this, the chief disturbing force, is concerned. But the Earth's orbit being eccentric, it is manifest that the sun's influence must be greater during that part of the year when the Earth is nearer to the sun than his mean distance, than during the part when she is farther. Hence, when the Earth is in what may be called the perihelion half of her orbit, her influence on the moon is diminished more, and the moon's rate of motion is more reduced, than when the Earth is in the aphelion half of her path round the sun. Hence arises a retardation of the moon which is greatest when the Earth is in perihelion, but acts from mean distance before perihelion to mean distance after perihelion, and so produces the greatest amount of retardation at the latter point, when the moon is about one-third of her diameter behind her mean place; while, compared with the mean motion there is an acceleration acting most effectively at aphelion, but which, since it operates in greater or less degree from mean distance preceding to mean distance following aphelion, attains its greatest value at the latter point, when the moon is about one-third of her diameter in advance of her mean place.

(486.) The peculiarity of the moon's motion thus arising is called the *annual equation*, and was first discovered by Tycho Brahé. The lunar theory, as developed through Newton's discovery of gravitation, fully accounts for it as due to the annual variation of the sun's radial perturbing action.[1]

(487.) A variation of the annual variation, first noticed by Halley, has proved of great interest in the history of the lunar theory. Halley had been led to suspect that the moon had advanced somewhat farther in her orbit than was consistent with ancient observations. Later observations fully justified Halley's supposition. Of this slight acceleration of the moon's motion even gravity seemed till long after Newton's time to give no account. Laplace was the first to show that it resulted from the diminution taking place century after century in the eccentricity of the Earth's orbit.

(488.) Since the radial disturbing force depends on the Earth's distance from the sun, the mean effect of this force in each year depends on the mean distance of the Earth from the sun—not the mean between the greatest and the least, but the mean of all the distances. That the value of this mean distance depends on the eccentricity of the Earth's orbit is shown by the fact that were there no eccentricity the orbit would be a circle having a diameter equal to the major axis of the elliptic orbit, so as to entirely enclose the orbit, extending beyond it everywhere except at the extremities of the major axis.[2] The greater the eccentricity of the Earth's orbit, which varies without any change in the major axis, the smaller becomes the actual area of the ellipse, and the

gravity acting on a slanted top to pull the top over and the real effect *when the top is spinning*, which sets the top moving at right angles to the direction of the Earth's gravitational pull.

[1] The matter is more fully dealt with in the second chapter of my treatise on the Moon. Here, space positively forbids a fuller account. The student should notice, however, that the effects of the sun's perturbing action vary as the cubes of the varying distances; or since the greatest mean and least distances are as the numbers 62, 61, and 60, they vary at these distances as 64, 61 and 58.

[2] Enclosing it in fact as the ellipse is enclosed within its auxiliary circle.

smaller, therefore, the Earth's mean distance from the sun. Hence with increase of the eccentricity there is an increase of the sun's perturbing influence; and therefore, since the Earth's influence on the moon is thus diminished, and the moon's mean period of revolution around the Earth increased, and *vice versâ*, the moon's mean period increases as the eccentricity diminishes.

(489.) Now at present the eccentricity of the Earth's orbit is diminishing; it has been diminishing for a long time in the past, and it will diminish for a long time to come.

(490.) Hence the moon's mean period is diminishing, and the moon's mean motion is undergoing a process of acceleration. The change, called *the secular acceleration of the moon's mean motion*, is very slow. Yet in calculating backwards for ancient eclipses, this slight variation of the moon's motion has to be taken into account, or the records left by ancient historians cannot be explained.

(491.) Although the effects of the *secular acceleration*, unlike those of the *annual variation*, are cumulative and in geometrical progression, yet in a century the moon would be only in advance of the place she would have had if the acceleration had not operated during the century by $\frac{1}{150}$th part of her own diameter. This, the theoretical effect of the gradual diminution of the sun's radial perturbing force, is also about the observed amount of lunar acceleration; and when Laplace had shown this to be the case, astronomers were satisfied that the problem had been completely solved. Unfortunately, when, much later, Professor Adams of Cambridge inquired into the effects of changes in the tangential force in affecting the moon's motion, effects which Laplace had supposed to be self-compensatory so far as the mean motions are concerned, he found that about half the correction obtained by Laplace was cancelled by the effects of the tangential forces. Thus the question was reopened under circumstances of considerable interest. The ranks of mathematicians were for a time divided on the subject: Leverrier, Pontecoulant, and others opposing Adams; Delaunay, Lubbock, Cayley, and others supporting him. He was, however, a host in himself on a question of this sort, and his views were ere long fully established and accepted. They left the question unsolved, however.

(492.) For a long time it was thought that the unexplained half of the lunar acceleration may indicate merely retardation of the terrestrial clock by which we measure the celestial movements; and this retardation was attributed to the action of the tidal wave, which travels always (as an undulation, however, not by the bodily transmission of the water) in a direction contrary to the Earth's rotation. But the latest inquiries into the subject, and in particular the investigations made by Professor Newcomb, of Washington, tend to show that there is a certain degree of irregularity affecting the acceleration as studied in the light of modern observations. Probably the contraction of the Earth's crust tending to produce a gradual but irregular decrease of the Earth's rotation-period, combined with the effect of the tidal wave tending to produce a gradual uniform increase of that rotation-period, lead to the observed irregularity of the change, though on the whole there is at present, and has been for several thousands of years, a gradual but irregular diminution in the Earth's rotation-spin.

(493.) The tangential perturbing forces, presented in fig. 141, act to hasten or retard the moon in her course round the Earth. We see that over the arc M_1M_4 the tangential forces act to retard the moon; over the arc M_2M_3 they act acceleratively; over the arc M_3M_4 they act retardatively; and finally over the arc M_4M_1 they accelerate

the moon's motion. It is further evident that the moon has her greatest velocity at M_1, her least at M_2, her greatest again at M_3, and her least again at M_4 (so far, at least, as the tangential effects are concerned. Hence somewhere near the points m_1, m_2, m_3, and m_4, at the middle of the arcs M_1M_2, M_2M_3, M_3M_4, and M_4M_1, the moon is moving with her mean velocity, and neither gaining nor losing; so that at these points she must have attained her greatest advance or regression. At m_1, the end of the arc m_4m_1, over which the moon has been moving with more than her mean motion, she attains her maximum advance; at m_2, the end of the arc m_1m_2, over which she has been moving with less than her mean motion, she is farthest behind her mean place; at m_3 she is farthest in advance; and lastly at m_4 she is farthest behind.

FIG. 141. –Tangential parts of the Sun's perturbing action on the Moon.

(494.) This inequality in the moon's motion is called the *variation*. It is the most marked of all the monthly lunar perturbations, the moon at M_1 and M_3 being about her own diameter in advance of her mean place, and at M_2 and M_4 as far behind. But the ancient observers failed to recognise the variation, because it occurs in parts of the moon's course with respect to the sun which they did not specially note, their observations being chiefly directed to the moon when new, as at M_1, or full, as at M_3. Tycho Brahé first recognised the variation.

(495.) If the dimensions of the moon's orbit were absolutely inappreciable compared with the sun's distance, the tangential perturbing forces on the half $M_4M_1M_2$ of the moon's orbit nearest the sun would be absolutely equal to those on the half farthest from the sun. But since E M, fig. 136, is equal to nearly $\frac{1}{388}$th part of the sun's distance, and the fraction $\frac{1}{388}$, though small, is appreciable, these two halves of the moon's orbit cannot be regarded in exact inquiries as at the same distance from the sun. The sun's perturbing action on the moon when she is traversing the arc $M_4M_1M_2$ is somewhat greater than his action on her when she is traversing the arc $M_2M_3M_4$. The difference specially affects the tangential action. The moon's advance when at m_1 is slightly yet measurably greater than her advance when at m_3; and her regression when at m_2 is measurably less than her regression when at m_4. Of course, however, the moon's greatest departure, due to this cause, from the place calculated independently of it, occurs when she is at M_2 and at M_4.[1] The discrepancy, called the *lunar parallactic inequality*, is but slight, amounting at its greatest to about two minutes of arc, or little more than $\frac{1}{15}$ of the moon's apparent diameter. But the inequality is full of interest. For, since it depends on the proportion which the sun's distance from the Earth bears to the moon's, its exact determination would enable us to determine the sun's distance. It was in fact from a determination of the sun's distance in this way by Hansen that astronomers were first led to question, in 1851, Encke's estimate of the sun's distance, which had been adopted for half a century. Many regard this method of determining the sun's distance as the most promising of all the various methods available.

[1] Strictly speaking, the arc M_4M_2, if determined by the points where tangents from the sun touch the moon's orbit, is not exactly half the moon's orbit. The points M_2 and M_4 must be regarded as displaced by about $8\frac{3}{4}$ minutes of arc towards the sun, from the extremities of a diameter through E at right angles to M_1M_3.

(496.) The effects of the perturbing forces, radial and tangential, in altering the eccentricity of the moon's orbit and the position of the perigee and apogee (the *apses* or *apsides*), though they can be readily dealt with in such sort as to be understood by the general student without any profound mathematical knowledge, would require more space than can be spared here. I have considered and illustrated them somewhat fully in the first edition of my treatise on the moon, where their study occupies thirty-three pages. Here it must suffice to note that, if we consider the effects of both the radial and tangential perturbing forces when the moon's orbit is situated in all the various positions its major axis can have with respect to E S, we find the observed changes of the eccentricity of the orbit, and the observed varying movement of the perigee, and the advance of the perigee on the whole, so as to complete a circuit in a period of 8·8545 years, fully accounted for.

(497.) The variation of the moon's eccentricity and changes in the position of the perigee cause the greatest of all the inequalities in the moon's motion—the *evection*. This amounts at its maximum to about 1° 18′ (about $2\frac{1}{2}$ times the moon's apparent diameter), by which amount the moon may pass in advance of or fall behind her mean position. The discovery of this inequality is commonly attributed to Ptolemy, but it appears to have been really made by Hipparchus, and doubtless was known centuries before his time to the astronomers of Chaldæa, Egypt, and India.

(498.) The perturbing action of the sun produces changes in the inclination of the moon's orbit and in the position of the nodes, or points of crossing the ecliptic, akin in their general character to those produced on the eccentricity and the position of the line of apses. In fact there is a much greater resemblance between the two pairs of elements than the student is apt at a first view to imagine. The *eccentricity* may be regarded as an inclination of the actual path to a mean circular path, in its own plane; while the *inclination* seems (but only *seems*) more truly so called, because it is an inclination of the real path to the mean path in another plane. Hence we find in the formulæ for changes in the nodes and inclination terms precisely corresponding in character to those obtained in the formulæ for changes in the lines of apses and eccentricity. And in the geometrical treatment of the two problems we find a kindred similarity—as I have shown in the first edition of my treatise on the moon.

(499.) The disturbance of the moon by any one among the planets may be treated in the same general way (though of course with differences of detail) as disturbances by the sun. Much smaller effects naturally result. Still the planetary perturbations have to be duly taken into account, especially those by Jupiter and Venus, in any full study of the lunar theory. Fortunately the nature of the problem is such that the effect of each planet may be considered separately. The most important planetary perturbations arise in cases where there is such an approach to synchronism of motion, that perturbation produced in a given direction at one time is renewed repeatedly at certain definite intervals during long periods.

(500.) We may next consider, since it was an important point in Newton's treatment of gravity, the problem of the tides.

Here it must be noticed at the outset the explanations usually given in books of geography and astronomy, even in some works where better work might be expected, as in Herschel's 'Outlines of Astronomy' and Professor Newcomb's 'Popular Astronomy,' are altogether insufficient, and if not absolutely incorrect fall nevertheless so far short of the truth as to be not less misleading than entirely untrue statements would be.

(501.) In the usual way of presenting the matter, we are shown a picture (fig. 142) of the moon at M, the Earth at E, with the water round the Earth outlined as at H L H′L′, and we are told that because the moon attracts the water at H on the side nearest to her more than she attracts the Earth at E, the water here is raised up and there is high water; while equally, there is high water on the opposite side from the moon, at H′, because the moon attracts the Earth at E more strongly than the water at H′, and so the Earth is relatively drawn away from the water at H′, or the water at H′ drawn relatively from the Earth at E.

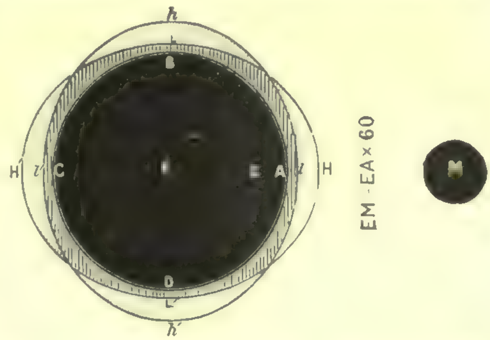


Fig. 142. Contrasting the usual theory of the Tides with the true theory.

(502.) All this is right, only it is better to show more fully what are the relative forces at work, by drawing three such pictures as 137, 140, and 141, where, regarding $M_1M_2M_3M_4$ (now) as the surface of the water, E as the Earth's centre, and the disturbing body as the moon, we find (1) the perturbing forces of the moon on different parts of the water as pictured in fig. 136, (2) the radial (i.e. lifting or depressing) parts of these forces as in fig. 137, and the tangentially pulling portions as in fig. 138. Quite obviously these forces, whether regarded in their totality or in their separate actions, radially and tangentially, tend to give such a shape to the watery envelope of the Earth, supposed complete, as H L H′L′ in fig. 139. It is equally true that, if under the action of the perturbing forces the ocean surface tended to assume such a form, the effects of frictional resistance as the earth rotates, subject always to this influence, would bring the double tidal wave to its observed position. So much Newton pointed out in the first edition of his 'Principia,' not noticing at the time that the explanation had no more real bearing on the actual problem of the tides than, for instance, the statement that gravity tends to pull over a slanted top has on the observed fact that the top does *not* fall over, but retains its inclination unchanged while it reels.

(503.) Some, who imagine, being ill-informed, that the above explanation is not only insufficient but incorrect, have invented another which, though altogether incorrect, is much more than sufficient to account for the tides. They argue that since the Earth really completes an orbit round the centre of gravity, G, of the Earth and moon, situated some 3,000 miles from E, fig. 142, there must be a tendency to an elongation of the water shell in the direction H H′ on account of the excess of the centrifugal tendency of the water at H′. But if the Earth really moved round G in the way supposed, the tides would be thirty times stronger than they are (as calculation readily shows), and the greater part of the Earth would be submerged at high water.

(504.) The real motion of the Earth round G is not such that different parts of the Earth travel in circles round G, but that the whole Earth moves in a circle having radius E G (more correctly in an ellipse resembling the lunar ellipse), each point in the Earth traversing a circle of this size once in each lunar month. This results of course in a centrifugal tendency which is just balanced by the moon's mean attraction. Professor Newcomb (who had been for awhile misled by the incorrect idea just mentioned) appears to consider that in calling attention to this (and

to the kindred centrifugal force in the case of the Earth as she moves round the sun, when considering precession) he was giving an entirely new and sufficient explanation. But as a matter of fact the explanation customarily given necessarily assumes this real centrifugal tendency, the balancing of which by the moon's mean attraction is either left to be taken for granted or definitely referred to (as in Lord Grimthorpe's 'Astronomy Without Mathematics'). The explanation when given in this form is not a whit more complete than as given in the old statical form. Nay, since it in some degree directs attention to the circumstance that the problem is a dynamical one, while leaving altogether unexplained the actual dynamical relations, it may be regarded as on the whole even more disappointing and insufficient than the other.

(505.) In the work just mentioned Lord Grimthorpe has given an excellent rendering of Dr. Young's wave theory of the tides, to which I would refer the student who wishes to view this interesting subject in different aspects. Here I prefer to give an explanation capable of readier illustration, advanced originally by Sir George Airy in 1867.

(506.) But first I would call attention to a comparatively simple yet sufficient explanation of the real relations resulting from the moon's disturbing action on the waters of the Earth.

I have pointed out that the natural idea resulting from the study of fig. 137 would be that the perturbing forces there pictured would tend to lengthen the lunar orbit in the direction M_1M_2. But as a matter of fact this tendency, though real, is more than counteracted by the tendency of the tangential forces. If we imagine a set of small moons set on a circular ring round the Earth, and subjected to the statical pull of the moon, this ring would lengthen itself into an oval having its major axis in the line M_1M_3 or assume a shape such as that of H L H′L′ in fig. 142. But as the moon is in motion, the radial perturbing forces do not permanently draw the moon outwards along the line M_1M_3, or inwards along the line M_2M_4. The increase of distance would be really greatest at the points O_1 and O_3, where the radial force changes the direction of its action from outwards to inwards,[1] having here completed its outward pull, while the decrease would be greatest at O_3, O_4; but in reality any changes of distance the radial forces may thus tend to produce are carried on to other parts of the orbit, and do not permanently affect its form. On the other hand the tangential forces which tend to bring the moon's velocity to its maximum at M_1 and M_3 and to reduce her velocity to a minimum at M_2 and M_4, must manifestly affect the form of the path pursued by the moon, tending to make the curvature least at M_1 and M_3 and greatest at M_2 and M_4, and tending also to send the moon farther out when rounding M_2 and M_4 (owing to the increased velocity at M_1 and M_3), and to draw her farther in when rounding M_1 and M_3 (owing to the reduced velocity at M_4 and M_1). Hence, if there were a ring of free moons all travelling round the Earth in a circle, if undisturbed, the effect of the perturbing forces would be to keep these in an ellipse having its *shorter* axis in direction M_1M_3 and its *longer* axis in direction M_2M_4, or having such a form as *l h l′h′* in fig. 142. In like manner, if we conceive the equatorial parts of the earth surrounded by a sea of free satellites, having, if undisturbed, a surface concentric with A B C D, and these satellites set travelling round the Earth

[1] M_1O_1, O_2M_3, M_3O_3, and O_4M_1 are arcs of 54° 44′. Through some miscalculation Sir J. Herschel in his 'Outlines' sets them at 64° 14′, which is obviously wrong, since the arc is clearly less than 60 degrees.

to accompany her rotation, then, under the perturbing influence of the moon, these satellites would no longer travel in circular paths, but in orbits elongated in direction *h h'*, and thus the surface of the satellite-sea would have such a form as *l h l' h'*.

The particles of the sea are not free; but as they are all subjected to such tangential influences as we have just considered, the water-surface must tend to the form *l h l' h'*, with low water under and opposite the moon.

(507.) Let us turn, however, to Sir George Airy's fuller explanation. I present it in my own way, as his paper was written for a scientific society, and would not, as actually worded, quite suit these pages. I also substitute a single illustration planned by myself for the two given by him.

(508.) Let A B C D (fig. 143) be a section of the Earth's globe, the strong ellipse around A B C D representing the position of the water-surface at a given moment, the

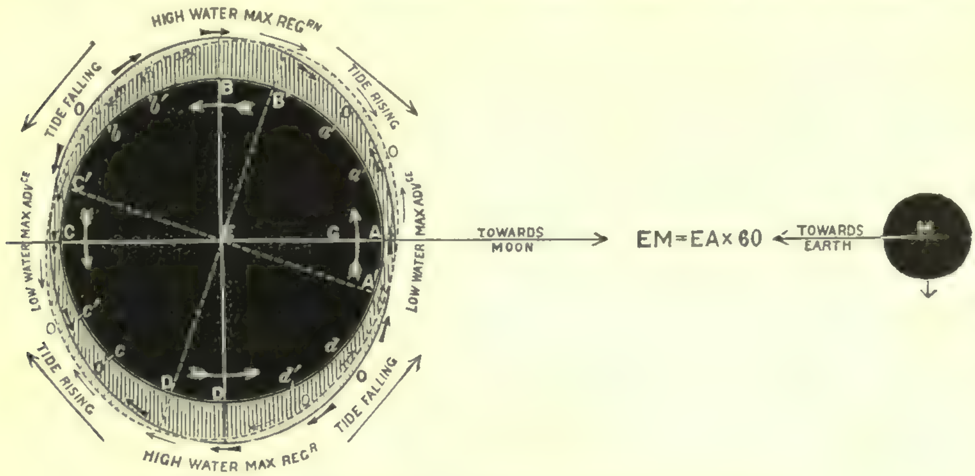


Fig. 143.—Illustrating a dynamical and complete explanation of the Tides.

dotted ellipse representing the position assumed by that surface a very short time after. Then we have to find where the moon must be that her attractions may produce the observed changes; and therefore we must ascertain what those changes are:—

(509.) At A, B, C, and D there has been no change of level (if the time-interval be taken very short: it is, of course, exaggerated in the figure). At *a* and *c* the water has risen, at *b* and *d* it has fallen, more rapidly than anywhere else. Now the tide can only rise through the flowing in of water from both sides, or through an inflow more rapid than the outflow; and the tide can only fall through the flowing out of water towards both sides, or through an outflow more rapid than the inflow. Where the tide is rising most rapidly there must be inflow from both sides; where it is falling most rapidly there must be outflow on both sides. Hence at *a*, *b*, *c*, and *d* the horizontal flow of the water must be in the directions shown by the short heavy arrows there, the horizontal flow changing in direction at these points—the momentary cessation of the flow before the change being indicated by zero marks. On the other hand, at each of the points A, B, C, and D there must be horizontal flow in one direction only, the direction of flow at each point being that shown by the arrows, since the points A, B, C, and D lie between the points *d*, *a*, *b*, and *c*, where the horizontal flow has changed direction as already shown: moreover, at A, B, C, and D the horizontal flow must be more rapid than anywhere else.

(510.) Applying the same considerations to the subsequent case, where the dotted

ellipse indicates the outline of the sea-surface, and setting in arrows to mark directions, and zeros to indicate the momentary cessation of the horizontal flow, followed by change of direction, we get the lighter set of arrows and zeros at a', b', c', and d', the same directions remaining at A′, B′, C′, and D′ as at A, B, C, and D.

(511.) Comparing the flow at a and a', we see that advance has been altered into rest (for opposite the advancing arrow on the strong curve we have a zero mark on the dotted or subsequent curve) and rest into regression (for opposite the zero on the strong curve we have a regression arrow on the dotted or subsequent curve). Hence at a there has been retardation. In like manner, noting the changes at b, c, and d, we see that there has been acceleration at b, retardation at c, and acceleration at d. The forces which have acted then have been such as are indicated by the large outside arrows.

(512.) Now, comparing fig. 143 with fig. 141, we see that the only positions in which the moon can exert the accelerative and retardative forces thus indicated are either as at M, fig. 143 (her distance much greater), or on the side opposite M. In other words, there is low water at places under the moon and opposite.[1]

No account has been taken here of the action of the head of water at B and D. This obviously tends to bring the water down from its greatest height at these points, and the actual gravity of these water-masses is increased by the radial action of the moon, which, as we see from fig. 137, here assists terrestrial gravity. Hence the tidal wave is increased in height through this cause.

(513.) Thus far we have taken no account of the resistance produced by friction, which, though not readily determined as to its amount, must be considerable, and is unbalanced by any of the forces in operation.

(514.) It is evident that friction is greatest at B and D, fig. 143, where the water is highest and deepest, and where also its direction of motion is contrary to that of the Earth's rotation. It is also obvious that friction tends to throw the wave-head at B towards C and beyond C, the tangential force helping friction over the arc B c, and the changed direction of friction over b C not availing to counterbalance the stronger frictional action over the arc a B b. But somewhere on the arc C D the combined effect of the tangential force and opposite friction will bring the wave-head to rest. Hence the greater axis, which in the statical case would have the position $k\,k'$, fig. 144, the moon being at M, and were there no friction would have the position $h\,h'$ in the actual

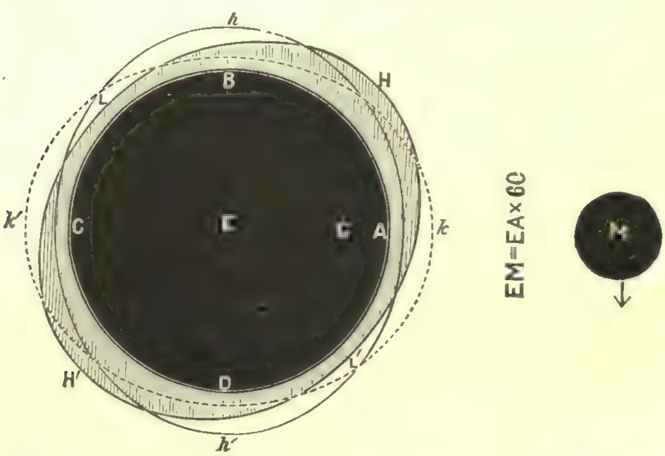


Fig. 144.—Illustrating the actual position assumed by the Tidal Wave.

[1] We get appreciably the same effect (1) by considering the moon's action on the ocean directly, (2) by supposing the moon divided into two equal parts, and conceiving one of these moving as the moon does, and the other always opposite the moon's real place and at the same distance from the Earth.

dynamical case, assumes under the action of friction the position H H′, high water at H appearing to follow the moon (whose motion from east to west, *relatively to the rotating earth*, is represented by the arrow). It appears then as though the statical wave-heads at k and k' (as purely imaginary in reality as the fall of an inclined top while in full spin) had lagged from k to H, and from k' to H′, whereas in reality the wave-heads at h and h' have been thrown back with reference to the direction of the tidal wave, forwards with reference to the Earth's rotation, to H and H′ respectively.

(515.) Similar considerations apply to the tidal wave raised by the sun. This, though less than the lunar wave, is nevertheless considerable. The action of the sun is greater, at equal distances, than that of the moon, in proportion as his mass is greater, or in round numbers 26½ millions of times. His direct pull on the waters of the sea is less than it would be at the moon's distance as the square of his distance is greater; but as the ratio which the Earth's radius bears to the distance of the attracting body has manifestly to be considered, the actual magnitude of the perturbing forces being directly proportioned, *cæteris paribus*, to this ratio, it follows that we must diminish the sun's tide-raising influence as the cube of his distance is greater, the Earth's radius being contained oftener in the sun's distance than in the moon's in direct proportion to the greater distance of the sun. Thus the sun's tide-raising influence is to the moon's as 26½ millions is to 388·4 cubed, or as 265 to 586, or roughly as 28 to 61. As actually observed, it would seem that the height of the solar tidal wave bears to that of the sun about the ratio 5 to 2. Thus the spring tides, when the lunar and solar tidal waves conjoin their waters, are proportioned to the neap tides, when the lunar wave is diminished by the whole amount of the solar wave, theoretically as 61 + 28 to 61—28, or as 89 to 33, and actually about as 7 to 3.

(516.) The precession of the equinoxes was another important problem whose explanation helped to establish the theory of gravitation.

Here, however, we must first take into account a circumstance which hitherto has not been noticed. So soon as Newton had advanced the theory that gravitation is a property of matter as such, it became necessary to inquire how far the heavenly bodies could be assumed to attract each other as if their masses were collected at their respective centres. Newton showed that for homogeneous spheres, or for spheres composed of concentric shells each uniform in density, this is the case; while for spheroids acting as the sun acts on the planets or as the planets act on the sun, the proposition is sufficiently near the truth. But in the case of a body like the Earth, acted on by the moon or by the sun, although the movements of the centre of gravity may not be appreciably affected by the spheroidal form of the planet, the axial pose of the Earth must be markedly affected, owing to the divergence of the mean pull exerted on the Earth from the direction of the Earth's centre.

(517.) Let us first, however, consider the way in which a homogeneous globe, or a globe composed of concentric homogeneous shells, acts on a particle, first within it, and secondly outside it.

Let p be a particle inside the exceedingly thin spherical shell A B C. Let $a\,p\,b$, $a'p\,b'$ be drawn through p, and suppose p the common apex of the two parts of an exceedingly thin double pyramid $a\,p\,b$, whose faces cut out minute elements of the shell at a and b. Then, because $a\,k$ and $l\,b$ are equal, and the bases $a\,a'$ and $b\,b'$ of the two pyramids have areas proportioned as $(a\,p)^2$ to $(p\,b)^2$, the volumes and masses of the two elements at a and b are proportioned as $(a\,p)^2$ to $(p\,b)^2$; and their gravity acting

on the particle at p inversely as the squares of the distances $a p$ and $p b$, it follows that they exert equal attractions in opposite directions. Hence, this being true of every pair of opposite elements of the shell A B C, it follows that the attractions exerted by this shell on p are perfectly balanced, or there is no resultant force on p. This being true of every thin shell around C as centre and passing outside p, is true of any shell, either homogeneous or made up of homogeneous concentric shells and passing outside p: if the sphere D A C B is solid, and either homogeneous or made up of homogeneous concentric shells, the part of it outside the concentric sphere $p m n$, whose surface passes through p, exerts no attraction on the particle at p, which therefore is acted on precisely as though attracted only by the sphere $p m n$. (This is not saying that the matter of the shell A C B $p m n$ produces no *pressures* on p; of course the weight of superincumbent matter such as that in the region p D, if the globe A C B represents the Earth, would produce great pressure on p, unless the weight were sustained by some exceedingly strong wall or shield.)

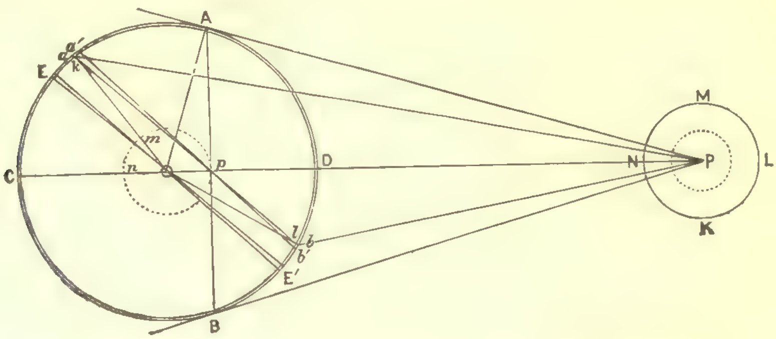


FIG. 145.—Attraction of a Spherical Shell on a point within or without it.

(518.) The reasoning in the above case applies equally if A C B represents a spheroidal or ellipsoidal shell forming part of an ellipsoid or spheroid A C B, so only that the inner and outer surfaces of the shell A C B and of all component shells dealt with are concentric and similar.

(519.) The case of a point outside cannot be quite so readily dealt with; and the law of attraction is only simple in the case of a *sphere* either homogeneous or composed of concentric homogeneous shells.

Suppose P, fig. 145, a particle outside the sphere A C B, the figure representing any section through the centre O. Draw tangents P A and P B, and join A B cutting P O C in p, and let a p b be a double elementary pyramid as in the former case. Join O a, O b. Now note that since O A P is a right angle, the rectangle O p, p P is equal to the square on A p, which is equal to the rectangle $a p$, $p b$; wherefore the points O, a, P, b lie on a circle: hence the angles a P O and b P O on the equal chords O a, O b are equal; the angles P a b, P O b are also equal; wherefore the triangle a P p is similar in all respects to the triangle O P b, and the triangle a O P to the triangle p b P.

Now let E O E′ be drawn parallel to $a p b$, and let an elementary double pyramid E O E′ be taken, having its small equal solid angles at O equal to the small equal solid angles of the double pyramid $a p a'$ at p; and let the equal elementary solids cut from the shell A C B by the double pyramid E O E′ have each a mass represented by e. Then it is easily shown that the attraction exerted on the particle at P

by the elementary masses at a and b is equal to what would be exerted by the two masses at E and E′, if the whole mass of each were collected at the point O.[1]

This being true of every pair of elements of the shell A C B, such as a and b, it follows, by taking the whole shell in such elements (by which obviously the corresponding elements such as those at E and E′ would also make up the whole shell) that the attraction of the shell A C B on the particle at P is the same as though the whole mass of the shell were collected at O. And, as in the former case, we see that the same is true for a shell of finite thickness or for a globe, whether homogeneous or formed of concentric homogeneous shells.

Moreover, it is clear that, since what is true of a unit of mass at P is true of every particle of a sphere such as L M N K, this sphere is acted upon by a spherical shell or globe having its centre at O, precisely as though the whole mass of the shell or globe were at O. And in like manner every particle of a globe or spherical shell having its centre at O is attracted by a globe or spherical shell having its centre at P, as if the whole mass were collected at P.

Hence, finally, two globes or spherical shells, either homogeneous or made up of concentric homogeneous shells, attract each other as if the whole mass of each were collected at its centre.

(520.) But this is not the case (though usually it is approximately the case) with spheroids. If p E p'E (fig. 146) be an oblate spheroid of homogeneous material whose axis is $p\,p'$ and its equator E O E′, we can regard the globe $p\,e'p'e$ as attracting a body at P as if its mass were all collected at O; but of the rest of the mass of the spheroid, the part on the side p E p' will obviously attract P more than the part on the side p E′ p'. The resultant attraction of the spheroid on P, and also that of P on the spheroid, will act along such a line as c P, c being a point on E′E very near O, towards E. We may, however, more conveniently consider the attractions of different parts of the equatorial protuberances precisely as we considered the perturbing attractions on the moon, as illustrated in fig. 136; we then find them tending to pull E downwards towards O P, and to thrust E′ upwards towards the same line. Thus, considering the pull of the mass P on the spheroid, there will be a tendency to bring the axis $p\,p'$ towards the position A B, a tendency which will not, however, produce this effect if the spheroid is rotating with due rapidity, any more than terrestrial gravity will make a slanted top fall over if the top is spinning swiftly on its axis.

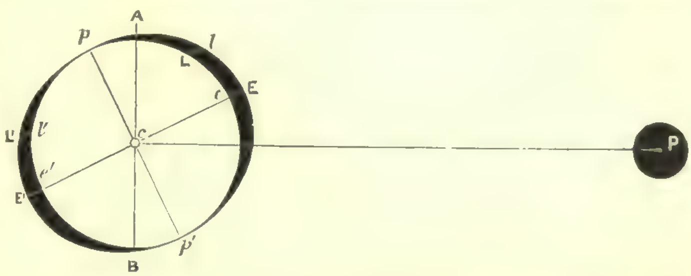


Fig. 146.—Effect of the Earth's spheroidal form on the direction of attractions excited upon it.

[1] Thus: The elements at a and b have the respective masses—

$$e\left(\frac{p\,a}{\mathrm{O\,E}}\right)^2 \sec \mathrm{O}\,a\,b \text{ and } e\left(\frac{p\,b}{\mathrm{O\,E}}\right)^2 \sec \mathrm{O}\,b\,a\,;$$

hence the sum of their attractions on P, resolved in direction P O, is—

$$e\,\frac{\sec \mathrm{O}\,a\,b}{(\mathrm{O\,E})^2}\left[\left(\frac{p\,a}{\mathrm{P}\,a}\right)^2+\left(\frac{p\,b}{\mathrm{P}\,b}\right)^2\right]\cos a\,\mathrm{P\,O}$$

$$=\frac{e}{(\mathrm{O\,E})^2}\left[\left(\frac{\mathrm{O}\,b}{\mathrm{O\,P}}\right)^2+\left(\frac{\mathrm{O}\,a}{\mathrm{O\,P}}\right)^2\right]$$

$$=\frac{2e}{(\mathrm{O\,P})^2}$$

(521.) Since the Earth does rotate rapidly on her axis, we have to consider next the effects of a pull such as that along c P on a rotating mass.

Let A B A′B′ (fig. 147) be a ring of uniform thickness and density, rotating rapidly in its own plane round its centre C in the direction shown by the feathered arrows. Suppose external forces, represented by the arrows A a, D d, &c., act on this ring in such a way that, if it were at rest, it would move round the diameter B B′ towards such a position as a B a'B′. As the ring is rotating, it clearly will not shift in this way; for if it did, then in the position a B a'B′ the directions of motion would have been most changed near B and B′—namely, from E B F to e B f, and from K B′ L to k B′ l—precisely where the external forces had been least: while near A and A′, where those forces had been greatest, they would have produced no change of direction at all; for the motion at a and a' would be parallel to the motion at A and A′ respectively. It is manifest that the particles at A moving towards Q, and acted upon as they pass A by the force A a, will move in such a direction as A q; those at A′, instead of continuing their motion towards Q′, will move towards q'; those moving at B and B′, not being acted on by any force, will not change their direction of motion, though, owing to influences affecting the ring bodily in compelling changed direction of the motions of the parts, its particles at B and B′ may be shifted bodily, still retaining their directions of motion. Manifestly then the rotating ring will take up such a position as A b A′b'; for in this new position the particles of the ring will be moving in the directions (more or less changed) resulting from the external forces.

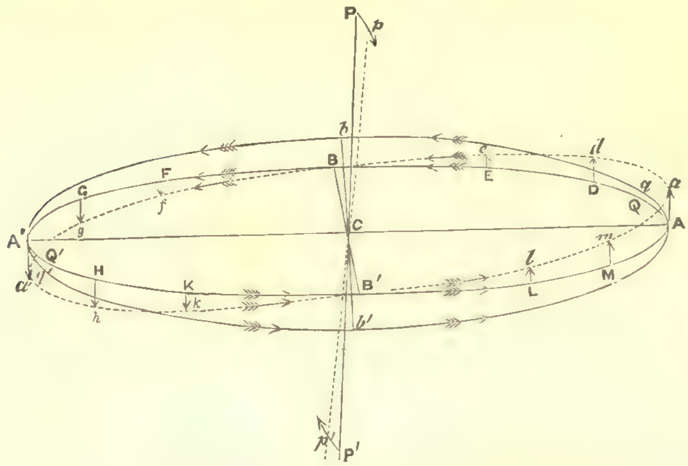


FIG. 147.—Comparing the effects of the same external force on a Ring at rest and rotating in its own plane.

(522.) The same will be true of every body rotating on its axis of figure, since every such body may be supposed to be made up of multitudinous such rings as A B A′B′ round the axis of figure (P C P′), all affected by such forces as A a, A′a', &c., when the whole body is acted on by external forces tending to shift it, were it not rotating, around B B′ or round an axis parallel to B B′. In every such case, whether the body be a spinning top, the disc of a gyroscope, a whirling ring or spheroid or planet, any force tending to rotate the body, were it at rest, round such an axis as B B′, will shift it, if it is rotating, round an axis A C A′ at right angles to B B′; a point P, on the axis of rotation, moving parallel to the particles passing A′ (where the external force has its greatest value *from* the side of A A′ on which P lies), while a point P′ on the axis on the other side of A A′ moves parallel to particles passing A. The axis of rotation takes up therefore such a position as p C p'.

(523.) In every such case, in order to ascertain at once in what direction a change will take place when a rotating body is acted on by external forces, let the student suppose he is looking at a side of the rotating body where the external action exerts its

greatest effect. Then the new direction along which the particles will move in that part of their circuit will lie between their original direction and the direction in which the external force acts.[1]

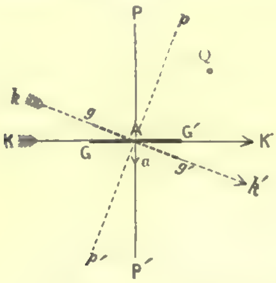


Fig. 148.—Shifting of a rotating body under the action of external forces.

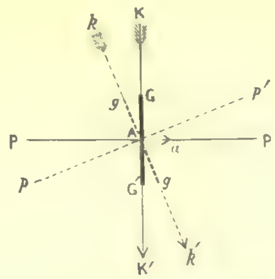


Fig. 149.—Another example of the same.

(524.) We are now in a position to deal with the case of the rotating spheroid of the Earth under the influence of the perturbing forces exerted by the moon chiefly, next by the sun, and lastly by the planets, though indeed the influence of these is so slight as to be scarcely appreciable. If M, fig. 150, is the moon, O the Earth's centre, and E e E e' a ring of matter round O as centre forming part of the Earth's equatorial protuberance, the intersection $e\,e'$ of E e E′ e' with the plane of the moon's orbit being supposed to be at right angles to O M, and therefore the inclination of E e E′ e' to M O at its greatest. Then we have seen that a force is exerted which tends to pull the parts E and E′ towards

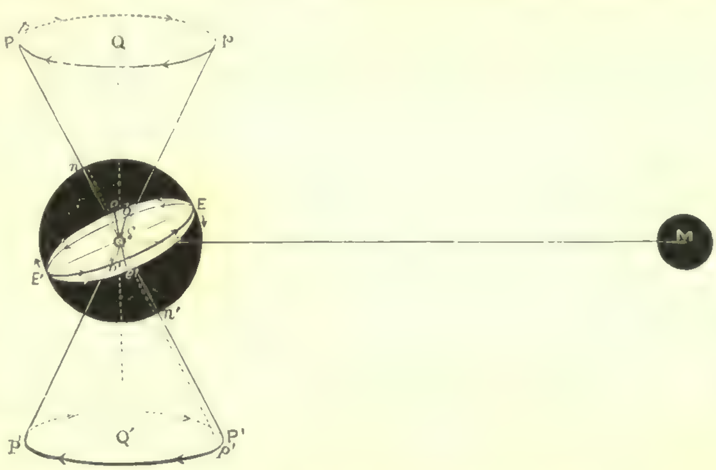


Fig. 150. Illustrating the Precession of the Equinoxes.

[1] Thus, suppose K A K′, in figs. 148 and 149, a part of the body moving in the direction shown by the feathered arrows, the arrow on A P in each figure showing the direction of an external force. Then $k\,k'$ represents the direction in which the action of this force will cause points passing A to move. If P P′ represent the direction of the axis before the external force acted, a line situated as $p\,p'$ will represent the direction of the axis afterwards. This rule will be found very simple of application to all cases. Suppose, for instance, A, fig. 148, is a point on the nearer half of an inclined top, whose axis of rotation is P P′—P nearer the eye than P′, which may represent the end of the peg. The action of gravity tending to pull the top over is represented by A a; an opposite force acts on the other side, the real action being that of a 'couple.' Then $p\,p'$ will represent the direction which the axis of the top will tend to take; and since the end of the peg cannot slide, this direction will be assumed by the swaying of the top on its peg P′ till its axis is in the situation P′Q, parallel to P′p; thus we see why a top reels always in the direction of the motion of its under side. Again, if A represents the nearer part of a vertical gyroscope, P P′ its axis of rotation, and A a a force acting at A, the tendency of the external forces being to turn the gyroscope, were it at rest, round a vertical axis, the result of this action when the gyroscope is rotating will be to shift the axis of rotation so that P descends and P′ ascends, $k\,k'$ being the direction in which the part at A will now be rotating.

that plane, in the way indicated by the arrows at E and E′; but as the ring E E′ is rotating in the direction shown by the feathered arrows, the moon's pull does not diminish the inclination of E e E′e' to O M, as it otherwise would, but tends to shift this plane into such a position as E b E′ b'. By this change the line of intersection $e\,e'$ shifts to such a position as $b\,b'$, corresponding to a movement in the plane of the moon's orbit in a retrograde direction, or contrary to the direction in which the Earth is rotating on her axis and revolving round the sun. If P P′ represent the direction of the Earth's axis at first, this direction is shifted as to p O p', p lying on the farther side of P and p' on the hither side of P′.

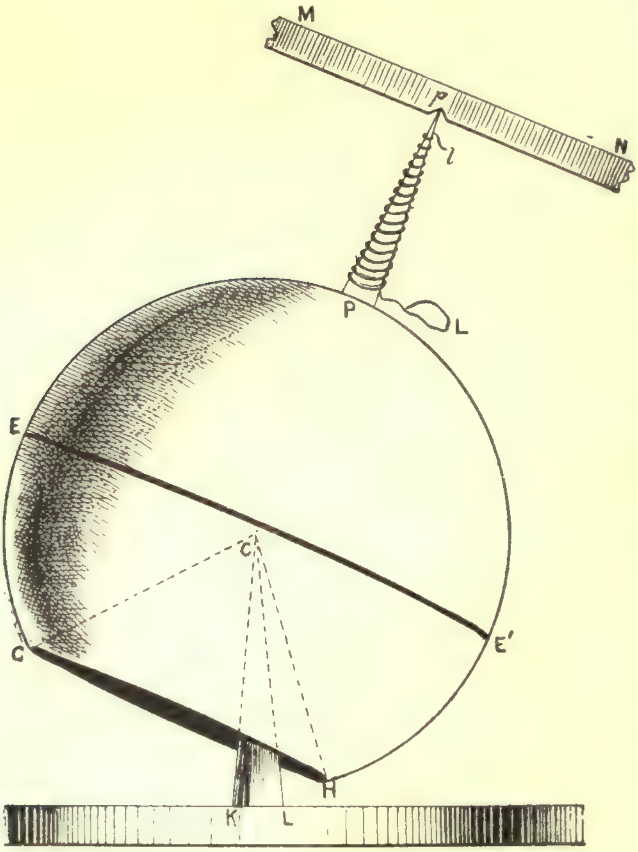


FIG. 151.—A practical way of illustrating the Precession of the Equinoxes.

(525.) It is clear further that so long as the plane E e E′e' is at all inclined to O M the motion of this plane will be thus precessional, though less and less as the inclination is less, and vanishing when the plane E e E′e' passes through the moon, or, in other words, when the moon is crossing the equator.[1]

(526.) Thus, supposing Q Q′, fig. 150, to be the poles of the moon's orbit, and that these poles remained unchanged so far as the moon's action is concerned, we see that P, P′, the poles of the equator, would traverse the two circles P p p and P′p p′, in the direction shown by the arrow-heads, with varying velocity, travelling faster the farther the moon was from the celestial equator, and only resting momentarily as the moon crossed the equator. The period of circuit on this supposition, supposing the moon's path at its mean inclination to the equator, would be about 36,000 years.

(527.) As a matter of fact, however, the pole of the moon's orbit-plane is not at rest, as we have supposed, but describes a nearly circular path, having a mean radius (measured on the sphere) of 5° 9′ around the pole of the ecliptic, in a period of about

[1] A good illustration of the precession of the equinoxes (regarded as uniform and unaffected by nutation) may be obtained as shown in fig. 151. The upper part of the globe G E′P E′H should be of lighter material than the lower, so that, despite the conical space G C H cut out from the lower half, the centre of gravity when the projecting spike P p is made upright may lie slightly below C. The globe is set spinning in a slightly inclined position by drawing the string L l (M N is part of a handle for holding the globe in position against the spike K C L while setting it spinning). Then the action of gravity were the globe not spinning would tend to restore it to uprightness. The case corresponds then with the action of the moon on the rotating Earth; but it will be found that p moves in a horizontal circle in a direction contrary to that of the globe's rotation.

18·6 years. Here we may take the path of this pole as a circle, $Q_1Q_2Q_3$, fig. 152, round C, the pole of the ecliptic,[1] and taking Q_1C to P_1C as 5° 9′ to 23° 27′, we have $P_1P_2P_3$ as the mean path traversed by the pole of the Earth's rotation; but the actual path only crosses this mean path at points such as a, b, &c. corresponding to the positions A and B of the pole of the moon's path at about the same distance as C from a and b respectively. When at a, the motion of the Earth's pole round A, being at right angles to A a, carries the pole outwards, while at b, where it moves at right angles to b B, it is carried inwards, across the circle $P_1P_2P_3$. At an intermediate point, p_2, where the pole of the Earth's rotation is moving round Q_2, the motion is neither outwards nor inwards, and here the

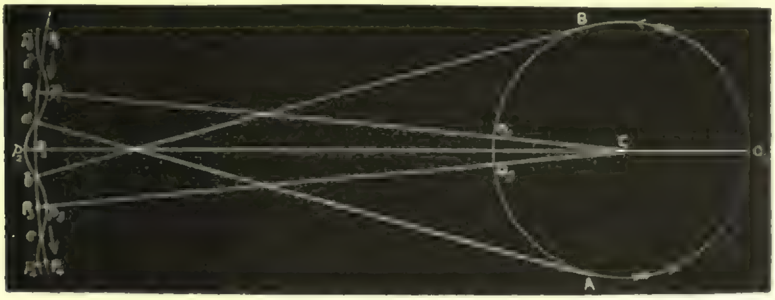


FIG. 152.—Precession and Nutation.

pole p_2 attains its greatest distance from C. At the points p_1 and p_3 the pole of the earth's rotation draws nearest to C, its motion being around Q_1 and Q_3 respectively. Thus, so far as lunar precession is concerned, the pole of the heavens would traverse an undulating curve such as $p_1 a\, p_2 b\, p_3$ . . ., passing alternately inside and outside of the circle $P_1P_2P_3$, in a period equal to that of the mean revolution of the moon's nodes, or about 18·6 years. (For the angle $Q_1C\,Q_3$ may be neglected in this inquiry, corresponding as it does to the minute precessional arc traversed by the Earth's pole in 18·6 years.)

(528.) The precession due to the sun's action, equal to about two-fifths of the mean lunar precession, or, more exactly, to $\frac{2}{5}\frac{6}{7}\frac{5}{7}$ (for this proportion is the same as for the tidal wave, dealt with in Art. 515), is precisely the same in character. We have only to regard M, in fig. 150, as representing the sun, and the reasoning is the same. But as the pole of the ecliptic is almost unchanging in position, we have not to consider such motion as along the undulating curve $p_1\, p_2\, p_3$, but simply motion along the mean circle $P_1P_2P_3$, this motion being more rapid according as the Earth is more inclined to the line joining the Earth and sun—attaining its maximum, therefore, at the solstices and vanishing at the equinoxes. The pole of the heavens would make a complete circuit round the pole of the ecliptic in about 90,500 years under the influence of solar action alone.

(529.) Under lunar and solar actions combined the pole of the heavens completes a circuit round the pole of the ecliptic in about 25,868 years on an undulating curve such as is shown in fig. 152, the only effect of the solar precession on this undulation being to increase the range P_1P_3 about as 7 to 5, leaving the breadth of the undulation $(2P_2p_2)$ unchanged. The arc P_1P_3 is in reality one of only about 15′ 32″, there being no fewer than 1390 undulations in the complete circuit. The distance $P_2\,p_2$ is about

[1] The motions represented in fig. 152 seem contrary to those shown in fig. 150, because in fig. 152 we are supposed to be looking at the vault of heaven from within, in fig. 150 to be looking at the precessional circuit of P_1 from without.

$9\frac{1}{4}''$, so that the breadth of the undulatory curves such as $p_1p_2p_3$ is about $18\frac{1}{2}''$. As the lunar precession is greater according as the pole of the moon's orbit is farther from the equator, it follows that the lunar precessional motion (so far as this variation is concerned) is at a minimum at p_1, p_3, &c., and at a maximum at p_2, p_4, &c., less than the mean along $d\,p_1a$, greater than the mean along $a\,p_2b$, and so on. Hence the pole of the heavens is behind its mean position when at a, in advance of its mean position when at b, and so on—*always* so far as the change due to the motion of the pole of the moon's orbit is concerned. The variation in distance from C, and in the advance going the precessional circuit, may both be represented thus: Suppose that while a point moves round the circle $P_1P_2P_3$, once in 25,868 years, with the precessional movements due to the action of the sun (varying, as we have seen, from nought at the equinoxes to a maximum at the solstices) and to the action of the moon (varying from nought when she crosses the equator to a maximum when she reaches her greatest distance from that circle, but with the inclination of her path to the ecliptic assumed to be constantly at its mean value), the pole of the heavens moves in a small ellipse round this circularly moving point, the major axis of the ellipse (directed always towards the pole of the ecliptic) being about $18\frac{1}{2}''$, and its minor axis $11\frac{3}{4}''$, and a circuit being completed in about $18\frac{3}{5}$ years.

(530.) The apparent motion of the pole of the earth's rotation round this small ellipse is called the *nutation* (or nodding) because it really corresponds to a nodding motion of the polar axis of the earth. The apparent displacement of the stars due to this cause, corresponding to the motion of each star in an ellipse similar to that described by the celestial pole about its mean position, was discovered by Bradley in 1747; or rather, it was in that year that, after twenty years' observation of the phenomenon, he completed and published the discovery with its explanation.

(531.) It should be remarked that the moon's action and the changes in the moon's action affect the obliquity of the ecliptic, though not altering its mean value. For the lunar action, which tends to shift the position of the equator without altering its inclination to the plane of the moon's orbit at the moment, necessarily shifts the inclination of the plane of the equator to the ecliptic, in varying ways and degree, as the plane of the moon's orbit varies in position.

(532.) In Plates VII. and VIII. the mean movements of the poles of the earth's rotation round the poles of the ecliptic are shown for long periods of time in the past and in the future. No account could be taken of nutation on the scale of such maps; and I did not deem it desirable to introduce complexity by taking account of the slight changes by which the position of the pole of the ecliptic and the amount of the ecliptic's obliquity have been affected within the last few thousands of years. I shall have occasion to speak more particularly of the teachings of these two maps in dealing with the constellations and the changed aspect of the stellar heavens, with reference to terrestrial horizons, since the time of the Great Pyramid. For the present I leave the student to examine these maps with reference to the effects of luni-solar precession, by which the poles of the heavens each circuit the poles of the ecliptic in a period of 25,868 years, and in a direction contrary to that in which the sun, moon, and planets make their respective circuits of the zodiac.

(533.) The law of universal gravitation as established by terrestrial experiments, by the explanation through it of the movements of the planets round the sun, of the moon round the Earth, and of the other satellites round their primaries, and lastly

by the explanation of such phenomena as the perturbations of the moon's motions, the tides, precession and nutation, &c. may be presented as follows:—

LAW OF UNIVERSAL GRAVITATION.—*Any two portions of matter in the universe attract each other with a force proportional directly to the product of the numbers representing their masses, and inversely to the square of the number representing their distances.*[1]

(534.) The law of universal gravitation and its consequences disposed finally of all doubts as to the real centre of the planetary motions; for the sun's attractive energy, which rules the planets, scarcely influenced at all by the Earth, must rule the Earth also. And the demonstration of the Earth's revolution carries with it necessarily the demonstration of her rotation also, and of such other movements as the precessional gyration and the nutation. It may be well, however, to close this chapter with a brief sketch of some proofs of the movements of the Earth which were supplied, directly or indirectly, through the demonstration of the law of gravity.

(535.) One of these proofs of the Earth's rotation is founded on the varying attraction of gravity in different parts of the Earth's surface. We have seen that the Earth's figure is that of a somewhat flattened sphere. If then a pendulum is carried from high latitudes to the equator, the distance of the pendulum from the centre of the Earth continually increases. It is not difficult to determine according to what law gravity diminishes in consequence of this increased distance, but the calculations would be unsuited to these pages.[2] Suffice it to say that gravity at the equator would be about 1-289th less than gravity at the poles, if the Earth were not rotating. Instead of this, gravity at the equator is found to be less than gravity at the poles (or rather than gravity as calculated for the pole from the observed gravity in high latitudes) by fully 1-195th part. The difference is equal to 1-195 − 1-289, or 1-599, and is not much less than half the estimated decrease, so that it is a quantity far too important to be neglected. It is readily calculable that the diminution of weight due to the centrifugal force at the equator, if the Earth really rotates once in a sidereal day, would be exactly 1-599th part. In other words, if the Earth were a perfect sphere, a body at the poles would weigh more than a body at the equator in the proportion of 599 to 598, supposing the Earth really to be rotating. Since, then, a body at the equator weighs less by 1-599th than it should weigh on the supposition that the

[1] This enunciation of the law implies, however, that the distance separating the attracting bodies is so great that compared with it the dimensions of the bodies may be neglected. Strictly speaking, the law as thus worded is only true when the portions of matter are particles. However, for ordinary purposes this wording suffices. If the two masses are m and m', and the distance between them d, the attraction exerted by each on the other is $m\,m' \div d^2$, and the accelerating forces exerted by the two bodies respectively on each of the other are respectively $m \div d^2$ and $m' \div d^2$.

[2] The matter is dealt with in Todhunter's *Analytical Statics*, chapter on 'Attraction.'

Earth does not rotate, we have direct evidence of great weight in favour of the view that the Earth does rotate.

(536.) The evidence derived from the descent of bodies from a considerable height is one of the most interesting of all the proofs of the Earth's rotation.

The general principles on which this proof depends are easily explained. The point of suspension moves faster (if the Earth is rotating) than the point on the Earth's surface vertically below it. Hence, if a body be let fall from a height it will not fall directly to the point vertically below, but will be carried by the excess of its eastwardly motion somewhat towards the east. The eastwardly displacement will be very small because the height of the point of suspension must always be very small compared with the Earth's radius; still it has been found possible, by very delicate appliances, to render sensible the displacement of a falling body towards the east (the direction of the Earth's rotation).[1]

[1] The following geometrical solution presents the falling body as independently obeying the Earth's attraction, like a small moon moving in a very eccentric orbit intersecting the Earth's globe.

FIG. 153.—A falling body regarded as a satellite of the Earth.

Let A (fig. 153) be the point of suspension, B the point vertically below it, C the Earth's centre, B E F the Earth's surface along a great circle through B A and touching the latitude-parallel (or small circle) through B, so that B F may be regarded as part of this latitude-parallel. The body falling from A, with such eastwardly motion as belongs to the point of suspension A, travels in an elongated ellipse A F A′, having C, the Earth's centre, as a focus, and reaches the ground at F, the arc A P F being appreciably parabolic. Suppose that while this descent is taking place the point of suspension A is carried by the Earth's rotation to D, and join D C and F C, D C cutting B F in E, and A P F in P. Also let the arc A D produced meet C F produced in G.

Then, since the point of suspension A, and the falling body when just leaving A, are sweeping out equal areas around C, and continue to sweep out areas uniformly during their motion (the former because of the uniform rotation of the Earth, the latter by Kepler's second law), it follows that

$$\text{Area } ADC = \text{area } AFC.$$

Whence, taking away from each the area A P C

$$\text{Area } APD = \text{area } CPF.$$

Now (since P E and E F are each very small compared with A B, B F, &c.)

$$\text{Area } APD = \tfrac{1}{3} PD.AD$$
$$= \tfrac{1}{3} AB.BE;$$

and $\quad \text{Area } CPF = \text{area } CEF;$

whence $\quad \dfrac{1}{3} AB.BE = \dfrac{1}{2} EF.BC;$

or $$EF = \frac{2}{3}\frac{AB.BE}{BC}.$$

If we put $AB = h$, $CB = r$, time of fall $= t$, Earth's rotation period $= P$, and latitude of $B = \lambda$, then

$$BE = 2\pi r \cos\lambda.\left(\frac{t}{P}\right)$$

and the easterly deflection

$$EF = \frac{2}{3}\cdot\frac{h}{r}\cdot 2\pi r\cos\lambda.\left(\frac{t}{P}\right)$$
$$= \frac{4\pi h t}{3P}\cos\lambda$$

But $$t = \sqrt{\frac{2h}{g}};$$

hence, finally, the eastwardly deflection

$$= \frac{4\pi}{3P}\sqrt{\frac{2h^3}{g}}\cos\lambda = (\cdot 00001208\cos\lambda.)\, h^{\frac{3}{2}}$$

the second and the foot being the units of time and space respectively.

(537.) Newton suggested this method of proving the Earth's rotation, and Dr. Hooke, then Secretary of the Royal Society, carried out the first experiment. He dropped a number of balls from a height of 27 feet and found that they fell towards the south-east. This result, however, though accordant with theory, cannot be depended upon, since the theoretical deviation for this height is only about the fiftieth part of an inch.

(538.) Guglielmini, 113 years later, next tried the experiment in the tower Degli Asinelli, at Bologna. The fall was about 300 feet; but he found that his experiments were interfered with during the daytime by the vibrations of the tower. He therefore worked at night, and only when the air was perfectly calm. The suspended balls were examined with a microscope until it was found that all oscillation had ceased; then they were liberated by burning the thread which held them. They fell on a cake of wax. So truly were the experiments conducted that the greatest deviation between the holes made by the balls did not exceed half a line. Guglielmini then had to determine where the true vertical would fall, and for this purpose he dropped a plumb-line in August, 1791. Some idea of the difficulty of the problem and of the delicacy of the operations necessary to secure success will be suggested by the fact that he had to wait half a year before his plumb-line came to perfect rest. He found then that the deviation of the balls he had dropped (16 in all) was 7·4 lines towards the east, and 5·27 towards the south.

(539.) The deviation should have been but about five lines towards the east, and none towards the south. On inquiry it was found that the difference might fairly enough be ascribed to two circumstances—first, the fact that the walls of the tower were perforated in different places, and the air consequently disturbed by currents; secondly, the fact that whereas the balls were dropped in the summer, the plumb-line was let fall in the winter, and the tower would doubtless be affected by the difference of temperature.

(540.) These considerations explained the observed discrepancy, but rendered the experiments valueless. New trials were therefore made with new precautions and new contrivances to render the results more trustworthy.

In 1820, Dr. Benzenberg began a series of experiments in St. Michael's Tower, in Hamburg. The height he was able to command was 340 feet. But he preferred to limit the fall to the portion of the tower which was completely closed, and thus he commanded a range of fall of about 235 feet only. The balls he used were 1½ inch in diameter, made of an alloy of equal parts of lead and tin, and a small proportion of zinc. They were carefully turned and polished, so that there might be no irregularity of figure

to occasion any departure from the line of fall. He tested them specially for this purpose by floating them in mercury, and rejected all those which showed any tendency to float in one position rather than in another. So carefully did he consider all the possible sources of error in the result, that he would not even allow the liberation of the balls to be effected by burning the thread, as Guglielmini had done, lest slight draughts of air thus occasioned might cause the ball to oscillate before being detached. The mode of liberation he actually employed was ingeniously devised to prevent the balls from being in any way influenced by the act of liberation. The suspending thread passed through a vertical aperture in a block, and was clasped above by nippers, opening in a horizontal plane.

(541.) He dropped, first, a number of balls experimentally, to gauge the requirements of the problem. At last, when all the arrangements had been made which these experiments suggested, he dropped 31 balls. Of these

21 fell towards the east.
8 ,, ,, west.
2 neither towards the east nor west.
16 fell towards the south.
11 ,, ,, north.
4 fell neither towards the south nor north.

(542.) The actual deviations carefully summed gave the following results :—

Sum of deviations towards the north	. .	46·4 lines.
,, ,, south	. .	92·6 ,,
,, ,, east	. . .	174·5 ,,
,, ,, west	. . .	50·5 ,,
Balance of deviations towards the south	. .	46·2 ,,
,, ,, east	. . .	124·0 ,,

(543.) As there should be no deviation towards the south, we must take the observed deviation in that direction as measuring the probable errors in the series of experiments. This leaves to be accounted for an obvious tendency towards the east—that is, in the direction according with the theory of the Earth's rotation. The mean deviation towards the east is $\frac{124}{31}$ or 4 lines. Calculation shows that the mean deviation should have been 3·85 lines, so that theory and observation agreed very closely.

(544.) But Benzenberg thought the southerly deviation large enough to render his experiments unsatisfactory. He therefore made new experiments in an abandoned coal-pit at Schlebusch, in Westphalia, with an available fall of 262 feet. He carefully covered the entrance of the pit, and blocked up all

its lower passages before commencing his experiments. He then dropped 29 balls. The results were as follows :—

Sum of deviations towards the	north	. . .	124 lines.
,, ,,	south	. . .	103 ,,
,, ,,	east	. . .	189 ,,
,, ,,	west	. . .	42 ,,

or a mean northern deviation of 0·7 line and a mean easterly deviation of 5·1 lines. The calculated easterly deviation for a fall of 262 feet is 4·6 lines ; so that in this experiment, as in the preceding, theory and observation agreed very closely together. As in these experiments, also, a balance of deviation towards the north was observed, while in the former the balance of deviation was towards the south, we see the more reason to regard northerly and southerly deviations as the result of those errors which can never be altogether avoided in experiments of this sort.

(545.) Lastly, a long and conclusive series of experiments was carried out on the same plan by Professor Reich in the mines of Freiburg. He was able to drop balls to a depth of no less than 488 feet, and he made no fewer than 106 experiments. There was a balance of southerly deviation of 48·76 lines, and a balance of easterly deviation of 1,093·92 lines ; so that the mean deviation towards the south was but 0·46 lines,[1] while the mean deviation towards the east was 10·32 lines.

[1] There must be a certain amount of southerly deviation from the *true* vertical (the reason for this emphasis on the word *true* will appear presently) in such experiments. For the plane in which the body moves as it falls is necessarily that of a great circle through the Earth's centre and touching the latitude-parallel through the place of the experiment, and such a circle through any northerly station passes south of the latitude-parallel.

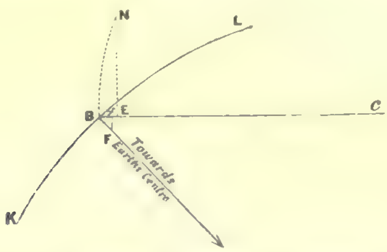


FIG. 154.—Southerly deviation of a falling body.

But the southerly deviation would be very slight. Let us calculate it for Reich's experiment. The fall being 488 feet, the time of fall was as nearly as possible $5\frac{1}{2}$ secs., which is about $\frac{1}{15700}$ part of a day, corresponding to rotation through about $8\frac{1}{4}''$. Let K B L, fig. 154, be an arc of the Earth's surface ; and let B c represent half the latitude-parallel through B, the place of experiment (Freiburg, lat. 48° north) ; then if B E represent the rotation-arc traversed by B while the body is falling, E is obtained geometrically by taking the arc B N, of $8\frac{1}{4}''$, about c as centre, and drawing N E perpendicular to B c. Wherefore, since

$$\mathrm{B}c = r\cos\lambda, \text{ and}$$

$$2\,\mathrm{BE}.\mathrm{B}c = (\mathrm{NE})^2 \text{ approximately,}$$

$$\mathrm{BE} = \frac{r\sin^2 8\frac{1}{4}''}{2}\cos 48^\circ.$$

Now if F represent the place at which the falling body reaches the ground, B F is part o the great circle through B touching the latitude-parallel B c, and c B F is an angle of 48°. We may take B F in this inquiry as equal to B E, the difference being small compared with either. Hence, drawing Ff perpendicular to B c, we get finally the southern deviation.

$$\mathrm{F}f = \mathrm{BF}\sin 48^\circ = \frac{r\sin^2 8\frac{1}{4}''}{2}\cos 48^\circ \sin 48$$

$$= \frac{r}{4}\sin^2 8\tfrac{1}{4}''\cos 6^\circ$$

$$= (62{,}500{,}000 \text{ inches})\sin^2 8\tfrac{1}{4}''\cos 6$$

$$= \cdot 0992 \text{ inch}$$

$$= 1\cdot 1904 \text{ line.}$$

This is a measurable quantity If Reich had been able to determine the point vertically below the

(546.) Here, then, we have in all a series of 166 experiments, with a result pointing very definitely to an easterly deviation in bodies falling from a great height. The Earth's rotation accounts most satisfactorily for this deviation, and no other explanation can be suggested.

(547.) It is worthy of notice that in all the 166 experiments considered above there was not a single instance in which a ball fell either exactly below the point of support or exactly where it should have fallen, according to theory. We can very well understand this, of course, when we remember the multitude of circumstances (individually minute, yet collectively appreciable) which affect the progress of a falling body, let our precautions to ensure accuracy be what they may.[1]

(548.) On the contrary, a body projected upwards from the Earth will always show westerly deflection, because during its flight it is in regions where the eastwardly motion, due to rotation, is greater than that which the projectile had at starting from the Earth's surface. Artillerists have to take this deviation into account in long ranges.[2]

FIG. 155.—Flight of a body projected vertically from the Earth, and regarded as a satellite during its flight.

point of suspension by optical observations, as by viewing the point of suspension from above reflected from the surface of still mercury below, the southerly deviation he recognised would have been little more than a third of what theory required. But with so great a fall, and inside a mine, this was manifestly impossible. He had to trust to a plumb-line. Now a plumb-line of this length does not hang in the vertical. When we take duly into account the centrifugal tendencies at the point of suspension and on the plumb bob below, we find a slight though measurable deviation; and the formula for this deviation is precisely the same, within the same limits of approximation, as the formula for the southerly deviation of the falling body.

[1] For a complete account of the theory of the method here considered, the mathematical reader is referred to Worms's admirable treatise on 'The Earth and its Mechanism,' to which I have been indebted for most of the above facts.

[2] The full treatment of the problem would occupy more space than can here be spared. I would refer the reader to *Knowledge*, vol. vi., Nos. 152, 154 (September 26 and October 10, 1884), where I have treated the problem geometrically, in full detail. The case for a body projected vertically to a height h may readily be dealt with as follows:—

Let $a\,k$ (fig. 155) be the direction of projection and $a\,A\,b$ the actual path of the missile, part of a long ellipse $A\,A'$ about C as farther focus, the portion $a\,A\,b$ being appreciably parabolic. Let the point a be carried to c during the flight of the missile. Join $C\,a$, $C\,b$, $C\,c$, and draw $A\,M$ to the bisection of the arc $a\,b$.

Then, by Kepler's second law, the missile in moving along the path $a\,A\,b$, sweeps out equal areas round C; and these areas are equal to those which the point a sweeps out round C as it is carried uniformly along the arc $a\,b\,c$. Consequently area $a\,C\,b\,A$ = sector $a\,C\,c$,

or, removing common sector $a\,C\,b$,

area $a\,A\,b$ = sector $b\,C\,c$;

that is, since $a\,A\,b$ is appreciably a parabola and $a\,M\,B$ a straight line perpendicular to the axis $A\,M$,

$$\frac{2}{3}\,a\,b\,.\,A\,M = \frac{1}{2}\,b\,c.b\,C,$$

or westerly deviation, $b\,c = \frac{4}{3}\,\frac{h}{r}\cdot a\,b.$

(549.) Science owes to M. Foucault the suggestion that the motions of a pendulum so suspended as to be free to swing in any vertical plane might be made to give ocular demonstration of the Earth's rotation.

(550). The principle of proof may be easily exhibited, though, like nearly all the evidences of the Earth's rotation, the complete theory of the matter can only be mastered by the aid of mathematical researches of considerable complexity.

Suppose A B (fig. 156) to be a straight bar in a horizontal position, bearing the free pendulum C D suspended in some such manner as is indicated at C; and suppose the pendulum to be set swinging in the direction of the length of the bar A B, so that the bob D remains throughout the oscillations vertically under this bar.

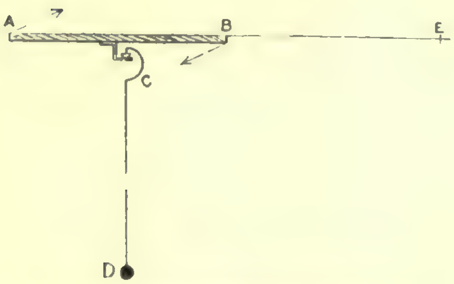


FIG. 156.—Freely suspended Pendulum.

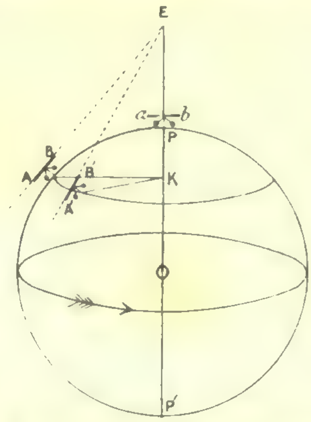


FIG. 157.—Illustrating Foucault's Pendulum experiment.

Now, if A B be shifted in the manner indicated by the arrows, its horizontality being preserved, the pendulum does not perceptibly partake in this motion. If the direction of A B was north-and-south at first, so that the pendulum was set swinging in a north-and-south direction, the pendulum will still swing in that direction, even though the bar be made to take up an east-and-west position.

(551.) Nor will it affect the direction of swing if the bar is shifted by moving the end A horizontally round a point as E on A B produced, the direction of the bar passing always through E. The plane of the pendulum's swing will be shifted *bodily*, but the direction of swing will still continue to be appreciably from north to south.

(552.) Now, let P O P′ (fig. 157) represent the polar axis of the Earth; *a b* a horizontal bar at the pole bearing a pendulum, as in fig. 156. It is clear that if the Earth is rotating about P O P′ in the direction shown by the arrow, the bar *a b* is being shifted round precisely as in the case first considered. The swinging pendulum below

Compared with $a\,b$, $b\,c$ is so small that we may put $a\,c$ for $a\,b$ without appreciably affecting the result; and if $2\,t$ is the time of flight (t for the ascent or descent), P the earth's rotation-period we have

$$a\,c = \frac{2\,t}{P} \cdot 2\,\pi\,r \cos\lambda$$

wherefore $$b\,c = \frac{16\,\pi\,h\,t}{3\,P}\cos\lambda$$

$$= \frac{16\pi}{3P}\sqrt{\frac{2\,h^3}{g}}\cos\lambda$$

$$= (\cdot 00004833 \cos\lambda)\,h^{\frac{3}{2}}$$

the second and foot being the units of time and space respectively.

I have given an analytical solution of this problem in *Knowledge* for July 27, 1883.

it will not partake in its motion; and thus, through whatever arc the Earth rotates from west to east, through the same arc will the plane of swing of the pendulum appear to travel from east to west under *a b*.

(553.) Since we cannot set up a pendulum to swing at the pole of the Earth, let us inquire what results the experiment should have if carried out elsewhere.

Suppose A B (fig. 157) to be our pendulum-bearing bar, placed in a north-and-south position. Then it is clear that A B produced meets the polar axis produced (in E, suppose); and when, owing to the Earth's rotation, the rod has been carried to the position A′B′, it still passes through the point E. Hence it has shifted through the angle A E A′, a motion which corresponds to the case of the motion of A B (in fig. 156) about the point E.[1] The plane of the pendulum's swing will therefore show a displacement equal to the angle A E A′. It will be at once seen that for a given arc of rotation the displacement is smaller in this case than in the former, since the angle A E A′ is obviously less than the angle A K A′ in the same proportion that A K is less than A E.[2] In our latitude a free pendulum should seem to shift through one degree in about five minutes.

(554.) It is obvious that a great deal depends on the mode of suspension. What is needed is that the pendulum should be as little affected as possible by its connection with the rotating Earth. It will surprise some readers, probably, to learn that in Foucault's original mode of suspension the upper end of the wire bearing the pendulum-bob was fastened to a metal plate by means of a screw. It might be supposed that the torsion of the wire would appreciably affect the result. In reality, however, the torsion was very small.

(555.) But other methods of suspension are obviously suggested by the requirements of the problem. M. Hansen made use of the plan exhibited in fig. 158. Mr.

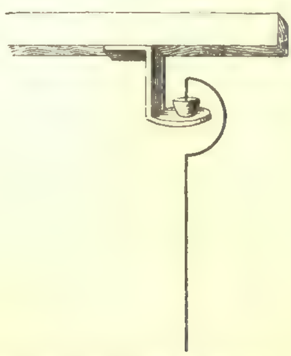


FIG. 158.—M. Hansen's method of suspending the Pendulum.

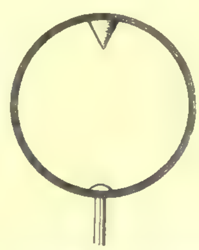


FIG. 159.—Illustrating the suspension employed by the author.

Worms, in a series of experiments carried out at King's College, London, adopted a somewhat similar arrangement, but in place of the hemispherical segment he employed a conoid, and a socket was provided in which the conoid could work freely. From experiments of my own, made in 1864, I am inclined to prefer a steel conoid such as is shown in fig. 159, working on a plane surface. Care must be taken that

[1] In reality A E moves to the position A′E over the surface of a cone having E P′ as axis and E as vertex; but for any small part of its motion, the effect is the same as though it travelled in a plane through E, touching this cone; and the sum of the effects should clearly be proportioned to the sum of the angular displacements.

[2] The angle is diminished as the sine of the angle A E O, or as the sine of the latitude.

the first swing of the pendulum takes place truly in one plane. For this reason the mode of liberation must be carefully attended to.

(556.) Many interesting experiments have been made upon the motions of a free pendulum, regarded as a proof of the Earth's rotation. When carefully conducted, the experiments have never failed to afford results closely corresponding with the effects theoretically due to the Earth's rotation.

(557.) Even more interesting in some respects than Foucault's pendulum experiment is the illustration of the Earth's rotation which the same ingenious physicist derived from the gyroscope, or in reality from any heavy disc, rapidly rotating, and set free as far as possible from any such connection with the Earth as would tend to shift its axis of rotation as the Earth moved. As a missile from the Earth's surface may be regarded during its flight as a satellite of the Earth (Arts. 451 and 548) and as a pendulum duly suspended may be regarded as free to maintain its plane of swing uninfluenced by the Earth's rotation (Arts. 550, 551), so a heavy and rapidly rotating disc or sphere, suspended so as to be as free as possible to maintain its axial pose, may be regarded as approximating to the condition of a minute planet, rotating freely on its course round the sun.

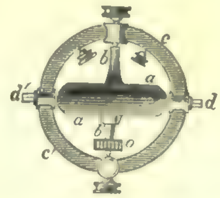


FIG. 160.—The heavy disc of the Gyroscope.

(558.) In Foucault's experiment for exhibiting the Earth's rotation by means of the gyroscope, everything is done to prevent influences by any external forces. In the first place, a very heavy gyroscope is used, which is set in very rapid rotation. This part of the apparatus is shown in fig. 160; $a\,a'$ is the heavy disc; $b\,b'$ is the axis on which it rotates; $c\,c'$ is a ring, within which the heavy disc turns freely; at d and d' are knife-edges on which the ring $c\,c'$, bearing the heavy disc, may be poised inside another ring $e\,e'$, as shown in fig. 161, the median plane of the disc being vertical. The ring $e\,e'$ is suspended by the torsionless thread shown in the upper part of fig. 161, and is supported underneath by the agate cup k, on which the fine hard point of the cone n rests. Thus everything possible is done to diminish the effect of the force which really acts upon the rotating disc to change the position of its axis. This force is the effect of the Earth's rotation. So carefully is the suspension managed that when the vertical ring is loaded with the heavy disc (at rest), the merest breath causes it to shift its place; also the ring $c\,c'$, poised on the knife-edges, oscillates with the utmost freedom when the ring is not rotating.

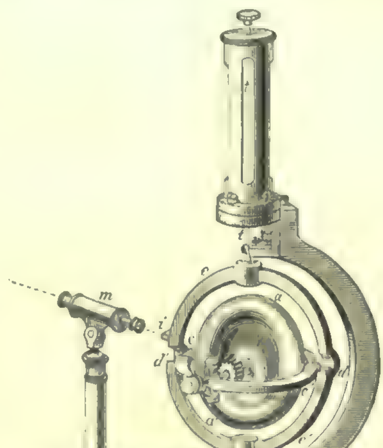


FIG. 161.—Foucault's original Gyroscope.

When the disc $a\,a'$ has been set by suitable machinery in very rapid rotation within the ring $c\,c'$, this ring is set carefully within the ring $e\,e'$, as shown in fig. 161, and the disc is ready for observation as shown in the same figure, a microscope being set at m to watch the movement of the finely-divided index-edge i.

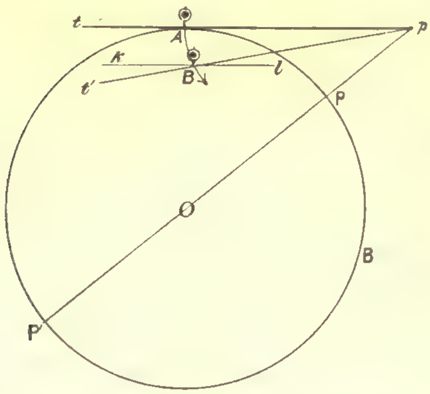


Fig. 162.—The Gyroscope on the rotating Earth.

Suppose, now, that there is a gyroscope at a point A on the rotating earth P A P′ (fig. 162), with the median plane of its heavy rotating disc north-and-south, as shown at N S in fig. 163, where we are supposed to be looking downwards at the plane $t\,p\,t'$ of fig. 162. This gyroscope will be carried by the Earth's rotation around her axis P O P′, to such a position as B; and if the gyroscope were not rotating the median plane of the disc would still remain north-and-south. Thus, supposing t A p, the tangent north-and-south line to the Earth's surface at A, meeting the earth's axis, produced, at p, the median plane of the gyroscopic disc which had coincided with t A p when the gyroscope was at A, would coincide with t'B p, the north-and-south tangent line through B, when the gyroscope was at B. This would correspond with the direction n C s in fig. 163. But the heavy, rotating, and delicately-poised disc of Foucault's gyroscope will undergo no such displacement. The median line N S will be only affected by such portion of the Earth's action as is conveyed through the suspending line and at the knife-edges supporting the internal ring, and this action, instead of shifting N C S to the position n C s (Art. 523), can only slightly shift the uppermost point C towards the left (the rotation being in the direction indicated by the arrows), leaving the points l and k (corresponding to N and S) on a line parallel to N S. Thus, if a horizontal microscope is directed to the median line of the disc, at N or at S, or to the index-edge i of the supporting ring $e\,e'$ (fig. 161), this line will appear to move in a direction opposed to that of the Earth's rotation, the northern side moving towards the east, the southern side moving towards the west, as shown by the line $k\,l$ in fig. 163, which corresponds in direction with the line $k\,l$ of fig. 162.

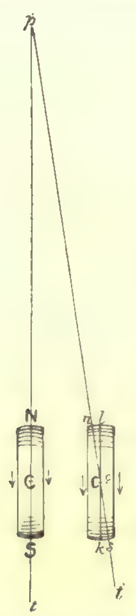


Fig. 163.—The Gyroscope's apparent change of pose.

(559.) When observed under the microscope, the gyroscope is seen steadily travelling against the direction of the Earth's rotation, at the precise rate which calculation shows to correspond to the latitude of the place of observation. For the time being, the gyroscope is, so to speak, like *an independent planetary body with its own proper polar axis directed constantly (or very nearly so) to the same point of the celestial sphere.*

(560.) We are led naturally to the proof of the Earth's rotation derived from the precession of the equinoxes. As certainly as the reeling of a top shows that the top is also rotating, the precessional gyration of the Earth

demonstrates the Earth's rotation. The reeling motion is shown by the slow gyration of the whole sphere of the fixed stars, to which we should have to attribute a real gyration unless we recognised the gyration of the Earth. This would be almost unimaginable—even were not this terrestrial gyration shown to be a necessary consequence of dynamical laws.

(561.) But in the displacement of the heavenly bodies due to nutation, we have a yet more decisive proof of the Earth's rotation. In consequence of the Earth's nutation, which itself, both in quality and quantity, is a consequence of her rotation, all the stars in the heavens, *all the planets*, and the sun and moon, appear, as it were, to be affected with a species of tremor, each vibration of which has a period of 18·6 years. It would be obviously unreasonable to suppose that the stars on the one hand, the planets, sun, and moon on the other, which are unlike as respects their principal movements, should only resemble each other in these strange vibrations. We have seen that a simple dynamical theory, the results of which we can readily confirm by experiment, accounts for all these observed tremors as due to the Earth's rotation.

(562.) It may be said, in fact, that every star in the heavens, as it traces out in its $18\frac{3}{5}$ years' period its small nutation-ellipse, $18\frac{1}{2}''$ by $11\frac{3}{4}''$, tells us of the rotation of the Earth.

(563.) And while all the untold millions of stars unite to tell by their nutation-ellipses the record of the Earth's rotation on her axis, they record not less distinctly the Earth's revolution round the sun, in the millions of aberration-ellipses, of constant span but various in breadth, which the stars trace year after year on the celestial concave.

(564.) It was in 1727 that Bradley began the series of observations which resulted in his discovery and interpretation of the aberration of the fixed stars. The general law of aberration, to which the apparent annual motions of the stars are subjected, is this:—Every star in the heavens, whether obvious to the naked eye or visible only by the aid of powerful telescopes, whether shining in solitary splendour or lost in the profundities of some rich star-cluster, travels once a year in a minute ellipse, whose major axis is somewhat more than two-thirds of an arc-minute in length, while its minor axis depends on the position of the star with reference to that great circle on the heavens in which the sun seems annually to travel. A star close by the pole of this circle—the ecliptic—has an almost circular aberration-ellipse; one near the ecliptic itself has an aberration-ellipse so eccentric as to be almost a straight line. But every star has an aberration-ellipse of the same major axis. And that major axis, though minute, belongs to the order of magnitudes which are obvious to the telescopist—palpable,

unmistakable, clear as the sun at noon, to the worker in a well-appointed observatory.[1]

(565.) Let A B C D, fig. 164, be a foreshortened view of the Earth's orbit round the sun, S, the feathered arrows indicating the direction of the Earth's motion; and let the parallel lines A s_1, B s_1, C s_1, and D s_1 indicate the direction of a star at or near the northern pole of the ecliptic; A s_2, B s_2, C s_2, and D s_2 indicating the direction of a star about midway between the ecliptic and its pole; while A s_3, B s_3, C s_3, and D s_3

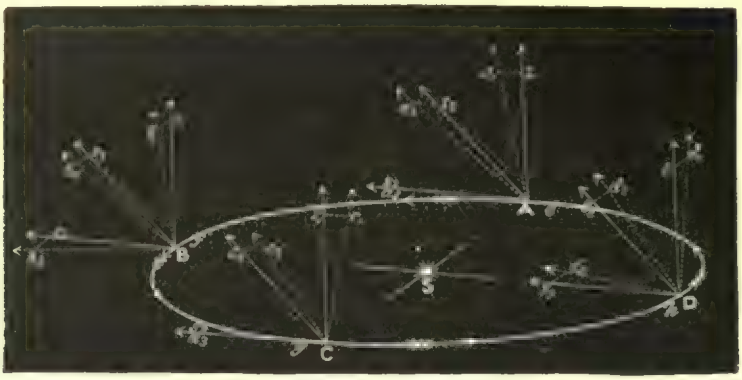


Fig. 164.—Illustrating the aberration of the Stars.

indicate the direction of a star on the ecliptic. Then when the Earth is at A, the star really lying towards s_1 is seen in direction A a_1, the angle s_1 A a_1 being one of about $20\frac{1}{4}''$, and a_1 s_1 being parallel to A e, the Earth's course at A; the star towards s_2 is seen in direction a_2, the angle s_2 A a_2 being less than s_1 A_1 a_1 in the degree shown by the figure, or as the sine of the angle s_2 A s_3 is less than unity; while the star in direction s_3 is seen unchanged in direction. When the Earth is at B, moving at right angles to all the directions B s_1, B s_2, B s_3, the stars lying severally in these directions are displaced so as to be seen towards b_1, b_2, b_3, the angular displacement of each being about $20\frac{1}{4}''$, and b_1 s_1, b_2 s_2, b_3 s_3 being each parallel to Bf, the Earth's course at B. At C the star really lying in direction C s_1 is displaced so as to be seen towards c_1; c_1 s_1 being parallel to C g, the Earth's course at C, and the displacement being the same in amount as at A, but in the opposite direction; the star in direction C s_2 is displaced so as to be seen towards c_2, the angle c_2 C s_2 being equal to the angle a_2 A s_2 but in the opposite direction; and the star towards s_3 is seen unchanged in direction. Lastly, when the Earth is at D, the real directions, D s_1, D s_2, and D s_3, of a star, are changed into the apparent directions D d_1, D d_2, and D d_3 respectively, the displacements being all in directions parallel to the motion of the Earth at D, and equal in amount to those observed at B, but in the opposite direction.

Fig. 165. — Aberration-ellipses of Stars in different celestial latitudes.

[1] A magnifying power of 100 gives to the aberration-ellipse of a star a span greater than twice the apparent diameter of the moon.

(566.) If the observations are continuous, it is found that as the Earth passes round the orbit A B C D, the star s_1, 90° from the ecliptic, describes a circle $a_1\ b_1\ c_1\ d_1$ (fig. 165) about its real position; the star s_2, 45° from the ecliptic, describes an ellipse $a_2\ b_2\ c_2\ d_2$ about its real position, s_2; and the star s_3 on the ecliptic moves backwards and forwards along the line $b_3\ d_3$ (being at b_3 when the Earth is at B and at d_3 when the Earth is at D, and passing the point s_3 when the Earth is at A and C), the motion in different parts of the line corresponding to such motion as would be seen if the star were travelling in a circle such as $a_1\ b_1\ c_1\ d_1$ seen edgewise. The ellipses $a\ b\ c\ d$ and $a'\ b'\ c'\ d'$ represent the aberration-paths traversed by stars s and s', at distances 60° and 30° respectively from the ecliptic. Corresponding movements affect stars in all celestial longitudes and latitudes.

(567.) Some time passed before Bradley was able to interpret these aberrations of the stars. But one day, sailing with a pleasure party on the Thames, he noticed that a small vane at the top of the boat's mast seemed to indicate a systematic change in the wind's direction as the boat changed her course in sailing to and fro. Learning from the boatmen that this was always observed under like conditions, he was led to recognise a resemblance between this seemingly commonplace phenomenon and the aberration of the fixed stars, regarded as a systematic change in the direction of the rays coming from each star as the Earth changes her course in moving round the sun. Römer had already discovered in 1675 that light is not transmitted instantaneously; and the observations of Jupiter's satellites made up to Bradley's time sufficed to indicate that the velocity of light was somewhat more than 10,200 times the velocity of the Earth in her orbit.

(568.) The light coming from a star to the Earth may be compared to the wind blowing on Bradley's boat. If we suppose s_1 A to represent the direction and velocity of rays coming from a star s_1, towards the Earth at A, and e A to represent the Earth's velocity (only A e should be less than the ten-thousandth part of A s_1), then the apparent direction of the rays as they reach the observer at A will be a_1 A,—precisely as rain falling through a distance s_1 A on a person at A while he advances through the distance A will seem to fall on him at A in the direction a_1 A. In like manner each of the observed displacements of the stars lying really towards s_1, s_2, and s_3 will be explained in quality, by taking into account the effect of the Earth's motion round her orbit in apparently modifying the direction of the light rays from these several stars. As regards quantity, the greatest angular displacement due to this cause would be measured by an angle a_1 A s_1, whose tangent ($a_1\ s_1 \div$ A s_1) is $1 \div 10{,}200$, or an angle of about $20\frac{1}{4}''$. So that the observed aberrations of the stars are explained both in kind and degree by taking into account the Earth's motion round her orbit and the motion of light with a velocity (independently determined) about 10,200 times greater.

(569.) No doubt, then, could remain as to the meaning of the aberration-ellipses traversed by the stars. They reflect, as it were, the orbital circuit of

the Earth—in fact, the aberration-ellipse of any star has precisely the *shape* (though enormously larger) which the Earth's orbit has as seen from that star.

(570.) It may be said, then, that the heavens have ever been speaking to man of the real movements of the Earth and her fellow worlds. The diurnal motion of the stars spoke from the beginning of the daily rotation of the Earth on its axis. The yearly movement of the star-sphere, as also of the sun, moon, and planets (such parts of the motions of these as are yearly), have recorded the yearly revolution of the Earth around the sun. The motions of the moon have disclosed the true nature of our satellite's path, at once round the Earth and round the sun; the movements of the planets in their various and ever-varying loops have shown how these bodies circuit round the ruling centre of the solar system. All this had in a sense been recorded independently of the recognition of the law of universal attraction.[1] With the discovery of this law came the definite interpretation of all those apparent movements, and also the recognition of the real meaning of others more delicate and recondite, but even more significant. The slow gyration of the heavens in the long precessional period of 25,868 years showed the Earth as reeling in that vast period like a stately and gigantic top. The movement of each star once in $18\frac{3}{5}$ years round its small nutation ellipse, only $18\frac{1}{2}''$ by $11\frac{3}{4}''$ in axial range, showed the Earth's reel affected by a tremulous vibration, passing in that comparatively short period through all its phases. And lastly, the annual aberration-ellipse of each star in an ellipse—not, like the nutation-ellipse, constant in form, but with constant longer axis of $40\frac{1}{2}''$ and shorter axis proportioned to the longer as the sine of the star's celestial latitude—shows the Earth circuiting around the sun in an orbit appreciably circular in shape, though eccentric with respect to him, once in every year. Of these last-mentioned movements, also, it is to be noted that while they are not merely discernible but obvious, nay, conspicuous, to telescopic scrutiny (aided by such micrometric devices as were described in the preceding chapter), they cannot possibly be explained on any hypothesis save that which regards the Earth as a globe moving in space under the direct attraction of the sun, and under the perturbing influence of the sun, moon, and planets. These proofs of the Earth's rotation and revolution are repeated by every star in the heavens,

[1] It is an impressive thought that this same mystery of mysteries, the law of gravitation, not only thus brings all the millions of stars within telescopic range to bear witness to the Earth's movements, but has in reality enabled men to deal with the molecules of matter, and the atoms of which such molecules are formed—seeing that it is only by the laws of motion, revealed and interpreted by gravity, that the physicist can determine the qualities of molecules, and only by the test of weight that the chemist can be sure that his analyses and syntheses are trustworthy.

according to its position on the celestial sphere. Thus the evidence is multiplied many million-fold. Less than a human life applied to the observation of any one of all the millions of stars revealed by the telescope would suffice to rediscover the precessional gyration of the star sphere, to follow the star through three or four nutation circuits, and to obtain once in each year from that star the evidence of the Earth's revolution round the sun.

CHAPTER V.

MEASURING AND WEIGHING THE SOLAR SYSTEM.

(571.) When the discovery of the law of universal gravitation had finally set the sun at the centre of the solar system, the proportions of that system were definitely determined, though as yet the scale of the system remained to be ascertained. Even before Newton's work was completed, the proportions of the solar system were known from the observed movements of the several planets. Indeed it was from observations of this kind that Kepler was able to establish the laws of planetary motion, the first and third of which indicate the shapes and proportionate dimensions of the planetary orbits. When Newton showed that these laws are necessary consequences of the law of gravitation, assuming the sun to be absolutely supreme by virtue of the degree in which his mass exceeds that of all the planets together, the proportions of the solar system already recognised were demonstrated. This applies not only to the orbits of the planets round the sun, but to the orbits of the satellites round their primaries, with one exception only ; even in Newton's time the proportion of the moon's orbit to the orbits of the other members of the solar system was not satisfactorily determined. But in all other respects the proportions of the solar system were assigned from the time when Kepler's laws were established.

(572.) These proportions are indicated in Plates IX. and X. To present them satisfactorily it is absolutely necessary to have two such diagrams ; for (as Plate X. shows) if all the orbits are included in a single drawing of moderate dimensions, those of the four planets Mercury, Venus, the Earth and Mars, are on too small a scale to be properly presented ; while, if the four inner orbits are shown on a suitable scale as in Plate IX. (fifty millions of miles to the inch), the whole system could only be presented in an unwieldy chart nearly ten feet in diameter (the span of Neptune's orbit being 111 inches on this scale). It is interesting, by the way, to note the similarity between the orbits of the outer planet Neptune, Uranus, Saturn, and Jupiter, and the orbits of the four inner planets, Mars, Earth, Venus, and Mercury, when the

latter are presented on a scale exactly twenty times greater than that of the former. It is one of the examples of a feature which is repeated on many scales throughout the universe. As the branches of a tree are miniatures of the trunk, and the twigs miniatures of the branches, so in the solar system the inner family of planets is a miniature of the outer family, the family of Jupiter is a miniature of the inner family; and so in like manner in the star-depths we recognise miniatures of the Galaxy within itself, and miniatures of these miniatures within their own extent. Such thoughts are no mere play of fancy; and though it would be rash to insist on the significance of such relations, it would be even more unwise to overlook them, or to regard them as meaningless. The resemblance between branch and tree, between twig and branch, and between the leaf-skeleton and the twig, must be recognised as a direct consequence of the laws of tree-growth, and as indicating the development of the tree in all its details, large and small, according to the uniform laws of development. It is altogether probable that the existence of corresponding relations in planetary and stellar systems has resulted from the operation of similarly uniform laws.

(573.) So also the general symmetry of the solar system, as presented in Plate X., must be regarded as not only characteristic but full of significance. It doubtless had its origin in the laws according to which the evolution of the solar system proceeded, though it may never be possible for science to ascertain what those laws were.

(574.) The symmetry in the planetary system is partially indicated in what has been called Bode's law, but is more correctly called the law of Titius, Bode having merely recalled the law to notice. It is a purely empirical law connecting the planetary distances by the following formula:

Numbering the planets in their order of distance from the sun, the distance of the n-th planet is approximately $4+3(2^{n-2})$—the distance of the Earth being represented by 10.

The following table shows how nearly the law approximates to the truth in the case of all the planets except Neptune and Mercury:—[1]

	Mercury	Venus	Earth	Mars	Asteroids	Jupiter	Saturn	Uranus	Neptune
Constant . .	4	4	4	4	4	4	4	4	4
Add. . . .	1·5	3	6	12	24	48	96	192	384
Sum . . .	5·5	7	10	16	28	52	100	196	388
True distance	3·9	7·3	10	15·2	27·4	52	95·4	192	300

[1] Mercury is usually included in the table, with 0 under the constant 4; but this is manifestly incorrect: for the numbers 0, 3, 6, 12, 24, &c., do not form a true series. The best way of writing the series, perhaps, is to double all the above numbers, making the constant 8, and the series 3, 6, 12, 24, &c. When this is done, the series of numbers to be added is the same as that usually

(575.) Recognising the proportions presented in Plates IX. and X. as correct, we have next to inquire into the real dimensions of the solar system.

All measurements of the distances of inaccessible objects depend directly or indirectly on triangulation, starting from some measured base-line as a side of the first of the triangles employed. Whether one triangle or a hundred be dealt with, and whether the work be the survey of a county, a country, or a continent, the survey of the solar system or the survey of the star-depths, we must in every case start from a base-line, and from a triangle of which that base-line is a side.

(576.) In fig. 166, A B represents such a measured base-line, O a distant object, and A O B O the true directions of O as seen from A and B. If the base A B is correctly measured and the angles O A B, O B A are correctly determined, the distances A O and B O can be determined either by geometrical construction or (much more satisfactorily) by trigonometrical calculation. If the angles at A and B are incorrectly determined, so as to give such directions as A 1 or A 2, B 3 or B 4, the position of O will not be exactly determined; yet it may be regarded as determined within certain limits of error. Suppose, for instance, two observations at A gave severally the directions A 1 and A 2, while two others at B gave the directions B 3 and B 4; then by taking these observations in pairs, each of those from A with each of those from B, we should get the four points 5, 6, 7, and 8, and might infer that O lay within the quadrilateral 5 7 6 8. Or if A 1 and A 3 indicated the limiting directions from A within which the true direction of the distant object certainly lay, while B 3 and B 4 indicated corresponding limiting directions from B, then we should be certain that O lay somewhere within this quadrilateral 5 7 6 8. By increasing the number of observations and the care and skill with which they were made, we should get a smaller limiting area within which the point O must lie, and we should furthermore increase the probability that the true position of O lay near the centre of gravity of this area; for, in such measurements, chances are generally almost exactly equal[1] that the error will lie on one side of the truth as that it will lie on the other; so that the mean value of many observations is likely to be near the truth, and so much the nearer as the number of observations is increased. In all cases, of course, the accuracy of the deduced distance depends on the accuracy with which the base-line has been measured.

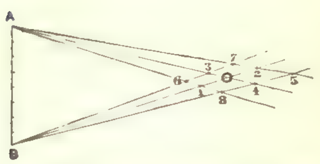


FIG. 166.—Determining by triangulation the distance of a remote object.

(577.) So much I have been careful to explain at the outset, because, while all problems of celestial measurement depend on such determinations as have been just considered, they are all necessarily approximate. Many who hear that the measurements of the distances of the moon, sun, and stars, have been corrected by astronomers,

shown, but is shifted so that the first term falls under Mercury's constant (8): we see then the discrepancy between the distance 11 assigned by the formula to Mercury, and Mercury's actual distance 7·8 when a true series is employed. It is somewhat remarkable that a series which thus fails completely both at the beginning and at the end of the nine terms, should be so nearly correct for the remaining seven.

[1] Not necessarily quite equal, as is often stated, for the circumstances may be such as to favour error in one direction rather than error in another.

and in some instances by amounts which seem considerable or even enormous, imagine that astronomers had been mistaken in their former measurements or that some change has taken place in the ideas of astronomers or even in the actual positions of the orbs in question. In reality, all astronomical statements about distances, masses, and so forth are presented only as the most probable mean of the best measurements available when such statements are made. The limits of probable error are not presented in books meant for general reading, because they would be apt to confuse the reader. But in the original calculations the amount of probable error is always carefully considered and stated. The ideas of astronomers on these questions of distance have not changed, and in the present position of astronomy, based (in such respects) on absolute demonstration, they cannot change. As for the distances of the heavenly bodies, we know certainly that the dimensions of the solar system have not altered appreciably within several thousand years, since any change of scale would involve an alteration of all the planetary periods.[1] We know, in like manner, that the moon's mean distance has not altered—at least, not beyond the limits of change corresponding to the calculable perturbations already dealt with. And though the distances of many of the stars from the solar system are changing enormously in absolute amount, many millions of miles in some cases each year, these changes are not such as to affect our estimate of the stars' distances relatively to our base-line—the span of the earth's orbit—for the distance of even the nearest fixed star exceeds the length of that base-line more than 200,000 times.

(578.) Our study of the measurements of distance may conveniently begin with the case of the moon—the easiest of all the problems of the kind with which the astronomer has to deal.

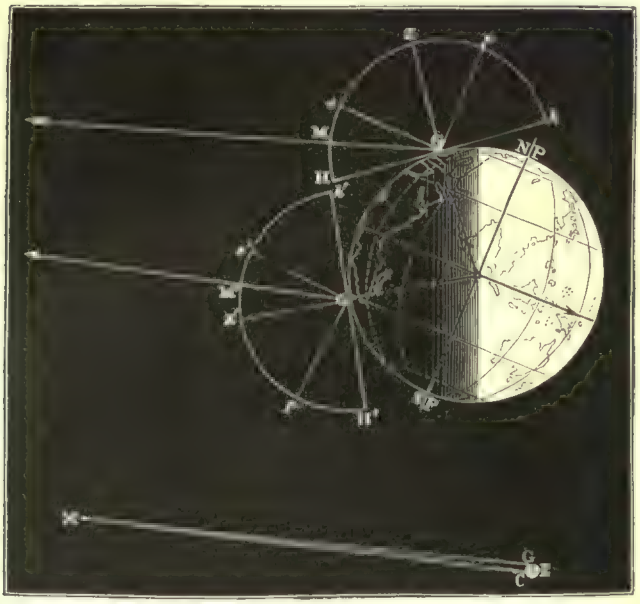
Figs. 167 and 168 shows the nature of the problem to be attacked.

Let us suppose, for convenience of illustration, that one station is the Greenwich Observatory, and the other the Observatory at the Cape of Good Hope. These two stations are not on the same meridian. At present, however, we shall not take into account the difference of longitude.

Fig. 167 represents a side view of the earth when Greenwich is at the place marked G on the night side of our planet, and Cape Town at C. The lines H h and H$'h'$ are north-and-south horizontal lines at Greenwich and Cape Town, G Z, C Z$'$ the verticals, G p and C p' (parallel to the Earth's polar axis) the polar axes of the heavens; one directed towards the North pole, the other towards the South. Let us suppose that the moon, when crossing the meridians of Greenwich and Cape Town, is seen in the direction G M and C M$'$ respectively. [There is a difference corresponding to $1\frac{1}{4}$ hour's rotation between the meridians of Cape Town and Greenwich (Cape Town being towards the east); but in this inquiry we may neglect this, and suppose the station C to be at the edge of the disc: (the astronomer, of course, takes it fully into account).]

[1] If the sun, for example, were nearer now than in the days of Hipparchus by $\frac{1}{30}$th of his distance at that time (or by about 3,000,000 miles), then by Kepler's third law the period of the Earth's revolution (that is, the year) would be less than the year in the time of Hipparchus as $(29)^{\frac{3}{2}}$ is less than $(30)^{\frac{3}{2}}$, or very nearly as 19 is less than 20, because $(29)^{\frac{3}{2}} = (30)^{\frac{3}{2}} (1 - \frac{1}{30})^{\frac{3}{2}}$, and $(1 - \frac{1}{30})^{\frac{3}{2}} = 1 - \frac{3}{2} \cdot \frac{1}{30}$ nearly $= 1 - \frac{1}{20} = \frac{19}{30}$. The year then would be shortened by $\frac{1}{20}$th of its duration in the time of Hipparchus, or by $\frac{1}{19}$th of its present length, say by three weeks. But we know that the year is not so much as ten seconds shorter now than in the time of Hipparchus. The distance of the sun might change by half a million miles without any of our direct measurements indicating the change for a quarter of century or more; but the resulting change in the length of the year would reveal the alteration at once.

Fig. 168 represents this state of things on a smaller scale, M being the moon, G Greenwich, and C the Cape of Good Hope. G M C is the triangle astronomers have to deal with in determining the moon's distance. The base-line G C (the chord, of course, not the arc) is known; and the angles at G and C are known from the observations pictured in fig. 167. Thus M C and M G (fig. 168) can be calculated, and thence the distance separating the centre of the moon M from the centre of the earth E.



Figs. 167 & 168.—Illustrating the measurement of the Moon's distance.

(579.) In dealing with astronomical measurements of this sort, it is customary to speak not of the distance but of the parallax of the orb dealt with. Fig. 169 illustrates the connection between distance and parallax. E is a point on the earth, whose centre is at P; M, M′, and M_H are supposed to represent the moon as seen in different directions in the heavens, Z being the zenith, M_H m_h marking the terrestrial and the celestial horizon. If now we draw from P lines P m, P m' and P m_h parallel to the directions in which the moon is thus seen from E, we obtain the angles M P m, M′ P m', M_H P M_H, indicating the displacement of the moon as seen from a place E on the earth as compared with her real direction from the earth's centre. This displacement is called her *parallax*. It is manifestly greatest when she is on the horizon as at M_H; is less as her altitude increases; and vanishes at the zenith. The maximum parallax, at the horizon, is called the *horizontal parallax*. If we know its value, that is, the value of the angle P M_H E, we evidently know the distance P M_H since E P÷P M_H is the sine of this known angle. It is found more convenient among astronomers to speak of the horizontal parallax than of the distance. As, however, the earth's radius is different in different latitudes, the term horizontal parallax would not be definite, unless we indicated the latitude; consequently it is customary to speak only of the horizontal parallax at the equator (where the parallax is greatest). Thus, instead of speaking of the mean distance of the sun, moon, or planets, the astronomer prefers to speak of the *mean equatorial horizontal parallax*.[1]

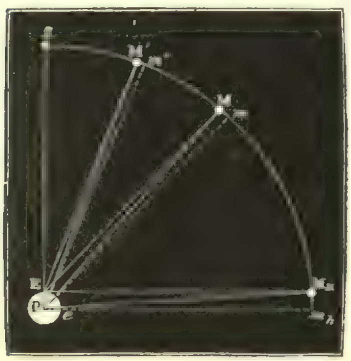


Fig. 169.—Illustrating Lunar Parallax.

[1] Some, indeed, use this convenient technicality when addressing those who are not astronomers; for whom, of course, it is mere jargon. In this affectation, which many mistakenly regard

(580.) We see from fig. 169 that the moon appears, through parallax, to be depressed below her true place, and more depressed the nearer she is to the horizon. Refraction raises her in some degree as she nears the horizon; but this effect the astronomer takes fully into account; and it is the moon's place corrected for refraction which we are supposed to be dealing with. It is obvious that in this parallactic change we have the means of determining the moon's distance by observations at a single station; indeed, if we were at a station where the moon crossed the meridian at the zenith Z, we need only ascertain how far the time of her motion over the arc Z M_{II} falls short of the calculated time over the arc Z m_h—that is, of a quarter-circuit—to determine the arc $M_{II} m_h$ which measures the moon's horizontal parallax, and gives us the moon's distance. A method akin to this was, in fact, followed by the earlier astronomers.

(581.) We may pause here for a moment to notice a peculiar phenomenon which seems entirely inconsistent with the relations pictured in fig. 169. It will be seen that the moon when at M_{II} is farther from the observer E than when at M, M′, or Z. Yet she always appears larger, and sometimes very much larger, when near the horizon than when high above it. So also with the sun. The effect of position is very much less in his case than in the moon's: still, when the sun is on the horizon he is slightly farther from us than when high above it; yet he looks larger, as if nearer.

(582.) The common idea, sometimes gravely advanced as a known fact, is that the air in some way magnifies the sun and moon when thus seen. This idea is altogether erroneous; indeed, it is not in itself worth controverting: but as the refractive action of the air, which does really modify the apparent discs of the sun and moon, is one of the most important factors in the correction of astronomical observations, we may with advantage take the present opportunity of considering this action.

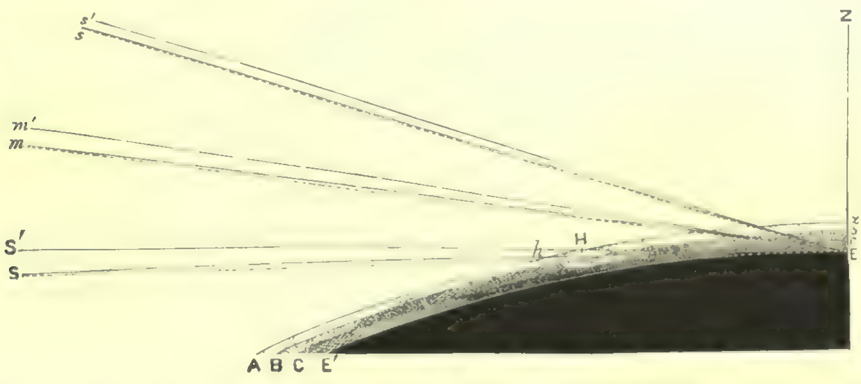


Fig. 170.—Illustrating the effects of Atmospheric Refraction.

(583.) In fig. 170, E E′ is supposed to represent the earth's surface; A H *z* the outer limit of the atmosphere so far as it has an appreciable density (the depth of the air is enormously exaggerated of course in fig. 170); B *b*, C *c*, the limits of layers of equal density, which must be supposed very great in number and severally very thin, the density of successive layers of course increasing downwards.

Since rays of light passing from a vacuum to a medium of appreciable density, or from a medium of less to one of greater density, are deflected towards the medium

as essential to what they call the dignity of science, we perhaps have the last remaining traces of the mystery with which the old astronomical priests found it desirable to enshroud all such references to the heavenly bodies as they had occasion to address to the uninitiated.

they are entering, and so much the more deflected as their course makes the smaller angle with the surface of the medium thus entered, it is evident that rays from heavenly bodies at *s* and *m* towards an observer at E—shown by the dotted lines—will be curved downwards as they fall on the air and pass into denser and denser strata, and thus would not reach the observer at E, who, therefore, will not see the orbs *s* and *m* where these orbs really are. But rays from *s* and *m* on some such course as is shown by the full lines (from these points), which, were they to travel straight on, would pass above E, will be curved downwards by the refractive action of the air so as to reach the observer at E, who will thus see these bodies *s* and *m* in the directions of tangents to these curved lines at the point E—that is, in such directions as E *s′* and E *m′*. The bodies *s* and *m* will thus be seen above their true places, and the deviation for *m* will be greater than for *s* because of the smaller angle at which rays from the former point are inclined to the surface of the atmospheric layers which they traverse. The greatest deviation occurs, then, in the case of a celestial body as S,

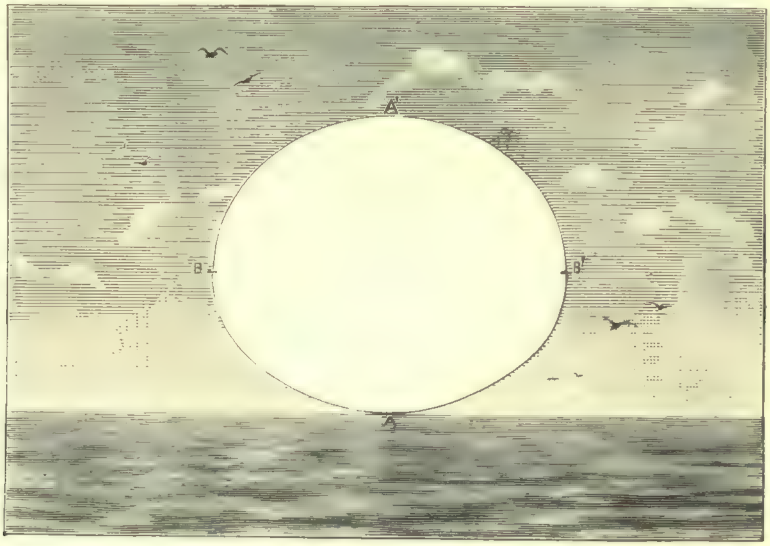


Fig. 171.—Changed shape of horizontal Sun or Moon.

whose rays, falling on the air as at *h*, are curved along the course *h* E, such that the tangent at E to the curve is horizontal, and the body is seen as at S′, or on the horizon itself. For a star situate as at S the refraction is found to average 34′ 54″, varying somewhat with the condition of the air. At only 30′ of apparent altitude the mean refraction is already reduced to 29′ 3″; at 1° to 24′ 25″; at 2° to 18′ 9″; at 3° to 14′ 15″; at 4° to 11′ 39″, and at 5° to 9′ 47″. At greater altitudes the refraction becomes steadily less, diminishing also more and more slowly. At the apparent altitude 10° the refraction is 5′ 16″; at 15° it is 3′ 32″; at 20° it is 2′ 37″; at 30° it is 1′ 40″; and at 45° it is only 58″; at 60° it is 33″; and at 75°, 15″; whence diminishing about 1″ for each degree of apparent altitude it reduces to nothing at the zenith. The arcs here mentioned indicate the correction for mean refraction at the corresponding altitudes. In actual work the state of air in regard to density, temperature, and humidity must be taken into account to obtain the true correction for refraction.

(584.) It is evident that the sun or moon, when apparently on the horizon, has its vertical diameter reduced by the effect of refraction. For the lowest point of the

disc (whose true diameter at the time is, let us say, 32′) is in reality 34′ 54″ below the horizon, when seen exactly on the horizon; while a point only 30′ above the horizon corresponds to a point in reality 29′ 3″ lower down, or only 57″ above the horizon; but such a point would be 35′ 51″ (34′ 54″ + 57″) above the lowest edge of the sun, or 3′ 51″ above the uppermost point of the sun. This uppermost point will therefore be raised less than 30′ above the horizon. From a fuller statement of mean refractions than has just been given, we find that the uppermost edge of the sun is raised about 26′ 43″ above the horizon when the lowermost just touches the horizon. The apparent vertical diameter of the horizontal sun or moon is reduced, then, from 32′ to less than 26¾′, or by about one-sixth. Thus the shape of the sun or moon when on the horizon is such as is shown in fig. 171, where the vertical diameter A A′ is made less than the horizontal diameter B B′ in this degree. It will be noticed that B B′ is set somewhat below the middle of A A′, the lower half of the disc being more affected by refraction than the upper.

(585.) Such is the only effect which the atmosphere in its ordinary condition can produce on the disc of the sun or moon. The vertical compression may be greater or less according to the condition of the air.[1] The horizontal diameter is not affected at all by atmospheric refraction except under conditions altogether unusual.

(586.) The horizontal disc of the sun and moon is *diminished*, then, by the refraction of the air, which diminishes the vertical diameter and leaves the horizontal diameter unchanged. In the case of the moon both diameters are less than those of the moon when high above the horizon, because, as we see from fig. 170, the moon is farther off when low down. It need hardly be said that the results of micrometrical measurement correspond exactly with these estimates.

(587.) How then, it may be asked, can the enlarged appearance of the horizontal moon or sun be explained? And, in particular, how can the increase in the amount of enlargement when the air is thick or misty be accounted for?

Of the enlargement due to position Ptolemy gave an adequate explanation —already probably old in his day—long ago. Apart from the special form, a somewhat flattened dome, which we unconsciously attribute to the heavens, we have a clearer consciousness of the real distance of the sky near the horizon than high above it, because of the way in which we have that distance impressed upon us, from our childhood upwards, by the objects seen within the circuit of the horizontal sky. Buildings, trees, hills, and mountains, beyond and behind which the sky seems to pass, assure us that there, at any rate, the sky is not within such a distance as we unconsciously attribute to the sky overhead. That the mind does thus set the overhead sky at an utterly inadequate distance, is shown by the dimensions attributed to objects seen in the sky. Many will describe a meteor as about a foot in diameter, or about as large as the full moon, indifferently, as though the two descriptions were naturally equivalent. Some regard the sun or moon as looking about a yard in diameter, while others consider even a foot too great and 9 inches as nearer the mark. A globe having a diameter of 1 foot would look as large as the moon at a distance of about 108 feet, or 36 yards; and even the largest of these estimates—one yard—would set the moon only 108 yards

[1] In abnormal states of the air the disc may be altogether distorted. This happens specially with the sun, as actually abnormal conditions are not apt to affect the part of the horizon where the full moon rises or sets. To affect the horizontal diameter of the sun or moon the density of the air must vary horizontally in marked degree. Such a peculiarity, though occasionally noticed, is altogether unusual, and seldom lasts more than a few minutes.

from us. This is probably about the greatest distance at which the untrained mind sets the sky overhead at night; for the moon when high appears to be on or near the background of sky. But when the moon is low down it becomes obvious that her orb must be much more than a yard in diameter; for she is seen to lie beyond objects known to be much more than a hundred yards from the observer. Often the apparent size of the moon may be compared directly with that of objects of known dimensions; and when it is seen that she looks larger than a house or a tree, though obviously farther away, the idea that her diameter is but a yard is at once dispelled. The mind is unable to distinguish between the obvious fact that the moon *is* larger than she had been unconsciously supposed to be, and the idea that she *looks* larger than she had done when judged to be but a yard or a foot in diameter.

(588.) The explanation of the further increased enlargement of the sun or moon —but especially of the moon—when seen near the horizon through mist, is somewhat similar. When the air is thick or misty, and the moon is low down, her lustre is greatly reduced, and so the idea of greatly increased distance is suggested. The mind is unable to distinguish (except, of course, by reasoning) between the idea that an object's distance has been increased without apparent change of size, and the idea that the object is magnified. In other words, the mind cannot distinguish between an object's seeming to look larger, and its really looking larger in the sense of occupying a larger area on the retina.[1] Hence the horizontal moon looks larger when the air is dense than when the air is clear. It need hardly be said that when micrometrically measured, the swollen-looking horizontal moon seen through dense or misty air is found to be no larger (in apparent diameter) than the horizontal moon seen through clear air; while the horizontal sun or moon, whether seen through clear or misty air, is not

[1] This is well shown by the phenomenon called 'looming,' at least in the great majority of cases to which this term is applied. There are, of course, cases in which an object is really magnified in a vertical direction by local peculiarities of the air, such cases being, however, far too abnormal to be in any way in question in dealing with the apparent enlargement of the horizontal sun or moon—besides that they always involve distortion. It is this last circumstance that distinguishes special cases of looming from that general enlargement of objects seen indistinctly through mist or haze, which is nearly always understood when looming is mentioned. A ship, bark, brig, schooner, or other object of known dimensions, often looks so gigantic when seen through haze, that the mind cannot get rid of the idea that she has been really magnified. But there is no real magnification. The mind simply plays the judgment a trick, by presenting an apparent increase of distance, unaccompanied by the diminution of size naturally attending increase of distance, as signifying an increase of size. If the angles subtended by the length and height of a looming ship are carefully measured with a sextant, and her real size is known or ascertained, it will be found that her apparent dimensions are just those due to her distance; or if the mist clears away while both the ship and the observer's station remain unchanged, which in harbour frequently happens, it will be found that she looks no smaller through the clear air than through the mist, though she seems to look very much smaller.

Another experiment, which may be very easily tried, shows how readily the mind is deceived in such cases. Look at a capital letter shown strongly in black on a white ground, till the eye begins to weary; then if the eye is directed to the white part of the paper, the letter is seen in brighter white on the white ground, unchanged in size. But if the eye, after long intent gazing on the black letter, is directed to a distant wall, a monstrous letter is seen on the wall, which yet, monstrous though it seems, looks no larger than it did before. It only seems to look larger, because the mind unconsciously sets it so much farther away; but it occupies the same area on the retina.

Rather amusingly, a patient observer some time since made a series of experiments on illusions affecting apparent size, with the object of obtaining some new explanation of the apparent enlargement of the horizontal sun and moon. It rather impaired the value of his results that he interpreted all the illusions as indicating real changes of the dimensions of the retinal images.

found to exceed in apparent diameter the moon shining high overhead. Yet the mind is scarcely able to admit the possibility that the swollen-looking copper-coloured horizon moon seen through a haze or mist occupies no larger, nay, in reality a somewhat smaller portion of the retina than the same orb when shining with full lustre near the zenith.

(589.) By the methods described in Articles 578–580. the moon's distance has been determined with gradually increasing accuracy; and the mean distance at present assigned, 238,830 miles, may be regarded as correct within limits of error not ranging more than twenty miles or so either way. The moon's mean equatorial horizontal parallax is $57' 3''$; her diameter estimated from her assigned distance is 2159·8 miles.

(590.) The determination of the sun's distance is a task of much greater difficulty.

It need hardly be said that the astronomer does not attack the problem directly. Looking at Plates IX. and X. and recognising the fact that though the scale may not be ascertained with precision the precise proportions are known, we see that since the Earth travels on the third orbit from the sun, the astronomer has the best chance of determining the scale of the solar system from observations of Venus and Mars, the planets travelling in the second and fourth orbits, when these bodies are nearest to him.

(591.) Venus, as having the nearer orbit of the two, would attract the astronomer's attention first.

It is obvious (see Plate IX.) that at any time when Venus is at her nearest to the Earth, she is apparently close to the sun, a position very unfavourable for observation. But sometimes Venus when thus at her nearest to the Earth is so near the plane of the Earth's orbit as to appear to cross the face of the sun. At such times the sun's face serves as a sort of index-plate, on which Venus's position can be measured by observers stationed on different parts of the Earth's surface, and thence her distance determined.

(592.) Venus can only be thus seen to traverse the face of the sun if she passes between the sun and Earth near one or other of the points where she crosses the plane of the Earth's orbit—her *nodes*, as they are called. Their positions are shown in fig. 172 at V and V′, V v V′v' being the orbit of Venus, E e E′e' that of the Earth, S the sun, Æ the Earth's position at the autumnal equinox. Thus Venus can only pass over the sun's face, or a *transit of Venus* can only occur, when she is in *inferior conjunction* at or near the time when the Earth is either at E Venus being at V, or at E′ Venus being at V′. The Earth is at E on or about December 7, and at E′ on or about June 6, and no transit can occur unless at the time of conjunction the Earth is on one or other of the short arcs $p\,p'$, $q\,q'$, the former of which is traversed in about 3½ days, the latter in about 3⅔ days.

(593.) Venus moves so nearly in synchronism with the Earth, making 13 circuits in almost the same time that the Earth occupies in making eight, that the times of

transits are determined according to very definite laws. Fig 173 illustrates the movements of the Earth and Venus in this respect. Starting from an inferior conjunction of Venus at V, the Earth being at E on the line S V E, they come next into conjunction on the line S V_1 E_1, the Earth having made $1\frac{3}{5}$ circuits, Venus $2\frac{3}{5}$ (approximately); next on the line S V_2 E_2, the Earth having made $3\frac{1}{5}$, Venus $5\frac{1}{5}$ circuits (approximately);

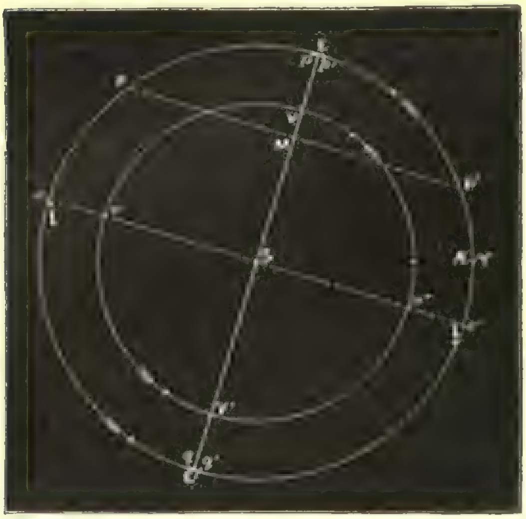


Fig. 172. Showing when transit of Venus can occur.

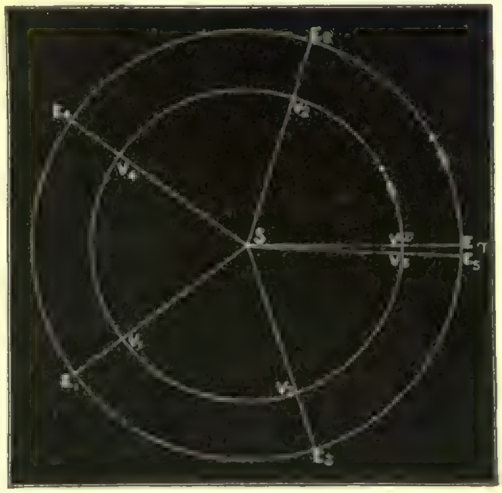


Fig. 173.—Illustrating the movements of the Earth and Venus (six conjunctions).

next on the line S V_3 E_3, the Earth having made $4\frac{4}{5}$, Venus $7\frac{4}{5}$ circuits (nearly); next on the line S V_4 E_4, the Earth having made $6\frac{2}{5}$, Venus $10\frac{2}{5}$ circuits (nearly); and lastly on the line S V_5 E_5, the Earth having made nearly 8 and Venus nearly 13 circuits, the actual deficiency for each planet—viz. the angle E S E_5—being only about $2\frac{1}{2}°$.

(594). Hence, when a transit has taken place, the chances are that 8 years after, when Venus will be in inferior conjunction only $2\frac{1}{2}°$ behind the former conjunction line, there will again be a transit—for the transit arcs $p\,p'$ are 3° 22′ and 3° 32′ long respectively.[1] A second transit is not *certain*, for if the first happened when the Earth was near the middle of either arc $p\,p'$ or $q\,q'$ the next would find her about $2\frac{1}{2}°$ beyond the middle of such arc, or outside of the arc, since $2\frac{1}{2}°$ is greater than the half of either arc. Still, in the majority of cases the transits thus occur in pairs. Fig. 174 illustrates the manner of this for December transits. A conjunction is supposed to have taken place on the line V E passing athwart the transit arc $p\,p'$; the next conjunction-lines are numbered 1, 2, 3, &c. to 5, the last falling 8 years later (less $2\frac{1}{2}$ days) on the line V_5 E_5, and again passing athwart the transit arc $p\,p'$; but 8 years (less $2\frac{1}{2}$ days) later still, the planets come to the conjunction-line numbered 10, which is outside the arc $p\,p'$. A similar construction would apply to the case of June transits.

(595.) After entering a transit-arc the conjunction-line, after each 8-year period,

[1] Owing to the eccentricities of the orbits both of Venus and the Earth, and the consequent variation in the rates of these planets' motions, the arcs $p\,p'$ and $q\,q'$ are not exactly equal in length. They are calculated in my *Transits of Venus*, p. 114, 3rd edition, and determined at 3° 22′ and 3° 32′ respectively. But as the Earth traverses an arc of nearly 1° 1′ about the Sun each day in the first week of December, while she moves only through about $57\frac{1}{3}'$ per day in the first week of June, the difference in the time of traversing these arcs is relatively greater, being less than $3\frac{1}{3}$ days for December transits, while it is more than $3\frac{3}{4}$ days for June transits.

passes back, step by step, $2\frac{1}{2}°$ back each time. At this rate it would occupy about 576 years (8 times 72, the number of times $2\frac{1}{2}$ is contained in 180) reaching the other

FIG. 174.—The conjunction lines (two transits) of Venus and the Earth.

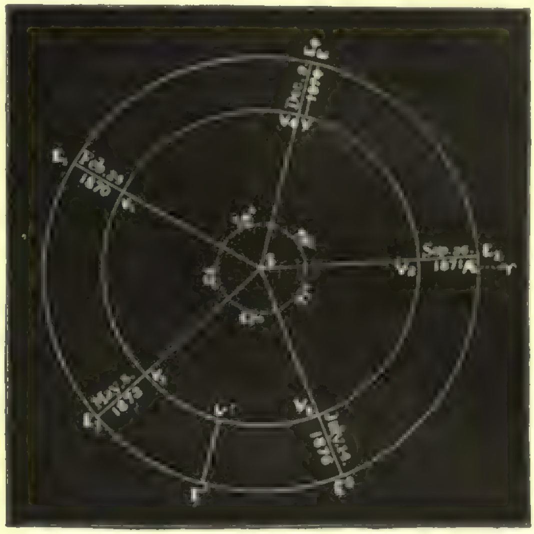


FIG. 175.—Illustrating the effects of the varying rates of motion of Venus and the Earth.

transit arc. But two other conjunction-lines, as we see from figs. 174 and 175, have first to pass that transit arc, the nearest having only $\frac{1}{5}$ of the distance to traverse, and so reaching it in a mean period of about 115 years, when another pair of transits (or it may be a single transit) will occur; and so on continually, the transits alternating between the December and June periods.

(596.) If the orbits of the Earth and Venus were not eccentric these relations would be uniform and uniformly repeated. But the orbits being eccentric, the motions are not quite uniform, as the arcs and dates indicated in fig. 175 serve to show. These are the conjunction-lines from the transit of December 9, 1874, to that of December 6, 1882. Owing to this want of uniformity, the interval between pairs of transits is not equal.

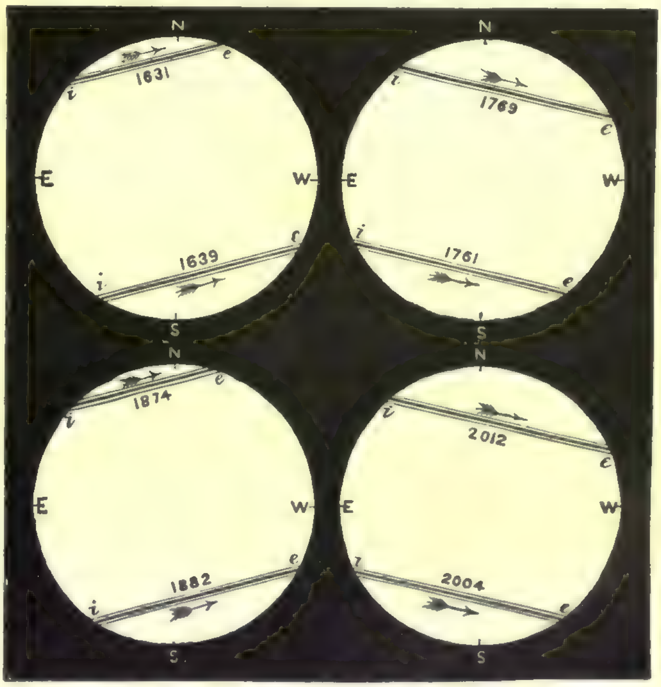


FIG. 176.—Paths of Venus across the Sun's face in 1631, 1639, 1761, 1769, 1874, 1882, 2004, and 2012.

From the second of a December pair of transits to the first of a June pair the interval is $121\frac{1}{2}$ years, whereas from the second of a June pair to the first of a June pair the interval is only $105\frac{1}{2}$ years.

(597.) Thus transits of Venus recur usually at intervals of 8 years (following first December transit), 121½ years to first June transit, 8 years, 105½ years, 8 years, 121½ years, and so on. This has gone on in the past and will continue in the future for many centuries, because it so chances that 243 years, the interval from a first December transit to the next, or from a first June transit to the next, is so nearly equal to 395 periods of Venus, corrected for the slow retrograde motion of Venus's node (so as to be made nodical periods), that the first of a pair of December transits is almost identical with the first of the next preceding and next following pairs of December transits, and the like for the first of a pair of June transits. This will be obvious from fig. 176, which shows the calculated paths of Venus across the sun's face in eight transits. Those of 1631 and 1639 are December transits almost identical with those of 1874 and 1882, which were the next pair of December transits; those of 1761 and 1769 were June transits almost identical with those of 2004 and 2012, which will also be June transits.[1]

(598.) Fig. 177 illustrates the principle on which the sun's distance is determined during a transit of Venus, though not the methods actually employed. V represents

Fig. 177.—Illustrating the determination of the Sun's distance by the observation of Venus in transit.

Venus passing between the Earth E and the sun S; and we see how an observer at E will see Venus as at v, while an observer at E′ will see her as at v'. The measurement of the distance $v\,v'$ as compared with the diameter of the sun's disc determines the angle v V v' or E V E′; whence the distance E V can be calculated from the known length of the base line E E′.[2] For instance, it is known (from the known proportions of the solar system) that E V bears to V v the proportion 28 to 72, or 7 to 18; whence E E′ bears to $v\,v'$ the same proportion. Suppose, now, the distance between the two stations E and E′ is known to be 7,000 miles, so that $v\,v'$ is 18,000 miles, and that $v\,v'$ is observed to be $\frac{1}{48}$ part of the sun's diameter. Thus the sun's diameter, as determined from this observation, is 48 times 18,000 miles, or 864,000 miles; whence from his known apparent size, which is that of a globe 107⅓ times farther away than its own diameter, his distance comes out 92,736,000 miles.

[1] The resemblance is still better indicated by comparing the lengths of the chords traversed by Venus in 1761 and 2004, two first June transits, and in 1769 and 2012, two second June transits. These were as follows (the times being for the earth's centre):—

			h.	m.	s.
Transit of June	1761	lasted	6	16	25
,, ,,	2004	will last	6	1	57
,, ,,	1769	lasted	6	0	56
,, ,,	2012	will last	6	19	55

The shortening in 2004 (by 14m. 28s.) indicates that the path of transit will be slightly lower than in 1761, or slightly farther from the centre; while the lengthening in 2012 (by 18m. 59s.) indicates that the path of Venus will also be slightly lower than in 1769, and therefore slightly nearer to the centre.

[2] It will be observed that $v\,v'$, the actual displacement of Venus as seen from the Earth E, is less than the real displacement as indicated by the direction-lines E V, E′V (or as measured by either distance E E′ or $v\,v'$ supposed to be observed from V), in the same degree that V v is less E v. This corresponds with the fact that the observers at E and E′ see the Sun as well as Venus displaced by parallax, so that it is not the full displacement of Venus as seen on the star-sphere, but the excess of her displacement over the displacement of the sun, which is actually measured.

(599.) This may be called the *direct method*, and now that photography is taking so large and growing a part in astronomical work, this method, with photographic aid for recording the place of Venus on the sun's face as observed from different stations, will probably come effectively into use. The usual methods of measurement fail. The observers of a transit are necessarily stationed far apart and where the observing conditions are all different, one set of observations being made in midwinter of the northern or southern hemisphere, the other in midsummer of the southern or northern. Hence the comparison of direct measurements is not satisfactory. The application of photography during the transits of 1874 and 1882 failed utterly, no adequate preliminary experiments having been made by the official astronomers responsible for such service. No doubt during the transits of 2004 and 2012 photography will be applied in ways such as the astronomers who arranged the plans for the last pair of transits did not even think of, and difficulties which in 1874 and 1882 seemed insuperable will be overcome.

(600.) Other methods, devised by Halley and Delisle in anticipation of the pair of transits which took place in 1761 and 1769, were recognised as the best methods available for observing the transits of 1874 and 1882.

It will be seen from fig. 177 that Venus, unless her line of transit is a diameter of the sun's face, traverses chords of unequal length as seen from different stations. In the case there illustrated *l m* the chord of transit for a southern station is shorter than *l′m′* the observed chord of transit for a northern station. It will readily be understood that if the duration of transit be observed from sets of northern and from sets of southern stations, the lengths of such chords as *l m* and *l′m′* can be determined, and hence their distance *r r′* from each other, which is what is wanted for the determination of the sun's distance. Of course the problem is complicated by the circumstance that during the transit (which may last several hours[1]) the Earth is rotating, in such sort that the centre of Venus's disc does not traverse a simple chord, but a slightly curved line corresponding to the rotational movement of each station. An illustration of the various paths traversed by Venus's centre as seen during transit from several stations is given in fig. 178, in which are shown, 1, the track of Venus's centre as supposed to be seen from the Earth's centre; 2 and 3, the limiting tracks (those next to the central track) of Venus's centre as seen from shifting stations always farthest north and south during the transit; 4 and 5, the tracks (those next to the outer dotted tracks) touched by Venus's disc as supposed to be seen from the Earth's centre; 6 and 7, the tracks (dotted) touched by Venus's disc as supposed to be seen most displaced north and south throughout the transit; and lastly, inside tracks 2 and 3, the tracks of Venus as seen from several northern and several southern stations (the tracks for northern stations being lower, those for southern stations higher, than the central track) which were shifted by rotation in different ways and at different rates during the progress of the transit.[2]

[1] The maximum duration is about 8 hours; in the transits of 1761, 1769, and 1882 the duration was about 6 hours, and in the transit of 1874 the duration was about 4 hours.

[2] Three outlines are given to the sun: the middle one corresponds to that supposed to be seen from the Earth's centre; the others to the greatest and least distances from the centre obtainable by ranging over the Earth's surface. Fig. 178 is a miniature of a picture, Plate XIX., of my *Transits of Venus*, in which these tracks are shown on such a scale that the median transit track is about 3½ feet in length. On such a scale a drawing can be obtained, and is there presented, from which the track of Venus's centre for any point whatsoever on the Earth can be marked in at once. The plan to be pursued depends on the following considerations:—

(601.) For this method it is obviously essential that the whole transit, or at least its beginning and end,[1] should be seen. As it is not always possible to secure a sufficient number of stations thus favourably situated, Delisle proposed another plan especially for observing the transit of 1761, the first of a pair of transits being usually unfavourable for Halley's method, as will presently be seen.

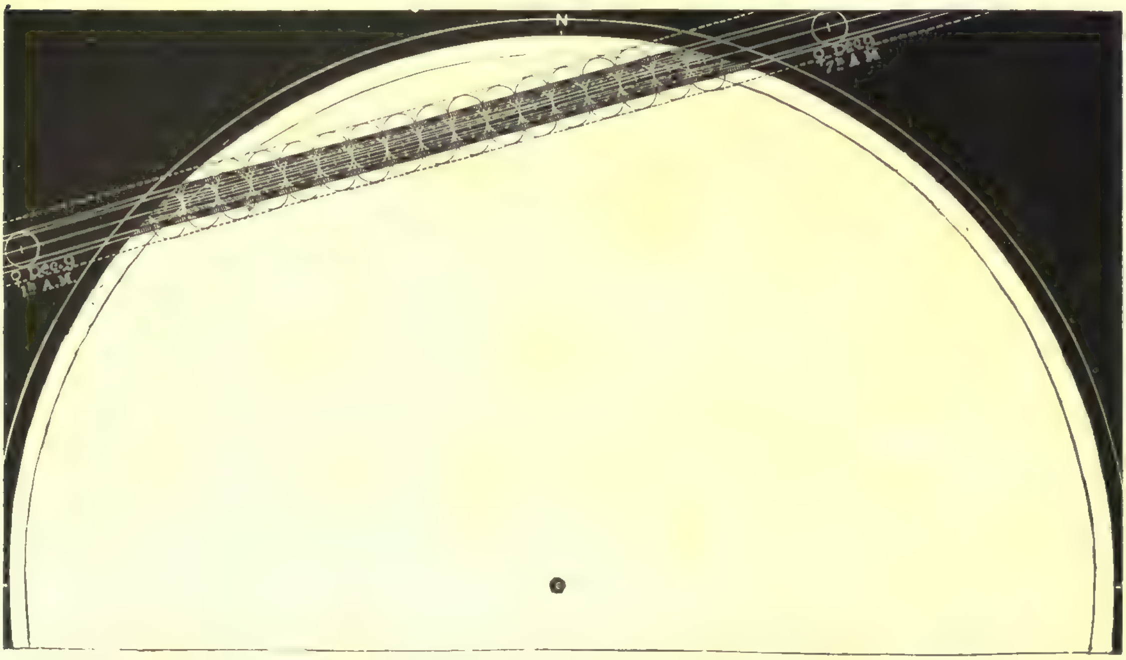


Fig. 178.—Various paths traversed by Venus's centre as seen from different parts of the Earth during the transit of December 9, 1874.

If we imagine that at any moment of the transit a straight line, 93 millions or so of miles in length, passing through the centre of Venus, has one extremity on the Earth's surface, and that this extremity is carried in an instant, after the fashion of a pantograph pointer, round the globe of the Earth, round the outlines of all the continents and islands on the sun-lit half of the Earth, along meridians and parallels (10°, say, apart) and to every important town on that face of the Earth, the other end will trace on the Sun an introverted picture of the Earth, its lands and seas, its meridians and parallels, and the positions of the several towns (12 are dealt with in the plate above named). And quite obviously, any point thus supposed to be indicated on the Sun's face is the point at which the centre of Venus was visible at the moment on the Sun's face from the corresponding point of the Earth's surface. Doing this at convenient time-intervals—in my picture each interval is a quarter of an hour—we obtain a series of introverted pictures of the Earth, and the successive positions of Venus's centre (at such intervals) as seen in transit across the face of the Sun from as many stations as we care to deal with. To introduce a transit-chord for any station not dealt with in the drawing all that is necessary is to mark in the station with its proper longitude and latitude in each of the introverted pictures of the Earth, and to connect the points thus obtained by a line: this line will be the true transit-chord for the station dealt with.

The student will find it an interesting task to apply this method to the transits of 2004 and 2012, using the elements presently given in the text. He will thus be able at the cost of a few hours of pleasant work to ascertain precisely the conditions under which those transits will be observable at every station whence any part of either transit can be seen. He can thus, in fact, easily, more than a century in advance, do just what the chief official astronomer of England should have done several years before the transits of 1874 and 1882, but failed to do,—obtaining only imperfect and misleading results from unsatisfactory methods.

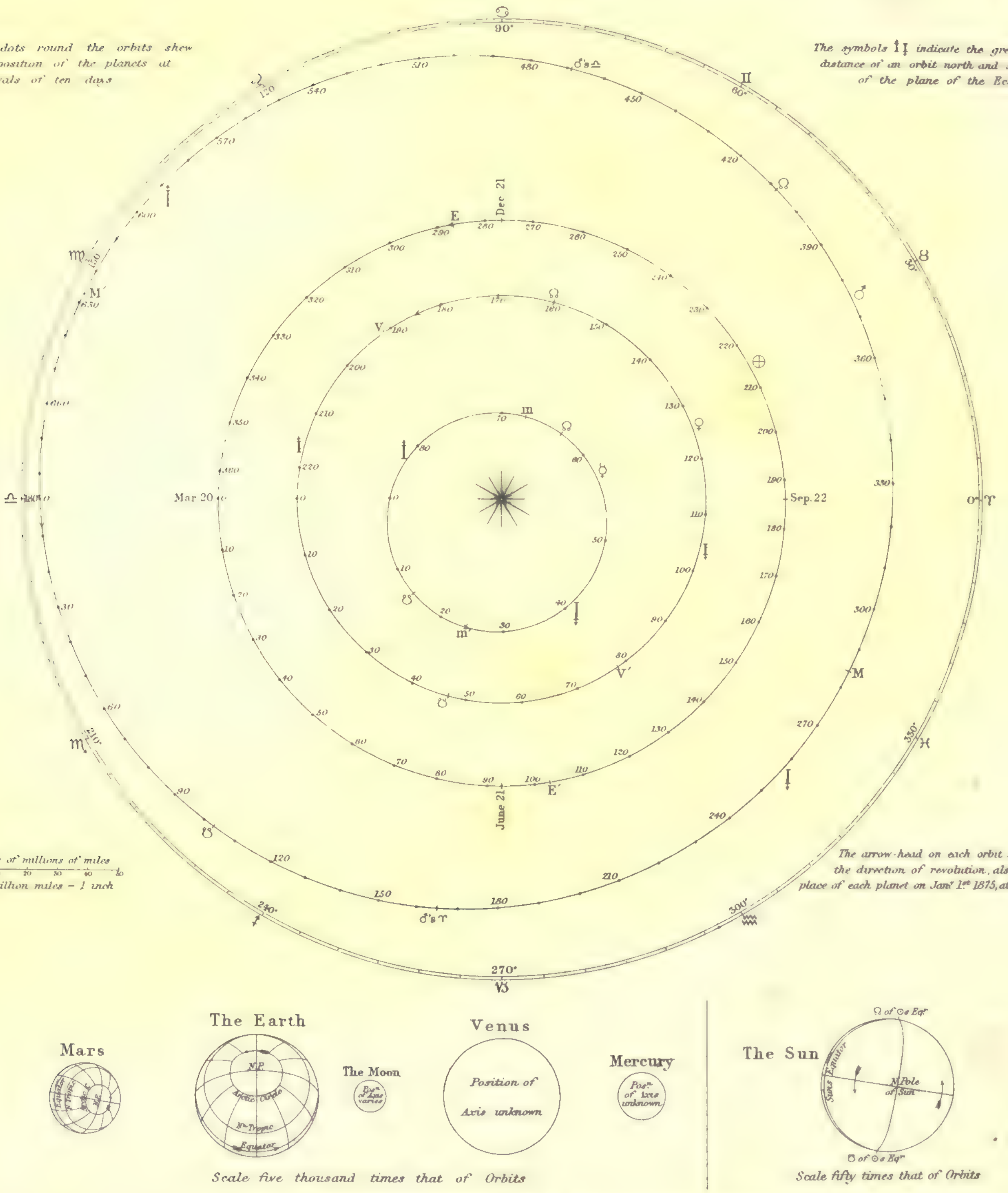
[1] The beginning and end may be seen at certain arctic and antarctic stations, although during the progress of the transit in the interval the observer's station is on the dark side of the Earth and neither Venus nor the sun visible.

ORBITS OF MARS (♂), THE EARTH (⊕), VENUS (♀), AND MERCURY (☿).

Nodes ☊ ☋, Nearer Apses M.E.V.m.

The dots round the orbits shew the position of the planets at intervals of ten days

The symbols ↥↧ indicate the greatest distance of an orbit north and south of the plane of the Ecliptic



Scale of millions of miles

50 million miles = 1 inch

The arrow-head on each orbit shews the direction of revolution, also the place of each planet on Jany 1st 1875, at noon

Scale five thousand times that of Orbits

Scale fifty times that of Orbits

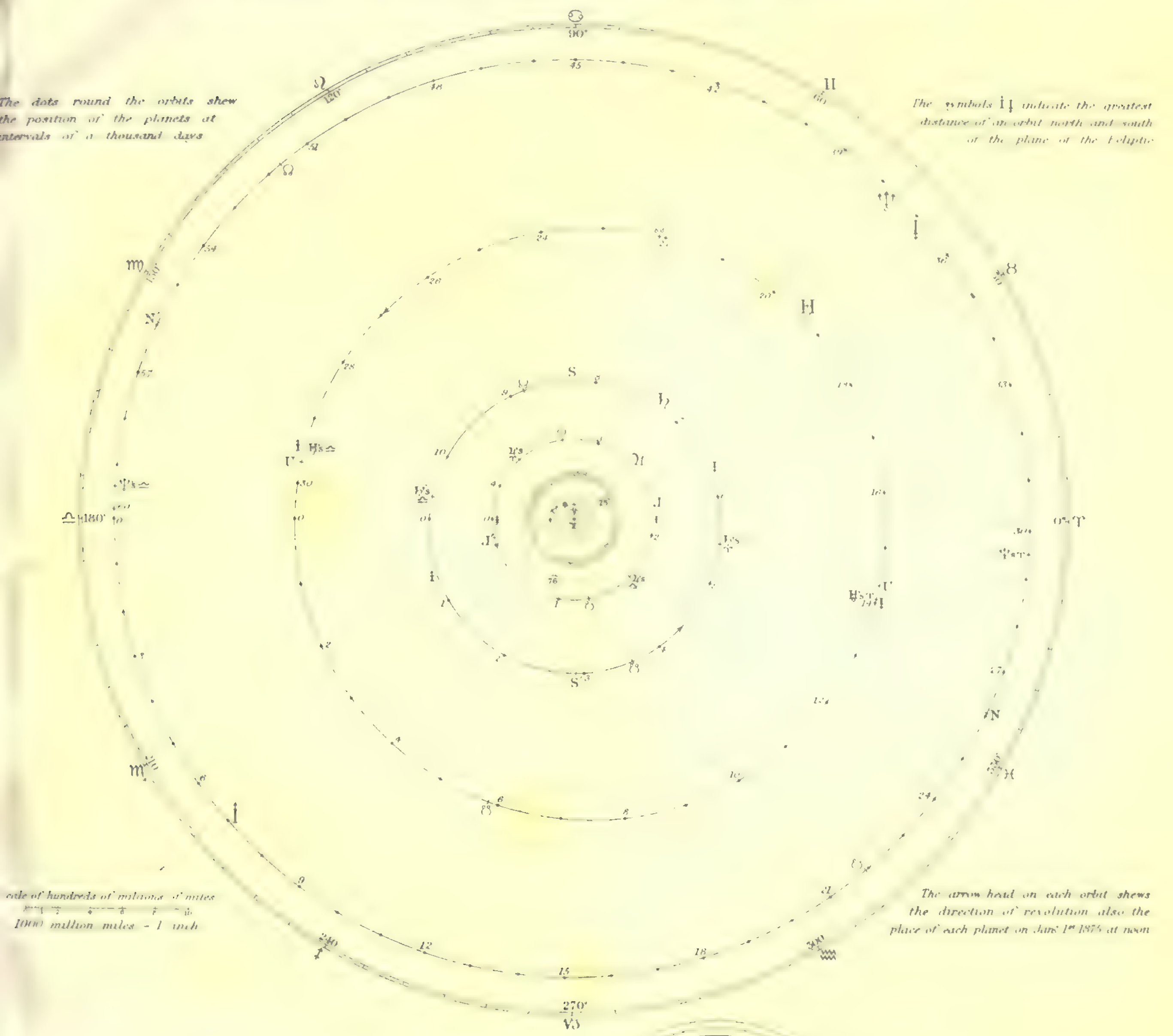
(The Earth, Mars, and the Sun, are shewn in their true axial position)

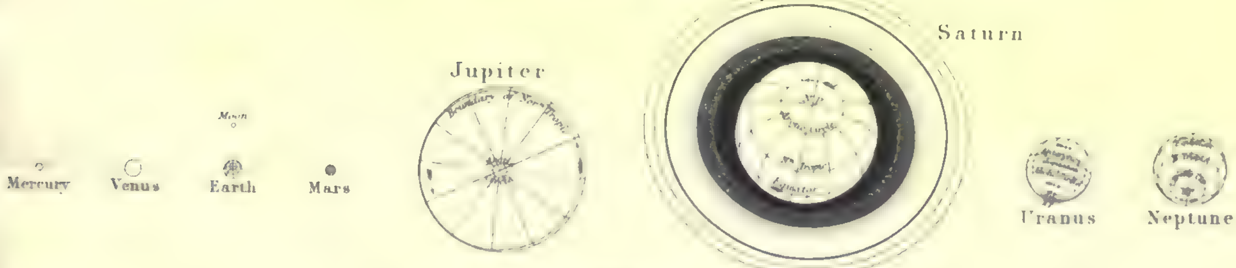
R.A.Proctor delt J. Bartholomew sculp

ASTRONOMY

ORBITS OF NEPTUNE ♆, URANUS ♅, SATURN ♄ AND JUPITER ♃.

Zone of Asteroids, and orbits of 76, 25 ♂, ⊕, ♀ and ☿. Nodes ☊ ☋. Nearer Apses N U S J

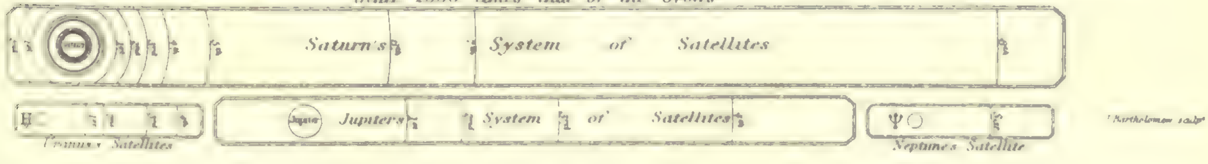




Scale of Planets 10,000 times that of Orbits

(Jupiter and Saturn are shewn in their true axial position, Uranus and Neptune in the axial positions inferred from the motions of their Satellites)

Scale 2000 times that of the Orbits



J. Bartholomew sculpt

(602.) It will be obvious from fig. 177, that at stations near E′, Venus, being situate as shown, the transit will begin earlier than at those near E, while at the former the transit will end later than at the latter). Delisle suggested that by setting observers near that part of the Earth where the transit of 1761 began earliest, and other observers near that part where it began latest, and determining the interval elapsing between the beginnings of transit as observed by these sets of observers, the distance between the chords *l m* and *l′m′* might be determined as readily (theoretically) as by observing the actual durations of transit. The like could also be as readily effected by setting observers where the transit ended earliest and latest.

(603.) Delisle's method requires exact knowledge of the absolute moment of observed ingress (or egress, as the case may be), whereas for Halley's method the determination of the absolute moments of ingress and egress is not necessary, the interval between the two being all that is required. The clock may show wrong time at the beginning, or indeed it may be started at the beginning of transit like a stop-watch: so long as its rate continues true during the transit, the Halleyan observation is effective. In applying Delisle's method the exact longitude of each observing station must be known, and also the true local time, otherwise no satisfactory determination could be made of the interval elapsing between the moment of beginning or ending at a northern station and the corresponding moment at some southern station six or seven thousand miles away. But longitudes could not be observed in the middle of the eighteenth century with the accuracy necessary to make this method effective; and the Delislean observations made in 1761 and 1769 proved of very little value compared with the observations of duration on Halley's plan.

(604.) The circumstances on which the choice of stations for observing a transit of Venus by either of these methods depends, are somewhat complicated and cannot be fully understood without considering the problem from several different points of view, as I have done in my 'Transits of Venus.' Here I can follow but a single line of explanation, choosing the one which seems to me on the whole the best.[1]

FIG 179. Venus's Shadow-cones umbral, penumbral, and partial.

(605.) Let S represent the Sun, V Venus, and E the Earth (supposed to be travelling just in front of Venus's advancing shadow-cone *v* V *v′*—all being monstrously exaggerated in size compared with the distances S V and S E). The shadow-cone has three parts: (1) The true shadow just behind Venus and not reaching very far from her; next, shaded in the figure, that part of the cone from within any point of which the whole disc of

[1] Sir Edmund Beckett (now Lord Grimthorpe) followed another method in that admirable work of his, *Astronomy without Mathematics*, which in reality corresponds to simply inverting the direction of view. In his method we are supposed to look from some station beyond the Earth towards the Earth, Venus, and the Sun; in mine we look from the Sun towards Venus, the Earth, and beyond. I would recommend the reader who wishes thoroughly to understand the matter to compare Lord Grimthorpe's explanation with my explanation here. Such matters can seldom be properly understood unless viewed from more standpoints than one. In reading any section of the *Astronomy without Mathematics* the student has the satisfaction of knowing that he is studying the reasoning of a mathematician who has mastered his subject so thoroughly that he can undertake to present it without mathematical or technical complications. The trouble about most simple explanations is that they are simply wrong.

Venus would be seen on the sun's face; and outside that, the portion of the cone from any point within which a part of Venus's disc (greater or less, according as the point is near the shaded part of the cone) would be seen on the sun's face. We may conveniently call these parts (1) the true shadow-cone (with which, however, we have no occasion to deal); (2) the umbral shadow-cone; and (3) the penumbral shadow.

(606.) Now it is obvious that so soon as the outer surface of the double cone $r\,V\,r'$ in its relative advance overtakes the Earth, Venus is seen (at that point first on the Earth's surface where the shadow-cone first touches the Earth) to begin to enter on the sun's surface; and when the inner surface of the double cone reaches the Earth, Venus is seen (at that point first on the Earth's surface where the inner surface of the cone first touches the Earth) to be just wholly within the Sun's disc. Passing on, the inner surface of the shadow-cone sweeps across the Earth until the Earth is just wholly within it, the point where it then touches the Earth being that where the complete entry of Venus on the sun's face is seen latest. From this moment the transit is in progress for the whole of the Earth's surface turned sunwards, until the double shadow-cone passes off the Earth, with corresponding effects in reverse order; first a point on the Earth being touched where Venus is earliest seen just touching the sun's disc on the inside, then a point on the Earth where Venus is latest seen so touching the sun, and last of all a point on the Earth being touched where Venus is last seen just touching the sun's disc on the outside.

(607.) Fig. 180 represents the true relative dimensions of the inner and outer shadow-cones where they sweep over the Earth, overtaking her (because of the swifter angular motion of Venus round the sun) from left to right. The Earth's disc is shown on the same scale. By conceiving the shadow at rest and the Earth passing through it from right to left with her proper relative motions, we get for the transit of 1874 the successive positions indicated in the figure, viz.—first the external contact (not numbered, as not being an important phase); then the earliest internal contact at ingress, (1); then (2), the latest internal contact at ingress; next (3 to 13), successive positions of the rotating Earth on her way through the shadow-cone, corresponding to successive positions of Venus in transit across the sun's face; then (14), the earliest internal contact at egress; and lastly (15), the latest internal contact at egress. The track of the Earth through Venus's shadow-cone during the transit of 1882 is also shown in fig. 180.

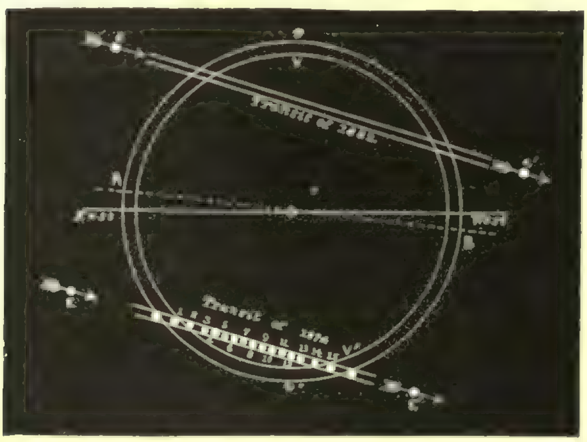


Fig. 180.—Passage of the Earth through Venus's Shadow-cones, in 1874 and 1882.

(608.) We require, however, to have the little discs representing the Earth enlarged, so that the effects of rotation may be duly taken into account. This is done in fig. 181 for the transit of 1874, and in fig. 182 for the transit of 1882.[1] The

[1] The transits of 1631 and 1639 are approximately included in these figures, the transit-chords being, as already stated, nearly the same during the transits of 1631 and 1874, and also during the transits of 1639 and 1882.

enlarged Earth-disc numbered 22 (the numbers thus shown on the figure belonging to their place in my 'Transits of Venus') shows how the front edge of the shadow-cone touching the Earth's face at *i* (*ingress most accelerated*) crosses it centrally at *e e'* (*mean ingress*) and again touches the Earth's face at *i'* (*ingress most retarded*). The

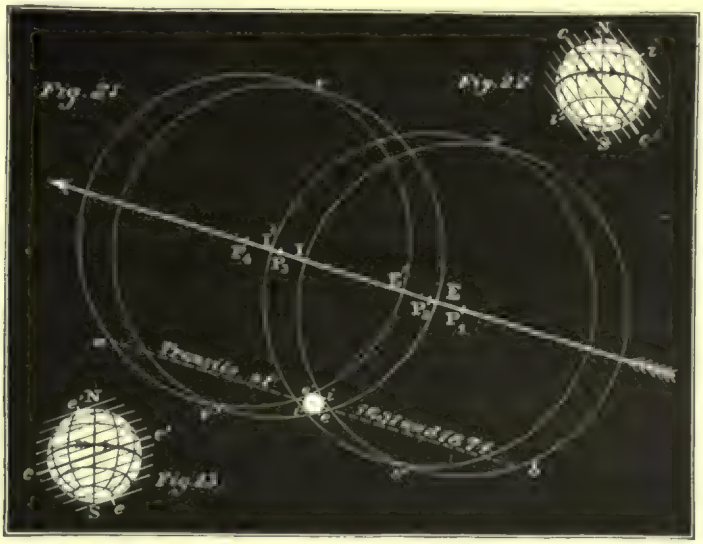


FIG. 181.—Passage of Venus's Shadow-cones over the Earth in 1631 and 1874.

disc numbered 23, shows in like manner how the rear edge of the shadow-cone first touches the Earth's face at *e* (*egress most accelerated*), crosses it centrally at *e e'* (*mean egress*), and again touches it at *e'* (*egress most retarded*). If the reader compares the

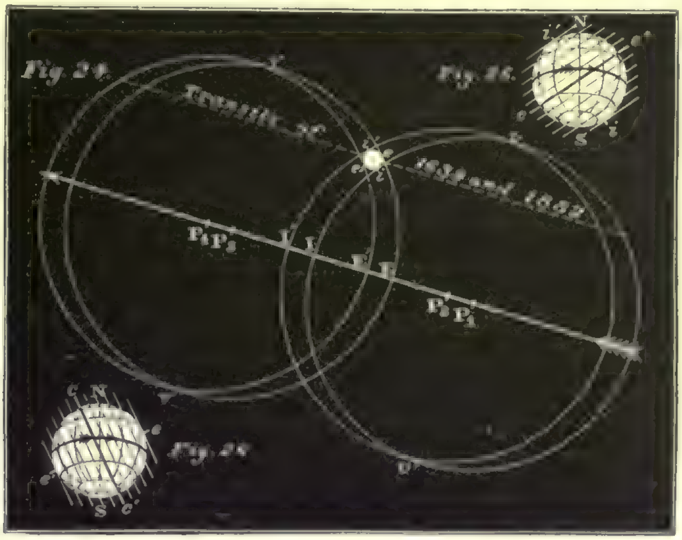


FIG. 182.—Passage of Venus's Shadow-cones over the Earth in 1639 and 1882.

lettering round the enlarged discs with that round the small disc *i e i'e'* he will have no difficulty in interpreting the position of the thwart lines on the enlarged discs, which represent the advance of the front edge and of the rear edge of the shadow-cone over the face of the rotating Earth.

(609.) Fig. 182 is to be similarly interpreted in considering the transit of 1882. Fig. 183 shows in like manner the progress of the shadow-cone of Venus over the face of the rotating Earth during the transits of 2004 and 2012, the upper pair of enlarged discs relating, as in the preceding cases, to the ingress and egress during the transit of 2004, the lower pair to the ingress and egress during the transit of 2012. The transits of 1761 and 1769 are almost equally well illustrated by the same figure.

(610.) So far as Delisle's method is concerned there is little to choose between one transit and another, as figs. 181 and 182 show. But there is an important difference for Halley's method, by which the first of a pair of transits is generally rendered less suitable for this method than the second. For Halley's method, it is essential that the difference of observed durations should be as great as possible; for this, one set of observers should have the beginning of transit as much accelerated and the

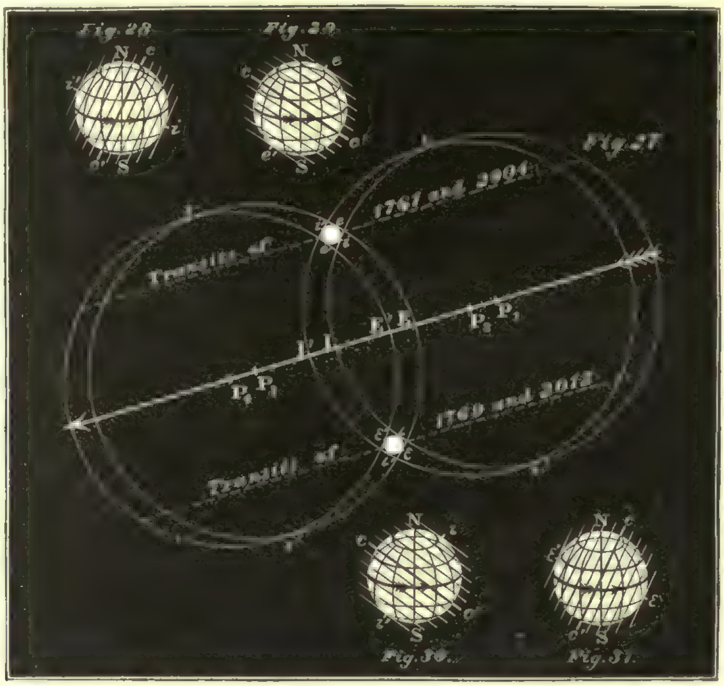


Fig. 183.—Passage of Venus's Shadow-cones over the Earth in 1761, 1769, 2004, and 2012.

end as much retarded, while the other set of observers should have the beginning as much retarded and the end as much accelerated, as possible.

(611.) Thus in all the cases illustrated by figs. 181, 182, and 183, it is desirable for Halley's method that one set of observers should be as near as possible both to i and e, while the others are as near as possible both to i' and to e'. Now if the student considers a point near i in fig. 183 (on the enlarged disc 28), he will see that the Earth's rotation, the direction of which is shown by the arrow, would presently carry such a point to the unilluminated side of the Earth, whence the end of the transit could not be observed. In the application of Halley's method, a northern observer must be set so far to the west of i that he will be near the mean ingress line $c\,c'$, and where ingress will not be much accelerated: a southern observer, on the contrary, can be set as near to i to observe ingress retarded as is consistent with sufficient elevation of the sun (the sun being, of course, on the horizon as seen from the point i' itself).

Now remembering that the meridians shown in the enlarged Earth discs of fig. 183 are two hours apart, while the transit lasted about six hours, it is seen that northern stations on the Earth, which at the beginning of the transit were near *i*, were at the end not very far from the line *c c′* of the second enlarged disc (29), or had egress very little retarded. The effect of rotation at northern stations, then, was to reduce the lengthening of transit due to position. As for southern stations, it will be obvious that a station which at the beginning of the transit was near *i′*, at the end was not very far from *c c′*, the mean egress line of disc 29; nor could much be gained by having stations which were near *c* at egress, since they were correspondingly ill placed at ingress. Here again then the effects of rotation tended to diminish that difference of durations on which the effective application of Halley's method depends.

(612.) But if we consider the later transits illustrated in the lower part of fig. 183, we shall see that rotation helps to increase the difference of duration. We can have a station near *ι* which, though presently passing to the unilluminated part of the Earth, is brought round in four or five hours to the illuminated part not very far from *ε′*, while a station near *ι′* at ingress will be carried near to *ε* at egress. Thus the beginning of transit is much accelerated and the end much retarded at suitably selected northern stations, while the beginning is much retarded and the end much accelerated at suitably selected southern stations; and Halley's method is applicable under very favourable conditions.

(613.) By studying in the same way figs. 181 and 182, and comparing the conditions under which the transit of 1874, first of a pair, and that of 1882, second of a pair, can be observed by Halley's method, it will appear at a first view that precisely similar considerations apply. Considering the effects of rotation qualitatively, they tend to diminish the difference of duration in the earlier of a pair of December transits, while they tend to increase that difference in the later transit of the pair. But when the effects of rotation are considered quantitatively, and when geographical considerations are taken into account, it is found that Halley's method is suitable for the earlier of such a pair, and practically unavailable for the second.

(614.) It was here that Sir G. Airy fell into error in his investigation of the transits of 1874 and 1882, and his resulting suggestions for observing those transits. He noticed, as far back as 1857, that both in the case of a June pair of transits and of a December pair, the Earth's rotation diminishes the difference of duration for the first of the pair, while it increases the difference for the second. And unfortunately, when in 1868 he for the first time dealt with the actual details of the transits of 1874 and 1882, he was so satisfied that the conclusion he had deduced in 1857 is general, that he limited his consideration of Halley's method to the second transit of the pair. He fell into a double error, in fact. He not only supposed that Halley's method failed totally for the transit of 1874, but he did not duly consider the difficulties affecting the application of Halley's method in 1882.[1]

[1] As I have been taken to task for mentioning this by some among the many mutually contradictory defenders of the Astronomer Royal's action throughout the matter of the transits, it may be well to quote the words with which he opened in 1868 the discussion of what he called 'the preparatory arrangements necessary for the efficient observation of the transits of Venus in 1874 and 1882.' 'On two occasions,' he began, 'I have called the attention of the Society to the transits of Venus across the sun's disc, which will occur in the years 1874 and 1882; and have pointed out that, for determination of the difference between the sun's parallax and the parallax of Venus, the method by observation of the interval in time between ingress and egress at each of two stations at least, on nearly opposite parts of the Earth' [commonly called *Halley's*

(615.) It will have been noticed that, to get the full advantage of rotation effects in the second transit of a pair, stations must be selected in very high latitudes, where the beginning and end, but not the middle, of the transit can be seen, or else in still higher latitudes where the sun does not set at all during the transit. In a June transit such stations will be in high northern latitudes, and many such can be found; in a December transit such stations must be sought in high southern latitudes, and there are scarcely any such, if indeed there are any at all. For the 1882 transit Sir George Airy pointed to two Antarctic stations, and two only—one in Repulse Bay, Sabrina Land (the chief objection to which was that there was no landing there), the other at Possession Island, Victoria Land; for which the Astronomer Royal had unfortunately, chiefly through the roughness of his maps, deduced a wrong solar elevation at the time of ingress, viz. 10°, instead of about 5°—altogether insufficient for satisfactory observation.

(616.) On the other hand, Sir G. Airy had failed to notice that the disadvantage of the earlier transit of a pair depends in large degree on the duration of the transit; in the case of a transit lasting only four hours, as in December 1874, instead of six, as

method] '(on which method, exclusively, reliance was placed in the treatment of the observations of the transit of Venus in 1769), *fails totally* ' [the italics are mine] 'for the transit of 1874, and is embarrassed in 1882 with the difficulty of finding a proper station on the almost unknown Southern Continent. The publication of M. Le Verrier's new tables of Venus, and of Mr. Hind's inferences from them as to the points of the Sun's limb at which ingress and egress will take place in each transit (which inferences I have in part verified), has induced me again to examine the whole subject. And, without giving up the hope of using the observation of interval between ingress and egress at each of two stations in 1882' [*Halley's method*, that is to say] 'I have come to the conclusion (from all the information which has reached me) that it will be unsafe to *trust exclusively to the chance of securing observations on the Southern Continent;* and that, *while observations are by all means to be attempted in that manner*, it is also very desirable to combine with them observations of the same phenomenon (at one time the ingress, at another time the egress), made at nearly opposite stations whose longitudes are *accurately* known, and recorded in *accurate* local time' [commonly called *Delisle's method*]. 'This principle being once admitted, the transit of 1874 is, or may be, as good for *observations of that class* as the transit of 1882; and the selection of localities for the observations must be made with equal care for the two transits.' And accordingly, the Astronomer Royal proceeded to describe the maps in which he had presented the places suitable for observing the transit of 1874 by Delisle's method only, and for observing the transit of 1882 both by Delisle's method and by Halley's. He remarks that the maps 'do not profess an extreme accuracy;' and indeed they were probably the roughest ever used for so important an inquiry; but he thought them trustworthy: 'they will, I trust, be found abundantly accurate for the purpose for which they are prepared.'

In regard to the Antarctic stations for Halleyan observation in 1882, the Astronomer Royal said: 'Choice of stations having been made' [between Sabrina Land and Victoria Land] 'I would not recommend any reconnaissance, but I would propose that an expedition should go direct to the selected point, in good time for the observation of the phenomenon. 'The season,' he added, 'is early for south polar expeditions, and any difficulties produced by ice would probably diminish every day.' Captain Richards, Hydrographer to the Admiralty, specially invited to attend the meeting of the Astronomical Society at which Airy submitted his plans, spoke of the Antarctic plan for 1882 as involving considerable difficulties, though not of such magnitude as many conceived. He suggested that astronomers should winter on Possession Island, Victoria Land, or one of the islands close by, and said he believed there would be '*little difficulty* in reaching one or other of these islands,' adding that a year passed there could be profitably employed in various scientific researches. Rear-Admiral Ommanney was enthusiastic on the same side. Commander Davis, who had accompanied Sir James Ross in his Antarctic voyages, and had himself landed at Possession Island, said there would, he believed, be *no difficulty in effecting a landing at the same spot. He had no doubt a winter could be passed there* '*very comfortably, with a pleasant prospect and plenty of penguins to live on.*' '*In comparison with Kerguelen Land*' [which, be it remembered, was actually occupied in 1874] '*and the Crozets*' Commander Davis '*believed the chances of observation to be greatly in favour of Victoria Land.*'

in June 1761 and 2004, the effects of rotation in diminishing the difference of duration are much less important.

(617.) Comparison being made (with details such as these duly taken into account) it appeared that the transit of 1874 was admirably suited for the application of Halley's method, which, even when weighed according to principles devised by Sir George Airy for the special defence of his plans, was found to be more valuable than Delisle's (and proved so, in the event), instead of failing totally as Sir G. Airy had supposed; while, on the other hand, for the transit of 1882 Halley's method was found to be so far inferior to Delisle's that it might be almost said to fail totally. The projected expeditions to reconnoitre and eventually winter at Antarctic stations were shown to be utterly useless for the transit of 1882, and by no means essential for the earlier transit, though useful if feasible. A number of northern stations in Siberia, North China, and Japan were shown to be desirable for Halleyan observations in 1874. These had very naturally been overlooked by the Astronomer Royal, misled as he was by the idea that Halley's method failed totally for 1874. To this idea and to the roughness of his maps must be attributed his failing to notice the advantages of certain parts of North India both for Halleyan and Delislean observations—a point in which correction was much needed, because these stations falling in British territory, American and foreign astronomers would not have been likely to occupy them, as they did the Siberian, Chinese, and Japanese stations in the north and the stations in high southern latitudes which I had indicated as desirable for Halleyan observations.

(618.) The first useful effect of my suggestions (conveyed to Sir G. Airy privately, at the outset) was that the idea of Antarctic expeditions for 1882 was abandoned. It was suddenly discovered that such expeditions were dangerous and difficult, and would probably prove useless! This being so, it was so much the more satisfactory that the plans for such expeditions in 1882 were abandoned.[1]

(619.) The four figures in Plate XI. serve sufficiently here to show the actual conditions under which the transits of 1874 and 1882 were observed. The first shows the earth's face presented sunwards at the beginning of the transit of 1874, or, more exactly, at the moment when the edge of the umbral cone of Venus was just passing the centre of the Earth's disc as seen from the sun. Transit really began earlier, when the umbral cone first touched the Earth at A, the edge of the shadow-cone passing onwards so as to traverse the whole face of the Earth in about twenty-five minutes, twelve minutes of this time elapsing before the centre was reached and thirteen minutes afterwards.[2] It

[1] In some inexplicable way the unfitness of the suggested Antarctic stations seemed to depend chiefly on my own suggestion that, if fit, they should be employed for observations which had been officially, but erroneously, proclaimed impossible. The Antarctic stations were, in fact, singularly complaisant towards official chiefs, changing their character in respectful accordance with official requirements. An attempt was made to secure similar support from the Siberian, Chinese, and Japanese regions overlooked by Sir George Airy. They were pronounced to be geographically unsuitable. But foreign and American astronomers impaired this argument by occupying these regions and making excellent observations from them. After a significant struggle the Astronomer Royal even yielded about the North Indian region, where eventually useful observations were made during the transit of 1874. In fact, between foreign assistance and official concessions, my suggestions could hardly have been more fully carried out than they were if I had been in official authority at Greenwich.

[2] The difference was due to the circumstance that the shadow-cone crossed the southern half of the Earth more aslant than the northern half. The formulæ given in the *Nautical Almanacs* for 1874 and 1882 took no account of this as indeed is customary with such formulæ, intended only for a first approximation. Nor did they take into account the curvature of the shadow-cone's outline across the Earth's disc. In the large projection

is easy to take into account the effects of rotation (which, on the scale of the figures in Plate XI., are very slight), the meridians showing these being two hours apart. For any station where the transit began before the mean time a backward rotation for a corresponding number of minutes must be supposed; and a forward rotation where the transit began after the mean time. Similar remarks apply to the end of the transit of 1874, as stations had to be taken near A to give the earliest possible beginning of transit, and near B to give the latest possible beginning, C and B being the points near which observers had to be set for the most favourable application of Delisle's method to the end of the transit, as illustrated in the second figure of Plate XI. It is easy, by comparing the two figures, to recognise what stations were best suited for Halley's method. It will be observed that, had it been possible to occupy Possession Island (numbered 7) or Enderby Land, southern observations for shortened duration would have been made under much more favourable conditions than in 1882.

(620.) The third and fourth figures in Plate XI. are to be similarly interpreted. The low sun at both the southern stations recommended by Sir George Airy is shown by the proximity of each station 1 and 2 at the beginning, and of 1 at the end, to the edge of the disc, where the sun, of course, was actually on the horizon. It will be seen also that the accelerations and retardations were much less in 1882 than in 1874.

(621.) I refer those who wish for a fuller account of past and future transits of Venus than is here possible to my two treatises, (1) 'The Coming Transits,' in which I give a full abstract of all my calculations in regard to the transits of Venus in 1874 and 1882, with all the charts and diagrams presented to the Astronomical Society; and (2) 'Transits of Venus,' in which the history of all observed transits, the general theory of transits, and other matter appertaining to the subject, is fully and exactly, but simply, dealt with—the third edition carrying on the account to include the results of observations made during the transit of 1874.

(622.) As regards the controversy which followed my announcement that Sir George Airy's suggestions were unsatisfactory, I may remark that, apart from the corrections brought out by that controversy, which were, I think, important, the controversy probably served a more useful purpose still in reminding the public (as well as the persons concerned) of the true position of official astronomers, and, indeed, of all persons employed officially. Such persons are apt, especially when high in their respective departments of service, to forget that they are servants of and paid by the public, acting as though in authority over others than their proper subordinates. Nevertheless, though I felt entitled, both by my position as a taxpayer and by my knowledge of the subject, to call Sir George Airy's attention to the mistakes into which he had fallen, I should have thought it rather hard (though strictly just) to have directed public attention to the matter in the first instance. It was only when he told me in effect that he would not himself correct the mistakes he had made—all of them affecting scientific research, and some (as his proposal with regard to the transit of 1882) affecting life and property—that I deemed it my duty to ensure, by a

which I constructed and presented to the Astronomical Society such points were duly attended to; and taking into account rotation during the few minutes of acceleration or retardation, it was easy to determine the time of the beginning or end of transit, the sun's elevation at the time, and other elements for any station, in a couple of minutes, with a degree of approximation at least ten times closer than from the formulæ usually given, the application of which requires more time.

public discussion of the matter, the correction of what was seriously amiss. The case was very simple, seeing that he always admitted (though he allowed his subordinates to dispute) the accuracy of my calculations—employing them, indeed, in his reply to the Admiralty, as the most accurate available. Either he was responsible for England's part in observing the transits, in the sense of this falling within the duties for which he was paid, or he was not: if he was not, he could not complain if I called attention to the inaccuracy of his results; if he was, then, as a taxpayer, claiming to know the exact details of the subject he had treated inexactly, I was within my right (nay, I was discharging a public duty) in calling attention to the circumstance that he was not properly attending to the work.

(623.) So much I note because I have been put, very unwisely, on my defence in this matter, where I might have claimed thanks—which, indeed, I have received from the unofficial world in more than due degree. For the rest, I prefer to close the discussion of this subject by quoting the opinion of one whose independence of judgment and capacity for judging in a matter of this kind none can question—that, namely, of Sir Edmund Beckett, now Lord Grimthorpe, as expressed in 1873, when my views, long resisted officially, had finally prevailed:—

(624.) 'The controversy between Sir G. Airy and Mr. Proctor,' says Lord Grimthorpe, 'as to the best mode of observing the 1874 transit is as much a part of the history of astronomy as the proceedings in the discovery of Neptune, and the conditions of the problem cannot be understood without some account of it.'

'Sir G. Airy told the Royal Astronomical Society, in a paper of 1857, and still more strongly in another of December 1868, that Halley's method would be inapplicable to the 1874 transit, because it is the first of a pair, and so no advantage can be taken of the Earth's rotation,' as explained already. 'For the same reason he said it would be applicable in 1882, and suggested Sabrina Land as an Antarctic station for it, in another paper of June 1864. In 1868 he expressed some doubt whether that would be suitable in point of climate, but strongly urged that an exploring expedition should be sent to ascertain that point. Several Admiralty and other naval authorities at once supported him, and said there was no insuperable difficulty in such expeditions, and expressed full confidence in its being undertaken. Representations to the Admiralty were duly made, and it was understood that the expeditions were to go; but that the 1874 transit was to be observed by Delisle's method only, as the Astronomer Royal said Halley's was unsuitable.'

'But a month after the publication of that paper of 1868, viz. in March 1869, Mr. Proctor, then a young man much less known than now, astonished the Royal Astronomical Society by announcing that he had found the Astronomer Royal's conclusion, that Halley's method "fails totally for 1874" was itself totally erroneous; and that on the contrary that transit would be on the whole singularly well adapted for it, giving larger differences of duration than that of 1769, or any other of which the elements are yet known. He further said that sending Antarctic expeditions for Halleyian observations in 1882 would be a perfectly useless waste of money and risk of sailors' lives, because the sun will be too near the horizon for any reliable observations at the places proposed, and no other suitable ones would be accessible. M. Puiseux, of the French Board of Longitude, also disputed Sir G. Airy's conclusions as to 1874, but not so decidedly.'

'In a further paper Mr. Proctor gave the mathematical proof of his conclusion about the 1874 transit. It was questioned on one point by Mr. Stone, which

he answered, and his accuracy was no further questioned either by Mr. Stone or any other mathematician in England or elsewhere. No public notice, however, was taken of this striking contradiction, beyond dropping the projected Antarctic expeditions for the 1882 transit. As 1874 drew near Mr. Proctor began to write again upon the subject, but still without the least notice from Sir G. Airy, until it was proposed and carried in the Council of the Royal Astronomical Society to give the Society's medal to Mr. Proctor for this and his other labours. Then he wrote a letter deprecating the confirmation of the award, and depreciating Mr. Proctor's work as much as possible, but still not disputing its accuracy, nor explaining how it could be unimportant unless it was wrong. Nevertheless the award was confirmed by a larger majority of two to one, though not three to one, which the bylaws require.'[1]

'Shortly after this there were articles in the *Spectator* and the *Times* calling public attention to the risk the nation was running of a more discreditable miscarriage than that of 1769, by wilfully neglecting what every astronomer had considered the best mode of observation, provided it is applicable. Mr. Proctor afterwards avowed the authorship of the former of these, and the latter was guessed

[1] My name had been proposed by Sir Edmund Beckett, whom I then knew only by reputation and by sight. Had I been consulted (of course I could not be) I should have begged to be excused; though I doubt whether that would have made any difference, Sir Edmund being *tenax propositi* (*justus atque*). I had on several occasions expressed the opinion that the earnest student of science should need no such encouragement as medals may afford to the weaker sort, and that the system seems better suited to indicate approval of prize cattle than of scientific researches. Sir Edmund Beckett fully shared this opinion, by the way, as I afterwards learned.

I do not say that my objections might not have been strengthened by the feeling (which also Sir Edmund Beckett shared) that though my nomination might be carried for which a bare majority would suffice—it could not possibly be confirmed, where a majority of three to one was required, in a Council including not only Sir G. Airy himself, but three official astronomers, who may be described as past, present, and future chief assistants at Greenwich, besides several of the older astronomers, who had very clearly shown that the zeal of a non-official and much younger man was by no means to their taste. My researches in other matters than the transit, and especially my theories of the stellar universe, were included by Sir Edmund Beckett in his statement. I knew that the outside public would be likely to regard the decision of the Council as relating to these matters as well as to the transits, and that thus probably the general acceptance of the new views would be apt to be retarded. (This, indeed, would probably have happened had not a new star in the great Andromeda nebula, and the French photographs of the nebulæ in the Pleiades, very convincingly established my new theory of the architecture of the universe.) However, even Sir G. Airy's special and somewhat pathetic appeal to the Council, in which he distinctly intimated that he should regard their confirming the award of the medal to me as a vote of censure on himself, did not avail to prevent a majority of two to one voting in favour of the award. This was singularly significant.

I may take this opportunity of remarking that, having been led by what happened in January 1873 to pay somewhat closer attention than before to the question of medalling, I came to the conclusion, which recent events have strikingly confirmed, that the system is a bad one. Examining the awards during many past years, I note that in almost every case the purpose which the medal of the Royal Astronomical Society was intended to subserve - the encouragement of scientific research—has been neglected. Where the jealousies which exist among native astronomers have permitted an award to an Englishman, it has nearly always been made to one who, being either rich or in a high official position and already salaried for his work, has needed no encouragement, the results obtained by any individual worker not chancing to be wealthy being passed over, as naturally they fall short, in quantity of work accomplished, with much help either from paid workers or from official subordinates. It is, indeed, impossible that it should be otherwise where three-fourths of a body composed largely of salaried astronomers are required for an award which, like office itself in the Society, has been repeatedly used as a stepping-stone to salaried preferment.

I have been interested to find that Darwin long since expressed similar views about the medalling system. In fact, all men who value science for its own sake think thus.

by many persons to be mine. Neither of us had any idea that the other was writing at the same time. Thereupon the Admiralty did actually move so far as to ask Sir G. Airy for an answer to these articles; and he wrote one and read it to the Royal Astronomical Society in March 1873. He no longer maintained the inapplicability of Halley's method, but argued that it would be on the whole inferior to Delisle's; and took up an entirely new ground, of climatical objection to the best stations for astronomical results both in the extreme north and south, especially the proposed Siberian ones, which he did not believe Russia would undertake with the very small probability (as he assumed) of fine weather there in December. It turned out, however, that both the probability and the fact were the other way, and most valuable observations were got there. He quoted Dr. Oppolzer, of Berlin, for the not very novel remark that longitudes for Delisle's method can be found more accurately now than in 1761, when it practically failed.'

'Mr. Proctor immediately replied, with a table of numerical results for the combination of every pair of stations in the world which had been suggested, which showed a considerable superiority in Halley's method in several of them. Not that it really signified whether Delisle's were as good as Halley's, as he had never proposed to abandon Delisle's, but only not to abandon Halley's, or north and south observations by photography, using each at the best places for them. As to the climatical objection for the south, he merely cited the naval opinions which had been so promptly given in favour of Antarctic expeditions when Sir G. Airy wanted them. One of those naval authorities, indeed, the then Admiralty Hydrographer, wrote to the *Times* to say, in short, that he had changed his mind, and now agreed with the Astronomer Royal in condemning an Antarctic expedition, as he had agreed with him in 1868 in recommending it—a truly valuable opinion.'

'The Admiralty—i.e. Mr. Goschen, the First Lord thereof—thought they had done quite enough by consulting the Astronomer Royal as to whether the Astronomer Royal was right or wrong, and told Parliament that they meant to follow his advice, and have no Halleyian observations. They had been furnished with Mr. Proctor's reply; but it is not the nature of official people to regard such things—even to the extent of referring the matter to some independent authority, as had been urged in the newspapers.'

'But by this time astronomers in general had begun to be alarmed; and after some unpublished correspondence (which I had not heard of when I wrote a summary of these proceedings in the *Times* of Jan. 2, 1874, in a letter with my name—none of the later articles were mine or Mr. Proctor's, except letters with his name) a resolution was moved by Professor Adams and unanimously carried at the Greenwich Visitation in June 1873, that an "application be made to the Government for the means of organising parties of observers in the Southern Ocean with a view to finding additional localities for observing the whole duration of the transit," i.e. for Halleyian observations.' [The application was made by the President of the Royal Astronomical Society as chairman of the Board of Visitors.] 'This was rather too weighty a communication for the Admiralty to extinguish with the dictum of their Astronomer Royal, and they agreed to send the required expedition, notwithstanding their Hydrographer's apprehensions, which, it is gratifying to find, were not realised.'

'Halleyian observations of the transit were also at last settled to be made at several places both by us and other nations. Indeed, other nations, and notably America, had rejected Sir G. Airy's views for Mr. Proctor's long before. Mr. Proctor publicly

expressed his satisfaction at this result, and also that some Indian observations were to be made, which he proposed and the Astronomer Royal had expressly rejected, both in his letter to the Admiralty and before. Nevertheless, in a letter to the Council of the Royal Astronomical Society Sir George Airy denied that any alteration had been made in his plans: which statement, with the whole matter, I leave to the reader's reflections, as I did the history of Neptune.'[1]

(625.) The next transits, as already mentioned, will be those of June 8, 2004, and June 6, 2012, in the preparations for which astronomers now living are not likely to take any important share. The following are the elements of these transits as given by Mr. Hind, the superintendent of the 'Nautical Almanac'; they can easily be carried to a much closer degree of approximation by applying the processes indicated in my 'Coming Transits' and 'Transits of Venus': –[2]

[1] Sir G. Airy's self-contradictions were remarkable; but they were not to be compared for wildness with the amazing and amusing contradictions of himself and of each other perpetrated by various official subordinates anxious to distinguish themselves by zeal in his defence. Here are a few of the contradictory statements which appeared in *Nature*, the *Academy*, the *Saturday Review*, the *Naval Magazine*, the *Times*, and elsewhere in 1874: –

(1) Sir G. Airy's paper of 1868 did not claim exactness; (2) it was the only paper which considered every detail; (3) Halley's method was out of date; (4) Airy rightly regarded it as most valuable for 1882; (5) he rightly said it 'failed totally' in 1874; (6) he had provided for its use in 1874; (7) it was to be applied but not much trusted; (8) it was not to be applied at all (the beginning and end, but not the duration, being timed!); (9) other nations would not apply it; (10) since other nations were to apply it, England need not; (11) though other nations were to apply it, they put little trust in it; (12) I was wrong in proposing North Indian stations; (13) especially for Delisle's method; (14) North India was to be occupied for the photographic method; (15) North India being occupied for Delisle's method showed that method was preferred to Halley's; (16) it was correct in 1868 to propose Antarctic expeditions; (17) and proper to ridicule them in 1873; (18) Sir G. Airy and the Admiralty people were right in proposing the Crozets for 1882; (19) and equally right in ridiculing my suggestions that they should be occupied in 1874; (20) it was at Sir G. Airy's suggestion the Americans proposed to occupy the Crozets in 1874; (21) America was wrong in declining to occupy Tahiti at his suggestion; (22) he never suggested that the Americans should occupy any station; (23) he successfully suggested Marquesas as a Delislean station for the French; (24) the French had not satisfied Sir G. Airy's wishes, as they declined to occupy Marquesas; (25) the Government Board of Visitors really agreed with Sir G. Airy; (26) being an irresponsible body their differing from him mattered little; (27) I began the discussion too early, in 1869, when no plans had been formed [Sir G. Airy in a letter to myself]; (28) the plans formed in 1868 were complete and not to be changed [the present Astronomer Royal in the *Academy*]; (29) I resumed the discussion too late in 1873, when everything was settled [Admiral Richards in the *Times*]; (30) I was mistaken in expressing anxiety in 1873; (31) and still more in being content in 1874; (32) longitude can be determined with half the error of a contact observation [Sir G. Airy, March 1869]; (33) since with this criterion Halley's method still showed superior, longitude can be determined with only a fourth the error of a contact observation [Sir G. Airy, May 1869]; (34) observations are more exact now than in 1769, and longitudes can be much better determined; (35) but contact observations cannot be determined at all better now than in 1769; (36) Greenwich experiments to improve contact observations were very valuable; (37) I kept a 'storm raging' [*Saturday Review*] in 1873, when it was too late to do any good; (38) I was silent in 1874 [again the *Saturday Review*!], showing I was in the wrong; (39) the Astronomer Royal had more important duties than attending to Venus; (40) it was no one's business but his to attend to her transits; (41) my corrections implied serious charges against Sir G. Airy; (42) they were of no real importance. The case was, in fine, a largely developed version of the old story—'The vase was cracked when we borrowed it, whole when we returned it, and we never had it.'

The illustration of official ways afforded by the whole matter was interesting and instructive.

[2] M. Leverrier's tables of the sun and Venus represent so closely the motions of the Earth and Venus in their orbits, that there can be little reason to doubt that the tables will be as sensibly perfect in 2004 and 2012 as they are at the present time.

TRANSIT OF 2004.

Greenwich Mean Time of Conjunction in Right Ascension = June $8^d\ 8^h\ 51^m\ 28^s{\cdot}8$.

For the centre of the Earth.

		d.	h.	m.	s.	
First external contact	. . . .	June 8	5	3	43	at 115·0
First internal contact	. . . .	,,	5	22	35	,, 118·0
Second internal contact	. . . .	,,	11	5	40	,, 214·6
Second external contact	. . . .	,,	11	24	32	,, 218·5

The angles are reckoned from N. towards E. for the direct image.
At Greenwich the entire transit will be visible.

TRANSIT OF 2012.

Greenwich Mean Time of Conjunction in Right Ascension = June $6^d\ 1^h\ 4^m\ 41^s{\cdot}3$.

For the centre of the Earth.

		d.	h.	m.	s.	
First external contact	. . . .	June 5	10	22	11	at 40·3
First internal contact	. . . .	,,	10	39	56	,, 37·8
Second internal contact	. . . .	,, 6	4	42	6	,, 293·1
Second external contact	. . . .	,,	5	0	0	,, 290·5

At Greenwich the egress only will be visible, the sun rising at $3^h\ 46^m$.

(626.) The regions of the Earth where (1) the ingress only, (2) the whole transit, (3) the egress only, and (4) no part of the transit can be observed, are indicated for the transit of 2004 in Plate VIII. and for the transit of 2012 in Plate IX. of my 'Transits of Venus,' in which maps also the four Delislean poles (or points of greatest acceleration and retardation) of ingress and egress, and the two Halleyan poles (or points of longest and shortest duration[1]) are indicated for both transits.

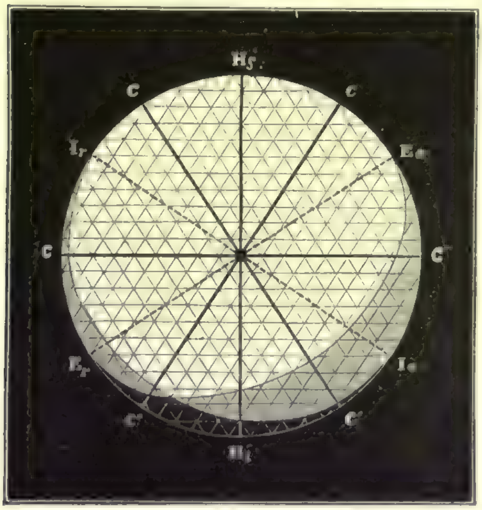


FIG. 184.—The Halleyan Poles and Circles of equal duration of the Transit of 1761 and 2004.

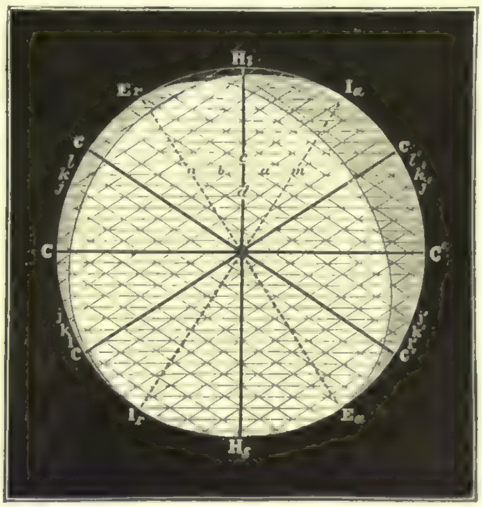


FIG. 185. The Halleyan Poles and Circles of equal duration in the Transit of 1874.

[1] These points, as determined theoretically, are never both within the part of the Earth where the transit is visible. There is a neat way of determining their position and the position of places, where the duration of transit has a given length, on the principles illustrated in figs. 184 and 185. In each the points I_a and I_r are those where ingress is most accelerated and most retarded respectively, or the Delislean poles for ingress; while E_a and E_r are those where egress

(627.) Observations both by Delisle's and Halley's methods require the exact determination of the moments when Venus's disc is just wholly upon the disc of the sun, touching its edge internally—called the moments of *internal contact.* Unfortunately optical difficulties interfere to render these time determinations somewhat difficult. Instead of appearing at the time of internal contact with neatly defined disc just touching the disc of the sun, Venus presents such forms as are shown in fig. 186, a ligament[1] of greater or less breadth connecting Venus with the sun's edge. Often the disc of Venus is greatly distorted, especially when the sun is low at the time of internal contact. The ligament itself is readily explained, though the effects of the phenomenon in rendering time observations doubtful cannot be readily corrected. If the linear arcs in figs. 187 and 188 represent the true outlines of the sun and Venus, the combined effects of irradiation, diffraction, spherical aberration, and chromatic dispersion will tend to extend the disc of the sun, and to encroach upon the disc of Venus, to greater or less extent, say to some such distance as is indicated in these figures. Hence at ingress before true internal contact the cusps at S′ and V′ will be rounded, and at the actual moment of true internal contact, the body of Venus will seem to be thrown some distance upon the sun's disc, except at the actual point of contact, where a ligament connecting the disc of Venus with the edge of the sun will

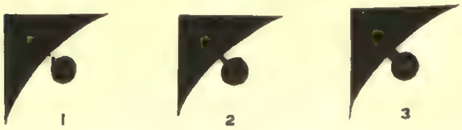


FIG. 186.—Illustrating the Black Drop. Three views.

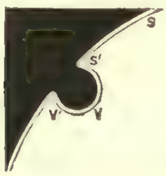


FIG. 187. Explaining the formation of the Black Drop.

FIG. 188.

is most accelerated and retarded respectively, or the Delislean poles for egress. Neglecting the effects of rotation during ingress and egress as slight, the points I_a and I_r lie at the extremities of a diameter of the Earth, as do likewise the points E_a and E_r. If these points are brought to the edge of the disc, as in figs. 184 and 185, the arcs along which acceleration or retardation are equal appear as the series of equidistant parallels to $c\,c'$ and $c\,c'$ in both figures. Now if we take the intersection of one of these lines indicating a given acceleration of ingress, and another indicating a given retardation of egress, we obtain a point where there is a certain lengthening of duration; if we pass to the intersection of a parallel indicating a certain amount less or more of acceleration with one indicating a certain amount more or less (respectively) of retardation, we get a point indicating the same duration as before; whence it follows that the series of equidistant thwart-lines parallel to C C′ in the two figures are lines of equal lengthening or shortening; H_l and H_s the extremities of a diameter perpendicular to C C′, are thus the points respectively of greatest lengthening and shortening, or the Halleyan poles. The shaded parts of the disc in each figure indicates the region where the transit is partially but not wholly seen, and the black part the region where the transit is wholly invisible; it will readily be seen how the shaded parts are obtained in any given case, and that one or the other Halleyan pole must lie on the black part of the disc. But it will be noticed that in fig. 184, which illustrates the conditions of such transits as those of 1761 and 2004, one Halleyan pole is much farther within the dark region than in fig. 185, which illustrates the transit of 1874. The student will find that the difference depends on the arc distance separating the points I_a and E_r, and the points I_r and E_a. The smaller this distance the better (*cæteris paribus*) are the Halleyan conditions. It will be readily seen by comparing figs. 181, 182, and 183, with figs. 184 and 185 (taking into account in considering the latter the effects of rotation in affecting the real positions of the points *i*, *i′*, *e*, and *e′*), that while the shortness of a transit's duration helps to bring the point I_a (*i* of figs. 181, 182 and 183) near to E_a, and I_r near to E_a, these points are always brought nearer to each other in the second than in the first transit of a pair.

[1] This appearance has been called the 'black drop'—apparently because it is seldom black and never like a drop. Certainly some students of science have strange ways in regard to nomenclature.

be seen, as shown in fig. 188. If the telescope has considerable optical defects, or the observer's vision is defective in regard to definition, the ligament will not be seen so fully drawn out as in fig. 188, but forms such as are shown in fig. 186 will be observed. When the atmosphere is disturbed, as it always is near the horizon, further distortion will be produced. Photographic records of ingress and egress, taken in closely succeeding series, may serve to give better results than can be obtained from direct observation; but in 1874 and 1882 no great success was obtained in this way.

(628.) It is probable that hereafter the direct method, by which Venus is observed when well on the sun's face, may be preferred to any of the methods depending on time-determinations of internal contact. It is obvious that, for the direct method, Venus should be observed at northern and southern stations, when near the middle of her chord of transit. Fig. 189 illustrates the principles on which stations should be selected. It shows the face of the Earth turned sunwards, at $5^h\ 8^m$ December 6, the time of the middle of the transit of 1882. The points M and N are those where Venus was most displaced north and south of her mean transit chord, being seen in her true place from the point O only at this moment. The numbers along the line M O N show by how many tenths of this maximum displacement Venus was displaced at the points so numbered, and along the several parallels through these points. The concentric circles round O indicate the sun's elevation. The best stations for observing Venus well displaced to the southwards were obviously those in the eastern parts of the United States, the Bermudas, and West Indian Islands. In the southern hemisphere there were not such convenient stations for observing Venus well displaced towards the north, the best being the Falkland Islands and Tierra del Fuego.

FIG. 189.—Illustrating the observation of Venus in mid-transit.

(629). The suitability of photography for mid-transit observations will be obvious; for if by some trustworthy method of instantaneous solar photography well-defined photographs of Venus in mid-transit can be obtained at suitable northern and southern transits, the sun's distance can be directly determined from careful microscopic measurements of such photographs. The chief difficulty resides in possible changes affecting the position of parts of the photographic film. But this difficulty can no doubt be overcome. Excellent service might have been done for science had official astronomers, in anticipation of the transits of 1874 and 1882, conducted a series of experiments on various processes available for the production of trustworthy photographs of the sun with Venus in mid-transit. Probably the best method for this special purpose would be one which would differ widely from any ordinarily in use. For instance, it might be that a metallic surface, prepared by some chemical process to receive rapidly (but

not too rapidly) and retain luminous impressions, would be better than any process, wet or dry, in which a prepared film or coating is the recording surface.

(630.) Before leaving the subject of mid-transit observations I must describe here the heliometric method of observation, as it is probably the best method of direct measurement available, not only for mid-transit observations, but for observations of Mars and certain among the asteroids for the determination of the sun's distance.

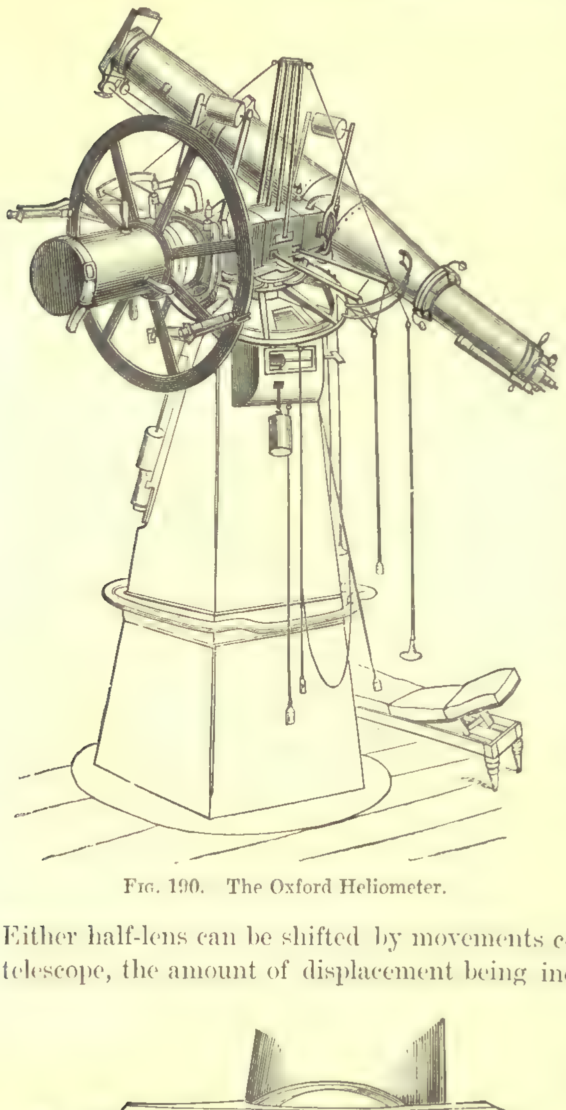


Fig. 190. The Oxford Heliometer.

(631.) The *heliometer*, so called because at first devised and used for measuring the sun, is also sometimes called the 'Divided object-glass micrometer.' Fig. 190 represents the heliometer of the Radcliffe Observatory at Oxford. It will be seen that it is an equatorial instrument, but it differs from an ordinary equatorial in having a divided object-glass, shown in fig. 191, the halves of which, E and F, can be shifted, the diameters of the two semicircular sections sliding along each other by the movement of the two plates A A′ and B B′ (themselves curved so that the parts of the lens as they are shifted retain their distance from the focus of the eye-lens unchanged; in other words, the radius of curvature for the plates is equal to the focal length of the object-glass. Either half-lens can be shifted by movements communicated from the eye-end of the telescope, the amount of displacement being indicated by the divided scales at C and D, combined with the movements of the micrometrically adjustable screw-heads G G′ and H H′ (the use of which is explained in Art. 139). There is also an adjustment, workable from the eye-end, by use of the Hooke's Joints shown in fig. 190, for bringing the diametral division of the object-glass into any desired position.

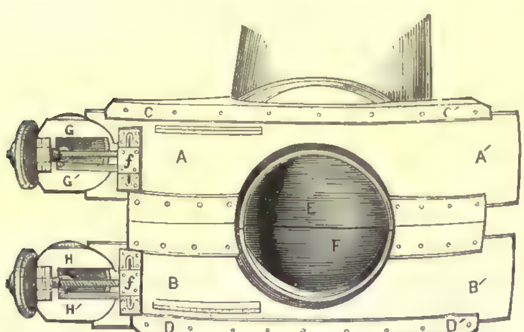


Fig. 191.—The divided Object-glass of the Oxford Heliometer enlarged.

(632.) When the two halves of the lens are as in fig. 191, they form a single object-glass, and only one image of an object to which the telescope is directed is seen. But when the centre of one half slides (along the common diametral edge) away from the centre of the other, each half forms an image in a

different position; the halves may be regarded, in fact, as the object-glasses of differently directed telescopes. Either forms a complete image of the object observed, this image being precisely equal in scale and illumination to the image formed by the other half, and separated from it by an angular displacement equal to the angle between the optical axes of the two semi-lenses, this displacement being made in any desired direction by suitably adjusting the object-glass holder A A′ B′B.

(633.) The use of this instrument may best be illustrated, in the first instance, by supposing it directed on a field of view in which are two stars (S and *s*, fig. 192), whose distance and relative position are to be determined. With the half-lenses together the two stars are in view in their correct positions. Now let the holder A A′ B′B be so adjusted that, when either half-lens is shifted, the image of each star will move along the line S *s*, and then let one lens be shifted in the direction thus determined in such sort that the star *s* is brought into exact coincidence with the star S, as shown by the unchanged half-lens; and let the amount of change as indicated by the scale and micrometer screw-heads be carefully noted. Next let the centre of the semi-lens thus moved be carried back, to and past coincidence with the centre of the other, until the star S is brought into exact coincidence with the star *s*, as shown by the unchanged lens, and let the micrometer indications be again read. Bringing this semi-lens again into coincidence with the other, let it remain in that position, while the semi-lens which had remained fixed is shifted, first on one side then on the other, in precisely the same way as the other had been. The mean of the four readings thus obtained indicates the distance between the stars; while the first adjustment, which in reality is tested by these four adjustments for distance, indicates the direction of the line S *s*. Observations can generally be made on both sides of the polar axis; and the holder of the object-glass should admit of being turned completely round, so that the desired direction of the sliding diameters of the object-glass may be obtained in two opposite ways. Thus there will usually be four ways of adjusting for direction, and errors due to flexure of the telescope and of the declination axis may thus be partially eliminated. Where the distance between two stars, or other observed points, is alone to be determined, the corrections for flexure are not important.

Fig. 192. Illustrating the measurement of Star-distances with the Heliometer.

(634.) The application of this instrument to the observations of Venus in mid-transit will be readily understood. Having Venus on the sun's face viewed singly, such adjustment must be made that by sliding either semi-lens the disc of Venus will be moved towards the sun's centre. Then one semi-lens is to be shifted until Venus is brought into apparent contact, first internal and then external, with the nearest part of the sun's edge, and brought back again. The same operation is then to be performed with the other semi-lens. The mean of the four displacements thus observed is the distance of the centre of Venus from the edge of the sun. If it is thought desirable, the disc of Venus may be carried in like manner past the sun's centre to internal and external contact with the farthest part of the sun's edge; and the mean of the four observations

thus made by sliding the two semi-lenses may be taken as giving the distance of the centre of Venus from that farther edge. The distance of Venus from the sun's centre follows, of course, from either set of observations, since the sun's apparent diameter may further be measured in the usual way with the heliometer, by carrying one image into contact, first on one side, then on the other, with the unchanged image, repeating the observation with both semi-lenses: the resulting determination of the sun's semi-diameter, compared with the known value of that diameter at the time of observation, will indicate the trustworthiness of the readings generally, and therefore the probable value of the measurements of Venus's distance from the sun's centre.

(635.) Observations of the transit of Venus in 1761 and 1769 were satisfactory with reference to the astronomy of the time, but would now be thought rough, precisely as the observations during the transits of 1874 and 1882 will doubtless be considered when the next pair of transits occur. The observations of 1769, when Halley's method was employed, were much more valuable than those of 1761. The distance of the sun, as inferred from the observations of the transit of 1761, ranged in value from about 76,000,000 to about 96,000,000 miles; those deduced from the observations of the transit of 1769 lay between the limits 92,000,000 and 96,000,000, as originally interpreted. Encke, in 1824, published the results of a laborious analysis of the observations made during the transit of 1769, from which he deduced 95,274,000 miles as the most probable distance of the sun: or, more precisely, he deduced $8''\cdot5776$ as the most probable value of the sun's mean equatorial horizontal parallax, whence with the estimate of the Earth's dimensions and of the sun's apparent diameter then in vogue a distance of 95,365,000 miles was inferred, corrected later to 95,274,000 miles by the use of improved estimates of the elements just mentioned. This value for the sun's distance, with corresponding values for the distances, diameters, masses, &c., of the various members of the solar system, maintained its position in treatises on astronomy till about the year 1850, at which time Hansen announced that the lunar parallactic inequality indicated a distance less by about four millions of miles.[1] Then it was found that Encke had much overestimated the value of the observations made in 1769. Powalky, in 1864, interpreted the observations so as to give a distance of about 92,000,000 miles. Mr. Stone, then first assistant at Greenwich, showed that by treating in one particular way the probable errors arising from the 'black drop' phenomenon a distance of about 91,840,000 miles could be deduced from these observations. Professor Newcomb, of the Washington Observatory, showed that the observations

[1] I used Hansen's value of the sun's distance, as then the most probable, in my first book, *Saturn and its System*, published in May 1865; but I think Sir Edmund Beckett's *Astronomy without Mathematics*, published in the same year, was as early, if not earlier, in the rejection of Encke's value of the sun's distance. It soon appeared, however, that Hansen's estimate was below the true value.

might be interpreted so as to give results different both from Mr. Stone's and from Encke's. In fact, it was shown, directly and indirectly, that the observations made during the transit of 1769 were altogether unsatisfactory. Certainly they indicated a solar distance somewhere between 90,000,000 miles and 97,000,000; but within these rather wide limits the sun's distance might be any whatever, so far as those observations were concerned.

(636.) The results deduced from observations made in 1874 and 1882 were, of course, much more trustworthy, yet they were not so satisfactory as had been expected. In fact, so much disappointment was felt after the observations of the earlier transit had been analysed, that Russia declined to take any part in observing the transit of 1882, alleging that observations of Mars and the asteroids would supply better materials for determining the sun's distance, and at much less expense. But probably the unfavourable conditions of the transit of 1882 had as much to do with this decision as either the question of expense or any general objection to the transit method of determining the solar parallax.

(637.) From a careful study of the British observations of the transit of 1874, Mr. Stone deduced a solar parallax of $8''\cdot88$, corresponding to a distance of 92,044,000 miles. Colonel Tupman, dealing with them differently, deduced a parallax of $8''\cdot81$, corresponding to a distance of 92,780,000 miles. Sir George Airy deduced a parallax of $8''\cdot754$, implying a distance of 93,375,700 miles, and, *more suo*, regarded this result as superior to all others: 'I do not think,' he said, 'there will be anything to compete with the value thus deduced.' He did not dwell much on the significant circumstance that the Delislean observations of ingress, considered alone, gave for the sun's distance 93,550,000 miles, while those of egress gave a distance of 92,395,000, or 1,155,000 miles less; but his accepting as satisfactory a distance nearly a million miles greater than resulted from one set of Delislean observations, and only 175,000 miles less than resulted from the other set, showed how small was the real reliance he placed on the method he had once regarded as the only one available. He evidently felt that the Halleyan observations in 1874 were those which could best be trusted. The French observations of contact (they observed in Japan and Pekin for lengthened duration, and in St. Paul's Island for shortened duration) indicated a parallax of $8''\cdot88$, or a distance of 92,044,000 miles. The photographs obtained by the astronomers belonging to observing parties sent out by European Governments failed totally, the method employed being unsuitable; but from the American photographic work and from that done by Lord Lindsay's party at Mauritius (who employed the same method—using telescopes of great focal length) excellent results were obtained. Mr. Todd, of the American

'Nautical Almanac,' deduced from them a solar parallax of 8″·883 with a probable error of only 0″·034, corresponding to a distance of 92,011,000 miles.

(638.) It will be seen from these results, which must be regarded merely as samples of the deductions from the transit of 1874, that Professor Harkness, of Washington, was justified in asserting that the range of uncertainty in regard to the sun's distance, so far as the observations of 1874 were concerned, amounted to nearly 1,600,000 miles. It may be observed, however, that while the mean of the various distances inferred amounted to about 92,500,000, none 'competed' [in magnitude] 'with' Sir George Airy's distance, 93,375,700 miles.

(639.) The results of the observations in 1882 were even less satisfactory, but doubtless that was due rather to the unfavourable conditions under which the later transit of the nineteenth-century pair took place—except, perhaps, for the mid-transit and photographic methods. American astronomers took many excellent photographs, and the Germans observed mid-transit ably with the heliometer. But the range of uncertainty as to the sun's distance was not reduced by the observations of the transit of 1882 below a million miles.

(640.) On the whole the results of efforts to determine the sun's distance by observations of Venus in transit have been such as to lead astronomers to turn to the Earth's neighbour planet on the other side—Mars—as likely to give more satisfactory information.

(641.) It will be seen from Plate IX. that, although the exceptionally eccentric orbit of Mars is much farther near M′ (its aphelion) from the Earth's orbit than is the orbit of Venus, there is little to choose between the distances of the two orbits when Mars is near perihelion at M at the time when he and the Earth are in conjunction. Such difference as there is, is more than compensated, so far as parallactic displacement is concerned, by the circumstance that, whereas in the case of Venus on the sun's face we see only a portion of the displacement, in the case of Mars on the background of the star-sphere we get the full displacement.

(642.) Let us compare the parallax of Mars when in opposition near perihelion with the parallactic displacement of Venus as seen on the sun's face. When Venus is in transit her distance from the Earth is 28, the Earth's distance from the sun being taken as 100. When Mars is in opposition near perihelion his distance from the Earth is 38. Hence the actual parallax of Venus is greater than that of Mars as 38 is greater than 28, or 19 than 14. But on the sun's face we see only $\frac{18}{25}$ of the parallactic displacement of Venus (Art. 598, *note*): hence the observable parallax of Venus is $\frac{18}{25}$ of $\frac{19}{14}$ or $\frac{171}{175}$ of that of Mars.[1]

[1] The mean horizontal (equatorial) parallax of the sun is taken throughout this volume as 8″·81; this would make the parallax of Venus in inferior conjunction 8″·81 × 100 ÷ 28, or 31″·48, and that of Mars in opposition near perihelion 8″·81 × 100 ÷ 38, or 23″·18. The effective paral-

(643.) It is evident then that, since the determination of Venus's position on the sun's face is certainly not easier or likely to be more exact than the determination of the position of Mars on the star-sphere, it has been only the trust in contact observations by the methods of Halley and Delisle which has led astronomers to expect more from Venus than from Mars. The superior advantages offered by Mars, so far as determinations of position are concerned, will be still more clearly seen if we consider that the oppositions of Mars near perihelion recur at intervals alternating between fifteen and seventeen years, whereas transits of Venus occur only in pairs, separated by more than a century. Moreover, whereas Venus is on the sun's face for only a few hours, and for only a small part of that time favourably placed for the determination of her position, Mars is favourably situated for observation at one of the oppositions in question for several weeks.

(644.) But there is another circumstance which in yet greater degree renders the observation of Mars for the determination of parallax more convenient, as well as more trustworthy, than observations of Venus in transit. Let us suppose fig. 193 to represent the night side of the Earth. [Instead of the day of the month actually marked in the figure (which was intended for another purpose), we should have to substitute August 22 (six months from February), a time corresponding with that of one of the favourable oppositions we are considering.] Now it is obvious that were observers set in Siberia as presented in fig. 193, that is, as turned towards Mars at midnight, in an August opposition, and other observers at St. Paul's Island and other islands near the Antarctic regions south of the Indian Ocean, Mars would be thrown southwards on the star-sphere as seen from the northern stations and northwards as seen from the southern stations, precisely as we have seen (Art. 598) that Venus correspondingly observed when in transit would be displaced on the sun's face. But it is also obvious that the occupation of suitable southern stations would involve considerable expense and inconvenience. The observations made under such different conditions as would exist near midnight at northern and southern stations in August (one set of observers enjoying the heat of summer, while the others would be exposed to the cold of an Antarctic winter) would be unsatisfactory from that cause alone; and being made by different observers, 'personal equation' would be apt to affect the results seriously, being variable, and probably irregularly variable, when observers are exposed to unusual climatic vicissitudes. But it is not at all necessary that observers should be set near the extremities of a north-and-south diameter of the Earth. They might make observations for parallax quite as effectually if set at points along any other diameter of the disc shown in fig. 193. And clearly, if the diameter selected be along the apparent direction of the Earth's equator in this figure, the scientific nations need not be at the pains to send observers to suitable

Fig. 193. Illustrating the Diurnal method of observing Mars for parallax.

lax of Venus as observed in transit over the sun's face is 8″·81 × 72 ÷ 28, or 22″·67. The actual maximum displacements for the whole Earth have, of course, twice these values, the horizontal parallax of an object being the angle subtended by the radius of the Earth as seen from that object, not by the diameter.

stations widely apart along some equatorial zone of the Earth. For the Earth's rotation will in nine or ten hours carry an observer from near the western side of the face (as in fig. 193) turned towards Mars to near the eastern edge. If he makes a set of carefully-timed observations on Mars soon after Mars has risen (which when Mars is in opposition is soon after sunset), and another such set shortly before Mars sets (or shortly before sunrise), taking duly into account, when interpreting his observations, the motion of Mars during the interval, that single observer will have results as trustworthy for comparison as a pair taken at the same instant by two observers set many thousand miles apart one in a high northern the other in a high southern latitude. Indeed, his results will be much more trustworthy, being made by one observer under conditions probably not very dissimilar at the western and eastern or evening and morning stations to which he has been carried by the Earth's rotation.

(645.) It is hardly necessary to explain the use of the heliometer in determining the position of Mars with respect to the star-field on which he is seen when under observation for the determination of his parallax—whether as observed from two distant stations, or from the same station as carried to different positions by terrestrial rotation. Some points, however, have to be noticed. By the method described in Art. 633 one image of Mars can be shifted from coincidence with the other in such sort that the centre of the moving image is brought into coincidence with a star whose distance is to be determined. But manifestly there would be room for error in doing this, because the position of the centre of the disc of Mars is not defined by any mark. To obviate this the disc of Mars must be carried centrally towards the star until the outline of the planet just touches the star, the instrument's reading taken, and then the disc of Mars must be carried farther on till the centre has passed and the outline again touches the star, the reading of the instrument being again taken. The mean of the two readings thus taken is the distance of the star from the centre of Mars, the difference being a determination of the planet's diameter. The moving image must next be carried back to and past the fixed image till the star touches the outline of the fixed image, first on one side, the reading being taken, and then on the other, this reading being taken. We thus obtain two more readings, whose mean is the distance required, and their difference the planet's diameter. Having brought the moving image back to coincidence with the other, a similar series of observations must be made by moving the other half of the object-glass, the image which had moved before being now fixed, and the fixed image moving. We thus have eight observations, the mean of which is the distance we require. Every pair gives the planet's diameter, which, though not required for the determination of parallax, is useful as serving to measure the accuracy of the observations; for of course the four determinations of the diameter should be equal to each other, and also to the diameter as calculated from the planet's real diameter (deduced from tens of thousands of former observations for that special purpose) and known distance.

(646.) The method just described is not so exact for position as for distance, because the planet's estimated centre is carried over the star, or the star carried over the planet's estimated centre, and this motion cannot define the direction of the line joining the star and actual centre of the planet's disc with perfect accuracy. But the distance is all in reality which we require in applying this method. Several stars near Mars are dealt with, and the measured distances of two or more stars from the centre of Mars determine the relative positions of the stars with respect to the planet. But also, by properly selecting the stars observed, we may eliminate the effect of posi-

tion, just as in observing a transit of Venus heliometrically we can render the exact direction of the line joining the centres of the sun and Venus a matter of small importance by choosing the time when Venus is near the middle of her chord of transit, at which time her distance from the sun's centre changes very slowly.

(647.) A difficulty of more importance arises from the circumstance that the edge of Mars is affected by irradiation, diffraction, and such optical defects as affect the work of even the best telescope. These are much slighter than in the case of the sun, Mars being so much less brilliant; moreover, by the method of double observations described, the errors arising from this cause are in great part corrected, their effects falling chiefly on the determination of the planet's diameter. Still there is less precision in the observations than in the determination of the distances separating star from star in the heliometer's field; though probably observations of Mars, even under these conditions, are not only more but many times more trustworthy than heliometric observations of Venus in transit.

(648.) The difficulty just considered led to the suggestion, first made, I believe, by Dr. Galle, of Berlin, that astronomers should look beyond Mars to the asteroids for the best means of directly determining the scale of the solar system. The asteroids, as their name (which signifies 'star-like') implies, present discs so small as scarcely to differ (practically) from the discs of stars. Some of them travel on paths which, though their mean distance may be much greater than that of Mars, yet, owing to their eccentricity, bring them in parts of their circuit within distances little exceeding his. Their parallax, when so situated, is little less than that of Mars. Some inferiority in this respect is probably much more than compensated, first, by the greater accuracy with which an asteroid's position with reference to the fixed stars in the same field may be determined; and secondly, by the more frequent occurrence of favourable opportunities, some one or other of the large family forming the asteroidal ring being tolerably sure to come into favourable position in the course of three or four years, if not yearly. What has been said in dealing with Mars with regard to two methods of observation for determining parallax applies to the asteroids also; the two methods may be called the meridional and the diurnal.

(649.) Observations of Mars and the asteroids for the measurement of the sun's distance have led to a number of very promising determinations. It is worthy of notice that the first really trustworthy estimates of the sun's distance were obtained from observations of Mars. Even before the use of the telescope, Kepler, from his study of Tycho Brahé's observations of Mars, stated confidently that the sun's parallax is certainly not greater than 1′—that is, the sun's distance not less than $13\frac{1}{2}$ millions of miles. This estimate necessarily depended on the diurnal method, since Kepler studied observations made at a single station. Flamsteed, by the same method—applied, of course,

under much more favourable conditions—deduced a solar parallax of 10″ from the Greenwich observations of the planet: this corresponds with a solar distance of 81,700,000 miles. From the observations of Cassini, Römer, and Picard, at several stations in France, combined with those of Richer at Cayenne, Cassini inferred a Martian parallax of 25″; or rather he set this as the superior limit, a result indicating a solar parallax not exceeding 10″. A similar result was obtained from the combination of observations made by Lacaille at the Cape of Good Hope with those made by several astronomers at European stations. Passing over other rough approximations, we do not find observations of Mars dealt with zealously for the determination of the solar parallax until the parallax deduced by Encke from last century's transits began to be seriously questioned. In 1862 Mr. Stone, then of Greenwich, now Radcliffe Observer at Oxford, deduced the solar parallax (1) from observations of Mars at Greenwich alone, (2) from observations made at Greenwich and Cape Town, and (3) from observations at Greenwich and Williamstown, deducing from the combined results a solar parallax of 8″·943 with a probable error of 0″·051, corresponding to a solar distance of 91,400,000 miles with a probable error of 500,000 miles. Winnecke, by combining observations of Mars made at Poulkowa and Capetown, deduced a parallax of 8″·964, corresponding to a distance of about 91,200,000 miles. Newcomb, by the diurnal method, deduced a parallax of 8″·855, corresponding to a distance of about 92,300,000 miles, which is not far from the value adopted throughout his 'Popular Astronomy.'[1]

(650.) It was not, however, till 1877, a time when Mars was in opposition under very favourable conditions, that a definite attack was made on the problem of the solar parallax by the Martian diurnal method. The estimates deduced before that time had been based on observations not specially directed to this problem. In that year Mr. Gill, afterwards Government Astronomer at the Capetown Observatory, went to Ascension Island for the purpose of applying the method which Flamsteed had advocated two centuries before. From his heliometric observations he deduced a solar parallax of 8″·78, corresponding to a solar distance of 93,090,000 miles.

[1] There is a rather suspicious tendency in all the approximations to the solar parallax made near any given time to range themselves around that value of the parallax which at the time is regarded as most probable. Thus in the first half of the present century the sun's distance came out as about 95,000,000 miles in most of the determinations deduced from observations of Mars (they were not many, however). In the period following Hansen's evaluation of the 'lunar parallactic inequality,' which led by a necessarily independent process to a solar distance of between 91,000,000 and 92,000,000 miles, all the results obtained by European astronomers by the method depending on observations of Mars ranged between these values. In America, exception was taken to certain European determinations of the solar parallax, and there alone—at that season—solar distances exceeding 92,000,000 were obtained.

This may be regarded as probably the best estimate by this method which has yet been made.

(651.) The asteroidal method was applied meridionally in 1872 to Phocœa, and in 1873 more satisfactorily to Flora, the deduced parallax being 8″·875, corresponding to a solar distance of 92,095,000 miles. At Mauritius, in 1874, the party sent out by Lord Lindsay to observe the transit of Venus employed a part of their time in the observation of Juno for parallax by the diurnal method; but that asteroid can hardly be regarded as well suited for observations of the kind, and less reliance can be placed on the results deduced than Lord Lindsay anticipated. (See Vol. xxxiv. of the 'Notices' of the Astronomical Society.)

(652.) The method of determining the sun's distance based on the 'lunar parallactic inequality' has already been considered (Art. 495). There is another, depending on the moon's orbital motion, or rather on the circling of the Earth and moon round their common centre of gravity, which will be touched upon later (Art. 733).

(653.) Yet another method which has been used for the determination of the solar parallax has special interest, because it brings terrestrial researches into alliance with celestial observations in a very striking manner. I refer to the method depending on the measurement of the velocity of light.

We have already seen (Art. 564, *et seq.*) that every star shows by its annual motion in its aberration-ellipse that light travels with about 10,000 times the velocity with which the Earth moves in her orbit; and we shall see further on that the eclipses, occultations, and other such phenomena of Jupiter's satellites, by occurring later than the calculated time the farther the planet is from the earth, show that light travels with measurable velocity. From observations of Jupiter's satellites we cannot obtain very trustworthy results; we shall see further on that such observations are difficult, and often inexact; but from stellar observation astronomy can determine the proportion which the velocity of light bears to the Earth's velocity in her orbit with an accuracy proportional to that of the determination of the longer axis of each star's aberration-ellipse. The degree of accuracy obtainable in this determination may be regarded as akin to that which can be obtained in observations of Mars for determining the solar parallax, since the half major axis of the aberration-ellipse of each star—the *constant of aberration*, as it is called—is about 20″·45 [1]; and probably the immense number of observations made on the stars more than compensates for the less favourable condi-

[1] Delambre inferred from observations of the phenomena of Jupiter's satellites that light would take 8 min. 13 sec. in traversing the diameter of the Earth's orbit. Glasenapp, from much better observations made in 1848–1873, deduced 8 min. 20 sec. as the time required for traversing this distance. The above estimate of the constant of aberration would make the time 8 min. 18½ sec. The agreement between these results seems close enough, but those who know the actual difficulty of precise time-observations of the phenomena of Jupiter's satellites, to say nothing of the present condition of the theory of their motions, can place very little reliance on estimates of the velocity of light deduced from such observations.

tions under which the annual study of each star's aberration-range are made, as compared with the diurnal study of Martian parallax. Struve's estimate of the constant of aberration was 20″·445. Nyrén's later estimate 20″·492 is probably too great. Professor A. Hall of Washington, from observations of Alpha Lyræ at Washington, deduces the value 20″·454 (with a probable error of 0″·014). It is probable that the constant of aberration lies within 0″·03 on either side of the value 20″·45. To this degree of accuracy—that is, within limits of error corresponding to about $\frac{1}{1023}$rd part of the value required—the relative velocities of light and of the Earth in her orbit have been inferred. It appears that light travels about 10,102 times faster than the Earth travels in her annual path. If we can measure in any way the velocity of light, we shall be able to infer (within limits of error corresponding to those affecting the determination of the constant of aberration) the mean velocity of the earth in her orbit; and since we know the time she takes in completing her circuit of that orbit, its extent can be deduced with corresponding accuracy. Thus the sun's distance may be determined by terrestrial measurements of the velocity of light.

(654.) At first sight it might appear hopeless to attempt to measure a velocity which we already know to be not much short of 200,000 miles in a second; yet this, which would seem likely to be the chief difficulty in this method, is that part of the work which has been most satisfactorily accomplished. We know the velocity of light within probably not more than fifty miles per second on either side of its true value.

(655.) Two methods have been devised and employed for measuring the velocity of light, both being singularly ingenious:—

The first method, invented by Fizeau, depends practically on the measurement of the time elapsing between rapidly succeeding light-signals, the interval measured

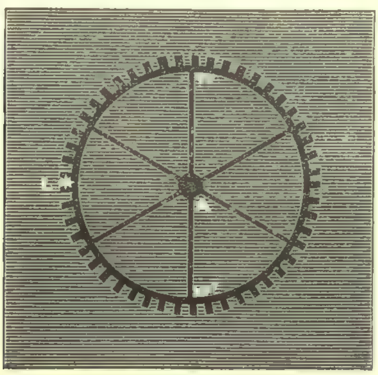


Fig. 194.—Illustrating the principle of Fizeau's method of measuring the Velocity of Light.

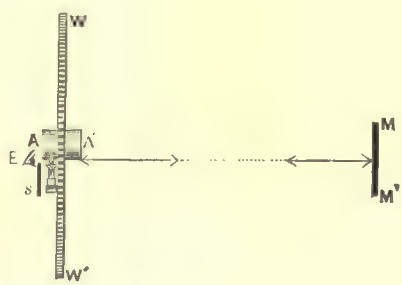


Fig. 195.—Illustrating the principle of Fizeau's method of measuring the Velocity of Light.

being that in which light has traversed a certain measured distance. Light from a suitable source at L (fig. 194), behind a rotating wheel W W′, passes between the teeth closely set round this wheel to a distant mirror, whence it is reflected along exactly the same course by which it had arrived, so as to pass through the same opening between the teeth to an eye set beyond the light and so shielded from this light as to

receive the light only by reflection from the distant mirror. The arrangement is (in principle) that shown in fig. 195, where W W′ is the wheel seen edgewise, E an eye behind the light, shown as a small flame, *s* a screen cutting off the direct light but not the light reflected from the distant mirror M M′. (Of course the actual arrangements are by no means so simple.) Now if the wheel W W′ is set in rapid rotation on the axis A (A A′ in fig. 195), it is clear that while the rays from L, which have passed through an opening between two teeth, are on their way to the mirror M M′ and back, the tooth between that opening and the next is on its way to occupy the space where the opening had been. If it can actually do so in such sort as precisely to fill that opening (the teeth being of the exact width of the spaces between them), then no light can reach the eye at E. As each opening comes before L light passes through to the mirror and back to the rotating wheel; but in every case, so long as the wheel retains its rate of motion unchanged, the light thus transmitted is intercepted on its return. Now, swiftly though light travels, it cannot pass over a distance which can easily be made as great as twenty miles (ten out and ten back) so quickly as one of the teeth of such a wheel as W W′ may be made to traverse half the distance separating it from its neighbour next in front.

In Cornu's second series of experiments, made in 1874 by this method, the mirror was 14 miles from the rotating wheel, so that the rays of light had to travel 28 miles on their way out and back. This distance light traverses in about $\frac{1}{6600}$ of a second. Cornu was able to make the wheel turn 1,600 times in a second, so that it made nearly a quarter of a revolution while the light was on its way to and from the distant mirror. In the most rapid rotation given to the wheel during M. Cornu's experiments, a tooth of the wheel made its journey over half the distance from tooth to tooth in about $\frac{1}{20}$ of the time taken by the light in travelling from the Observatory at Paris to Montlhery and back.

It will afford the reader an idea of the contrast between the simple arrangement shown in fig. 194 and the arrangements M. Cornu had to adopt in his experiments, to mention that the flashes of light were sent and received by a telescope 14 inches in aperture and 29 feet in focal length; while the mirror at Montlhery was placed in the focus of a telescope 6 inches in aperture, its stand being a cast-iron tube solidly fixed in masonry. The large telescope was so placed that the teeth passed through its focus at a point through which the light also passed on its way to the mirror, which was in the focus of the smaller distant telescope, and could be adjusted so as to send back the light precisely through the focus of the large telescope, beyond which the observer's eye received it, no light reaching him except that thus reflected.

(656.) M. Cornu in these experiments was not content with a velocity of rotation causing the interception of the returning light by the tooth past which it had travelled on its outward course, nor with three times that velocity, by which the returning light would have been caught by the next tooth but one, nor even with five times that velocity, by which the light would have been caught by the third tooth. His experiments began with the interception of the light on the fourth tooth, which required a velocity seven times as great as that necessary to catch it on the first tooth,[1]

[1] The teeth being equal in breadth to the spaces between them, it is clear that if the velocity of rotation necessary to just wholly catch the returning light on the first tooth is V, a velocity 2V will just fully admit the light through the next space to that through which the light passed outwards; a velocity 3V will catch the light on the second tooth; a velocity 4V will admit the light through the second space; a velocity 5V will catch the light on the third tooth, and so on velocities of 7V, 9V, 11V, &c., being necessary to catch the returning light wholly on the fourth, fifth, sixth tooth, &c., respectively.

and he determined the velocity of light not only with this velocity of rotation, but with the velocities necessary to catch the returning light wholly on the fifth tooth, the sixth, seventh, eighth, and so on up to the twenty-first (omitting only the twentieth, and that casually).

(657.) The velocity of light, as deduced from these experiments severally, was as follows:—

Tooth	Kilometres	Miles per second	Tooth	Kilometres	Miles per second	Tooth	Kilometres	Miles per second
4	300,130	186,500 –	10	300,640	186,810 +	16	300,620	186,800 –
5	300,530	186,740 +	11	300,350	186,630 –	17	300,000	186,410 +
6	300,750	186,880 –	12	300,500	186,730 –	18	300,150	186,510 –
7	300,820	186,920 +	13	300,340	186,630 –	19	299,550	186,110 –
8	299,940	186,380 –	14	300,350	186,630 +	20	no experiments on this tooth	
9	300,550	186,760 –	15	300,290	186,590 +	21	300,060	186,450 +
Sum	1,802,720	1,120,180		1,802,470	1,120,020		1,500,380	932,280
Mean	300,453	186,697		300,412	186,670		300,076	186,456

	Kilometres	Miles
Grand total	5,105,570	3,172,480
Mean value	300,328	186,616

(658.) Cornu himself took the mean value 300,330 kilometres, or 186,620 miles, as the distance traversed by light in a second in air, which would correspond to a velocity of 300,400 kilometres, or 186,660 miles, per second in a vacuum,[1] according to these experiments. But M. Helmert, of Aix, called attention to the circumstance (sufficiently indicated by the method of summation employed above) that with increasing velocity given to the rotating wheel the mean results tended to diminish; and since it is manifest that such measurements must be more trustworthy the greater the velocity with which the teeth move[2] (for every increase of velocity makes the contrast between the velocity of light and that of the teeth *less*), we seem justified in inferring from Cornu's experiments that the velocity of light in air is less even than the 300,076 kilometres, or 186,456 miles, per second, which is the mean of the last five sets of experiments, as above shown. Helmert suggested that the velocity actually pointed to by Cornu's experiments is 300,060 kilometres, or 186,450 miles per second in air, corresponding to 299,990 kilometres, or 186,410 miles per second in a vacuum.

(659.) It is generally conceded that Fizeau's method of determining the velocity of light is not capable of such accuracy as that devised by Foucault, and based on a method invented by Wheatstone for measuring the duration of the electric spark. Possibly—certain optical difficulties being first overcome—Fizeau's method might be brought into more favourable comparison with Foucault's, by using two wheels

[1] Among the most interesting results of experiments made by MM. Fizeau and Foucault on the velocity of light, must be mentioned their proof that light travels more quickly through air than through media of greater refractive power. In effect, though not quantitatively, this was a proof of the undulatory theory of light, according to which the velocity of light in media of different refractive power is *inversely* proportional to the refractive index. According to the emission theory the velocity of light would be *directly* proportional to the refractive index.

[2] It might seem at first sight that there must be great difficulty in determining the exact rate of rotation in these experiments. But great though the velocities of rotation used in Cornu's experiments and in those made by Foucault's method have been, they are measured with the utmost precision by electro-chronographic records.

rotating with equal velocity in different directions, which would double the relative velocity of the teeth (or, which is the same thing, would halve the intervals between the light-flashes); but probably Foucault's method will always be the more accurate, if applied with equal care and under equally favourable conditions.

(660.) The principle of this method may be thus indicated: M C M′ is a fixed concave mirror whose centre is at p; m p m′ a plane mirror capable of rapid rotation about a vertical axis through p. If, now, a single ray proceeds from a light at L to p, this ray is reflected in the direction p C, such that p C and p L make equal angles with p n, the normal to the face m p m′, and being reflected at C returns on the same track

FIG. 196. Illustrating the Wheatstone-Foucault method of determining the velocity of light.

to p and thence to L. To an eye placed at E, and capable of receiving the return ray, while shielded from the light L itself, the return ray will come back in the exact direction p L, howsoever the mirror may be turned on its axis, so long as the ray reflected at p falls anywhere within the arc M C M′. If the mirror be turned with sufficient rapidity to keep up the visual impression—say 20 or 30 times per second—light will be seen constantly coming in direction p L, though fainter than the actual light from L in the same degree that the arc M C M′ is less than a complete circumference. But if the mirror is turned with much greater rapidity than this, the light which, reflected from the mirror in position m p m′, had passed outwards along p C and then returned along C p, will find the mirror appreciably shifted, as to the position *m* p *m*′, and will therefore be reflected in the direction p E′, such that p C and p E′ make equal angles on either side of p n′, the new position of the normal at p to the mirror's surface. The angular displacement E p E′ is obviously equal to twice the angle n p n′—that is, to twice the angle m p *m*, through which the mirror has rotated while the light had been travelling from p to C and thence back to p. If the angle E p E′ is determined, and the rate at which the mirror is revolving is known, we know the length of time occupied by light in traversing twice the distance p C; in other words, we have measured the velocity of light.

(661.) The experiments made by Foucault, assisted by Fizeau and Breguet, by this method, in 1842, indicated a velocity of 191,000 miles per second. But later researches, conducted with the special object of testing by this method the reduced estimate of the sun's distance, gave 298,000 kilometres, or 185,200 miles, as the distance traversed by light in a second in vacuum, corresponding to a solar distance of 92,137,000 miles.

(662.) Lieut. Michelson, of the United States Navy, deduced by Foucault's method 299,940 kilometres, or 186,377 miles, as the distance traversed by light per second in a vacuum.

(663.) Lastly, Professor Newcomb, of the Washington Observatory, employing Foucault's method, with increased precautions to ensure accuracy, has deduced 186,326 ± 25 miles per second as the velocity of light *in vacuo.* This would correspond with a solar distance of 92,790,000 miles; affected, however, by a much greater probable error than the estimate of the velocity of light, since it depends on the constant of aberration, of which, as we have seen, we can only aver that it lies somewhere between 20″·43 and 20″·47.[1]

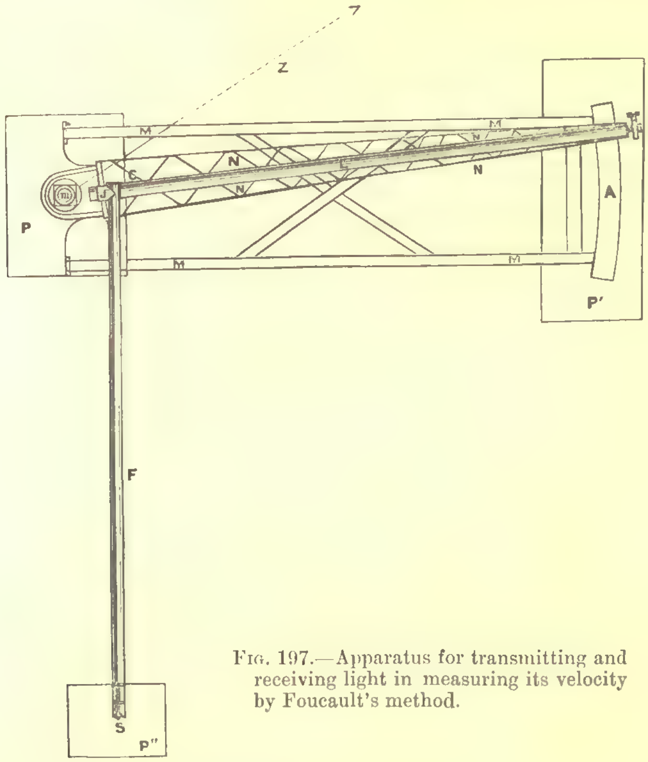


Fig. 197.—Apparatus for transmitting and receiving light in measuring its velocity by Foucault's method.

[1] To give the student a sufficient idea of the way in which such methods as Fizeau's and Foucault's for determining the velocity of light are applied in practice—of the difficulties, on the one hand, which have to be overcome, and of the mastery over the problem which, on the other hand, the use of the telescope gives the physicist, I may here touch on the method actually employed by Prof. Newcomb for transmitting and receiving the light.

The light was sent out from Fort Myer, on the Virginia side of the Potomac, the fixed mirror being set on the other side of the river, first in the grounds of the Naval Observatory, 1·585 mile away, and afterwards at the foot of the Washington Monument, 2·312 miles away. In fig. 197 F is the telescope by which light, received through the slit S, was transmitted after reflection by a diagonal prism, through the object-glass J to the revolving mirror *m*, and thence in the direction *m* Z to the fixed mirror, distant in the final experiments 2·312 miles. The light on its return was received (after reflection on the revolving mirror *m*) through the object-glass (which may be seen to the left of J) of the telescope L. When the mirror was not rotating the return ray travelled in the direction *m* A; when the mirror was revolving rapidly the return ray passed on one side or the other of A, and the receiving telescope L was directed accordingly. The range of motion of the frame N N, which bore the receiving telescope, was about 8°, and the actual displacement from A was read by the micrometer shown on the eye-end of the telescope L.

(664.) Considering carefully the various estimates of the sun's distance which have been deduced by the methods hitherto considered, viz. from the lunar parallactic inequality (Art. 495), from observations made in various ways during transits of Venus, from observations of Mars and of the asteroids, both by the meridional and diurnal method and by the measurement of the velocity of light combined with the determination of the constant of aberration, we find first that at present we cannot regard the sun's distance as determined within much less than 500,000 miles either way, and secondly that, within this distance of probable error either way, we may regard $92\frac{3}{4}$ millions of miles as a fair estimate of the distance. Throughout this work the distance of the sun is supposed to be 92,780,000 miles (corresponding to parallax $8''\cdot811$).

(665.) When we note the wide range of the values which have been deduced from year to year, we see that at present the sun's distance cannot be regarded as determined within four or five hundred thousand miles or so; yet this distance has been determined with surprising accuracy considering the nature of the problem. The chances are tens to one against the real mean distance of the sun being less than 92 millions of miles or greater than $93\frac{1}{3}$ millions of miles, hundreds to one against the distance being less than $91\frac{2}{3}$ millions of miles or greater than $93\frac{2}{3}$ millions, and thousands to one against its being less than 91 or greater than $94\frac{1}{3}$ millions of miles.

(666.) The scale of the solar system, adopting the mean distance of the sun used throughout this work, is fully indicated in the tables of elements at the end of the volume. But a more convenient presentation of the results dealt with in the present chapter will be found a little farther on (Art. 722). It may be noticed here, in reference to the plates illustrating this chapter, that the span of the orbit of Mars—the outermost in Plate IX.—is about $282\frac{3}{4}$ millions of miles, while the span of the orbit of Neptune—the outermost in Plate X.—is about 5,577 millions of miles. The span of the system formed by the Earth and moon is 477,636 miles; the span of the Martian system (that is, the diameter of the orbit of the outer satellite of Mars) is 29,200 miles; the span of the Jovian system is $2\frac{1}{3}$ millions of miles; that of the Saturnian system $4\frac{1}{2}$ millions of miles; of the Uranian system nearly three-quarters of a million of miles; and finally, the diameter of the orbit of the one known satellite of Neptune is somewhat more than 440,000 miles.

(667.) The scale of the solar system being ascertained, the masses of the various bodies composing it have next to be considered.

We cannot determine the absolute mass of any member of the solar system until we know the mass of the Earth; for all our estimates depend on the determination of the attractive power of each orb as compared with that of the Earth—at equal distances. We must begin, then, our study of

the masses of the sun and planets with the consideration of those various methods which have been employed for determining the mass of the Earth on which we live.

(668.) The great difficulty in determining the Earth's mass arises from the circumstance that it surpasses so enormously that of any known body with which we can compare it. A mountain may exert an attraction measurably comparable with that of the Earth; but a mountain is not a known mass, for we are not able to examine the mountain's whole structure, and short of such examination the mountain's mass must remain doubtful. We may descend into mines and recognise a change in the force of gravity suggestive of the law or laws according to which the density of the Earth's interior varies as the centre is approached; but we must always remain in doubt how far the change may be due to peculiarities in the structure of the strata through which the mine penetrates. The only trustworthy determinations of the Earth's mass are those in which the attraction of some known mass of matter, as a globe of lead or platinum, is directly compared with that of the Earth; and the largest masses of the kind which have yet been used in this way have been so infinitesimally small compared with the mass of the Earth, that it is only by apparatus of extreme delicacy that any measurable alteration of terrestrial attraction has been produced; so that all experiments of the sort are affected by a certain degree of uncertainty, depending on the delicacy of the methods employed and what may be called the tenuity of the results deduced.

(669.) It is a serious mistake to describe the measurements of the Earth's mass made by Maskelyne on Mount Schehallien, and by Mr. Dunkin in the Hartley coal-mine, in terms implying that the precision of the processes employed involved a corresponding degree of accuracy in the results obtained. Nothing but the most precise and careful observation and experiment could have led to any observable, still less to any measurable, results. But we must not suppose, because exceedingly delicate and precise observation can alone lead to any results at all in such experiments, that therefore results obtained by such observation are themselves of corresponding precision; on the contrary, the reverse must be assumed, unless the results obtained by different researches of this class shall be found to agree so closely as to indicate more trustworthiness in such methods than had been anticipated. But this has not happened. On the contrary, the reverse has thus far proved to be the case.

(670.) The Schehallien experiment, and experiments of the same type, may, with amply adequate precision, be thus described :—

Let M m, M′m', fig. 198, be the slopes of a mountain such as Schehallien, suit-

ably situate so that whatever attraction its mass may be capable of exerting can be estimated with fair chance of freedom from serious error; in other words, the mountain must stand in some degree apart, must admit of being thoroughly surveyed by geologists, and must present such features as suggest probable uniformity of interior stratification. Suppose now that B and B′ are two stations on a meridian — that is, on the same north-and-south line — whose positions have been determined by trigonometrical survey. Observations made at B and B′ with the zenith sector (Art. 206) would indicate the true difference of latitude between B and B′, were the plumb-lines at B and B′ in the true vertical directions A B and A′ B′. But if the attraction of the mountain's mass draws these plumb-lines into the positions A *b* and A′ *b*′, then the altitudes of B and B′ as determined by the zenith sector would be incorrect. Evidently, B being the northern station, and B P the direction of the polar axis, the plumb-line A *b* makes a smaller angle than A B, the true vertical, with the direction of the pole,

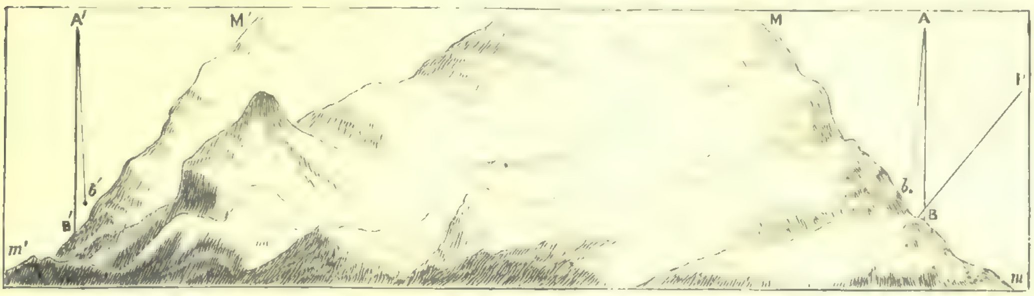


FIG. 198. The Schehallien experiment for determining the Earth's mass.

or seems to indicate for B too high a latitude; while A′*b*′ makes a larger angle than A′B′ with the polar axis, and seems to indicate for B′ too low a latitude. Both displacements increase the apparent difference of latitude; and comparing the difference of latitude determined by trigonometrical survey with the difference indicated by the plumb-lines at B and B′, the disturbing effect of the attraction of the mountain-mass is determined. Hence, after reductions and computations which, though seemingly complex, depend on established and even simple principles, the astronomer is able to compare the attraction of the Earth with the attraction of the mountain; and if the mass of the mountain can be ascertained, the mass of the Earth can be deduced.

(671.) Bouguer and La Condamine, during experiments on Chimborazo, recognised the effect of the mountain's attraction in deflecting the plumb-line (by about 11″), but no estimate of the Earth's mass was obtained, the conditions being too complex and uncertain. It was not till 1774 that a definite attempt was made to measure the Earth's mass by this method. In that year, Maskelyne made observations on the northern and southern slopes of Schehallien, a mountain only 3,000 feet in altitude, but well suited for the experiment. The sum of the deflections observed amounted to 11″·6; and from this result Messrs. Hutton and Playfair, after as thorough an examination of the mountain's structure as could be made, deduced for the Earth's mass a value corresponding to a mean density exceeding that of water 4·713 times. Observations made

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in 1855 on Arthur's Seat by Colonel James, Superintendent of the Ordnance Survey, indicated a mean density of 5·316.

(672.) Another method depends on the diminution of attraction as we ascend above the Earth's surface. The rate of oscillation of a pendulum will be calculably reduced at a height of two or three miles above the sea-level. If set at such a height on a mountain summit, the attraction of the mountain's mass will diminish the reduction of the rate of oscillation. Hence, if we know the mass of the mountain and measure the effect of its attraction, we can deduce the mass of the Earth.[1] From observations made on Mont Cenis by this plan, Plana and Carlini deduced a value of 4·950 for the Earth's mean density.

(673.) The converse of this method has also been used. If the oscillations of a pendulum at the sea-level be compared with those of a pendulum at a great depth below that level (as in a deep mine), we can compare the mass of the whole Earth with that of an outer shell limited by an imaginary spheroidal surface concentric with the surface of the Earth, and passing through the underground station. For this shell supposing it homogeneous, would exert no attraction at all at the lower station, and its non-homogeneity can be taken into account. The pendulum at the lower station is attracted only by the mass within the imagined spheroidal surface, with such corrections as may be due to the irregularities of the structure of the shell exterior to it, and more particularly to peculiarities in the neighbourhood of the mine where the experiment is tried. If the Earth were of uniform density the attraction at the lower station would obviously be less than that at the outer, in the same degree that its distance from the centre is less; for the masses of the complete sphere and of the sphere within the lower station are proportioned as the cubes of their radii, while gravity at the two stations would be as these masses directly and inversely as the squares of those radii; hence, considering both the masses and the distance, the attractions at the two stations would be inversely as the distances of these stations from the Earth's centre. But if the Earth's density increases towards the centre, the attraction at the lower station may be equal to or greater than that at the upper. Apart, then, from considerations depending on the configuration of the mine, and the structure of the strata

[1] The time of oscillation of a pendulum of given length, l, is inversely proportional to $\sqrt{g}$, where g represents the force of gravity as measured by the velocity it can generate in a unit of time (*see* Art. 441). Hence, if g, g' represent gravity at the sea-level and at a height h above that level respectively, and t, t' are the respective periods of oscillation as calculated for stations so situated respectively (the attraction of the mountain itself being neglected), we should have, approximately,

$$t : t' :: \sqrt{g'} : \sqrt{g}$$
$$:: \quad r \quad : r + h$$

(r being the Earth's radius). For, regarding the Earth as a globe made up of concentric layers, each of uniform density, the Earth's whole mass may be supposed at the centre, and gravity therefore inversely as the squares of the distances from the centre, that is, $g : g' :: (r+h)^2 : r^2$.

But if calculations based on the known configuration and structure of the mountain indicate that its mass would produce an attraction f on a body at its summit, and t'' be the observed time of oscillation at the mountain's summit,

$$t : t'' :: \sqrt{g' + f} : \sqrt{g}.$$

Thus, if the times of oscillation t and t'' be accurately compared, while t' is computed, we obtain the ratio of t' to t'', or $\sqrt{g'+f}$ to $\sqrt{g'}$; whence the ratio of f to g' and thence to g is obtained; and thus the mass of the Earth may be compared directly with that of the mountain. Of course, though the principle of the method is thus indicated, the actual computations are by no means so simple.

through which it has been dug,[1] the interpretation of the observed difference of attraction at the top and bottom of the mine is a matter of sufficient simplicity.

Fig. 199.—Illustrating the Mine method of weighing the Earth.

Fig. 200.—Illustrating the determination of Gravity at the bottom of a Mine.

[1] The effect of the removal of large quantities of material from below the surface is to *increase* the attraction on the pendulum at the bottom of the mine. It will suffice, in illustration of this, to consider the Earth as a homogeneous globe, and the space dug out as of some regular figure, —for instance, a cylinder. The student will readily infer the nature of the general considerations (all I wish here to indicate) which have to be taken into account in dealing with such problems:—

Let B, fig. 199, be a mine of any figure, with, however, a horizontal base. Take the spherical shell A S C, of which the inner surface coincides with the base of the mine. Then the attraction at the bottom of the mine is that due to the mass of the sphere within the shell, plus the attraction of the incomplete shell; and the incomplete shell attracts the particle towards the Earth's centre with a force precisely equal to the repulsion due to a mass filling the space B. For if the attraction exerted by the incomplete shell be A, and that exerted by the portion B (supposed filled) be B, then we know that

$$A + B = 0$$

whence

$$A = - B.$$

Hence, instead of considering the incomplete shell A, we need only consider the *repulsion* exerted by a mass filling B, every particle of which is to be supposed to repel with a force exactly equal to that with which it would in reality attract.

Let us suppose our mine to be cylindrical, fig. 200, representing a section through the axis and the shaft S coincident with the prolongation of the axis. A pendulum is swinging at Q and another at P in Q S produced. The shaft is supposed so narrow that we need not consider it; and for convenience we suppose its length equal to half the depth of the mine:—

Let r = Earth's radius
$2a$ = mine's depth (Q P)
a = length of shaft (Q S)
b = radius of cylinder (B P)

Then the attraction at Q is equivalent to the attraction of a sphere of radius r, diminished by the attraction of the cylinder A D, that is (from the known value of the last-named attraction)—

$$= \frac{4\pi\rho r}{3} - 2\pi\rho\left\{a - \sqrt{4a^2 + b^2} + \sqrt{a^2 + b^2}\right\} (\alpha)$$

(ρ being the density of the supposed homogeneous sphere).

Again, the attraction at P is equivalent to the attraction of a sphere of radius $(r - 2a)$, increased by the *repulsion* of the cylinder A D, that is

$$= \frac{4\pi\rho(r - 2a)}{3} + 2\pi\rho\left\{a - \sqrt{a^2 + b^2} + b\right\}, (\beta)$$

The excess of the attraction at Q over that at P—that is, the difference of the expressions (α) and (β) is

$$\frac{8\pi\rho a}{3} + 2\pi\rho\left\{\sqrt{4a^2 + b^2} - 2a - b\right\}$$

$$= 2\pi\rho\left\{\sqrt{4a^2 + b^2} - b\right\} - \frac{4\pi\rho a}{3}; (\gamma)$$

and according as this expression is positive, zero, or negative, the attraction at Q is greater than, equal to, or less than the attraction at P. Putting (γ) equal to zero, we get

$$3\sqrt{4a^2 + b^2} = 3b + 2a$$

whence

$$36a^2 + 9b^2 = 9b^2 + 12ab + 4a^2$$

that is

$$32a = 12b.$$

Hence, if b, the radius of the cylindrical mine, is equal to four-thirds its total depth, the attraction at P will be equal to that at Q. If the mine be wider the attraction will be greater at P than at Q.

All other cases may be similarly treated, though, of course, the problem will not in all cases be so simple as the above.

(674.) Though it is tolerably obvious that this method of determining the Earth's attraction must be even less trustworthy than experiments of the Schehallien type, the method commended itself to the mind of Sir George (then Professor) Airy, and after two failures in the Dolcoath mines, he had the method tried in the Harton Colliery, near South Shields, where a depth of 1,260 feet was available. It is not necessary to describe the contrivances by which two pendulums were compared, one swinging at the top, the other at the bottom of the mine, the pendulums being interchanged after intervals of 104 hours and 60 hours—that is, each working 104 hours above and below, and then each working 60 hours above and below. The work was conducted with great care and skill by Mr. Dunkin, of the Greenwich Observatory, to whom, however, no portion of the credit has hitherto been given in treatises on astronomy.

(675.) The observations showed that at the foot of the mine either pendulum gained two seconds and a quarter per day, showing that gravity was increased by $\frac{1}{19190}$ part. This result cannot be sensibly in error, so carefully were Mr. Dunkin's operations conducted. But the inferred increase of density towards the Earth's centre—that is, the deduced mean density of the Earth—can by no means be regarded as ascertained with corresponding accuracy. We do not make an ordinary foot-rule more precise for measuring purposes by careful and complicated experiments on the changes it undergoes under the varying influences of temperature, moisture, and so forth; it remains a foot-rule still, with all a foot-rule's shortcomings as a measurer. The complicated calculations and corrections effected by Mr. Dunkin did not even touch the real defects of the mine method of weighing the Earth; it remained rough and untrustworthy. A comparison of the final result with that obtained by experiments of the Schehallien class—which, though certainly not trustworthy, were at least as trustworthy as the mine experiments—suffices to show how little either method can be trusted. The Earth's mean density, as calculated by Mr. Airy from the Harton experiments, was 6·565 times that of water; and the results of the mountain and mine methods combined appear as follows:—

	Mean Density (Water's = 1).
Maskelyne's Schehallien experiment (corrected by Playfair)	4·713
Carlini's pendulum experiments on Mont Cenis (corrected by Giulio)	4·950
Colonel H. James, from attraction of Arthur's Seat	5·316
Dunkin and Airy, from experiments in Harton Colliery	6·565
Mean	5·386

(676.) The mean value is probably, as we shall see, near the truth; but

as the greatest value differs from the least by 1·852, or more than a third of the mean, the results—except Colonel James's—are discredited by the very circumstance that the mean value is nearly right; and all three *methods* are discredited, the correctness of Colonel James's result being thus shown to be merely accidental. If the length of a piece of ground had to be measured, and one workman said that measuring it in a certain way he found it to be 471 feet long, and another using a different method made it 495 feet long, while a third, using yet another method, found it to be 657 feet long, their employer would not regard their work or their methods as satisfactory if he presently found that a thoroughly trustworthy measurement showed the piece of ground to be 550 feet long, even though that is not far from the mean of the results obtained by three unsatisfactory methods. And if, later, a fourth workman, employing one of those methods, deduced as a result 539 feet, trust in that method would not be greatly increased. It would be felt that either the approach of the result to the truth was accidental or it was more or less consciously forced.

(677.) Airy expressed the opinion that the result of the Harton Colliery experiment was comparable on at least equal terms with those obtained by other methods, though it differs by twenty per cent. from the mean of the results obtained by all methods, and the results presently to be considered do not differ by more than two per cent. from their mean value, nor by four per cent. from the general mean. I prefer the opinion of Sir Edmund Beckett (now Lord Grimthorpe), that the result of the Harton Colliery experiments 'cannot be accepted;' and is 'not to be compared in value' with those obtained by the Cavendish experiment.

(678.) The ingenious Michell, to whom science owes the first satisfactory reasoning about the architecture of the sidereal heavens, devised the method of weighing the Earth which is commonly named after the eminent chemist, Cavendish, who first successfully applied it.

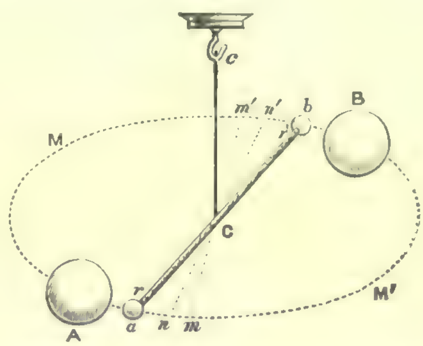


Fig. 201.—Illustrating the principle of the Cavendish experiment. (*Baily.*)

(679.) The principle of the method is illustrated in fig. 201. Here a and b are two small globes at the ends of a uniform rod $r\,r'$, suspended in a horizontal position by the cord or wire $c\,C$, attached to its centre C. The horizontal rod, left to itself, tends to a mean position which may be called the *position of rest*, though, as a matter of fact, when the suspension is as delicate as it has to be in the experiments considered, the rod never is at rest, but oscillates constantly, and very slowly, through short arcs on either side of its mean position. A and B are two heavy globes, which

can be brought readily into such positions as are shown in fig. 201, where their attractions tend to draw *a* and *b* towards them, (*a* towards A, *b* towards B). The result of these disturbing influences is to sway the rod *r r′* from what had been its position of rest, when undisturbed, to some new position of rest, as *n n′* or *m m′*, about which it oscillates as before.

(680.) The processes of observation are as follows: First, the time of oscillation of the undisturbed rod is noted, to ascertain the force of torsion which has to be overcome to produce a given displacement. Then, the large globes being brought up to such positions as A and B, their distances from the positions of rest of *a* and *b* when these were undisturbed is carefully measured, and the new position of rest taken up by *a* and *b* is ascertained, the times of oscillation being noted throughout, so that any change in the torsion may be recognised and taken into account. This having been done, the globes A and B are removed to the mean positions M, M′, and the balls *a* and *b* are allowed to return to their position of rest. Then the globes A and B are carried round in the same direction until A is close by *b* on the left, and B close by *a* on the right, when their attractions tend to displace *a* and *b* in directions contrary to those before observed; and the position of rest of the rod *r r′* is ascertained (the times of observation being throughout carefully noted) as before. From these observations, the attraction of the globes A and B on the balls *a* and *b* (or *b* and *a*) can be determined, since the times of oscillation indicate the torsion, and the position of rest determines how much of the torsion is overcome by the globes' attraction. By calculating next what the attraction of either globe would be if, instead of being at its measured distance from the neighbouring ball, it were at the Earth's centre, and comparing this with the known attraction of the whole Earth, we can ascertain how much the whole mass of the Earth exceeds the known mass of the leaden globe: in other words, we can ascertain the mass of the Earth, and therefore its volume being known —we can determine its mean density.

(681.) Cavendish, near the end of last century ('Phil. Trans.,' 1798), applied this method in a form somewhat simpler than that described, but depending practically on the same principles, deducing a mean terrestrial density of 5·48. Hutton, re-examining Cavendish's experiments, reduced the deduced density to 5·32; but many prefer Cavendish's own treatment of his observations.

(682.) The experiments made by Cavendish were not very numerous, neither were those of Reich, of Freiberg, who in 1838 deduced by the same method a mean terrestrial density of 5·44. We must attach much more weight to the experiments made by Francis Baily in 1838–42, since they were not only conducted with singular care and caution, but were obtained in many different ways (though, of course, all by the same general method) and deduced from a very large number of experiments. It will be well to give a few details respecting Baily's work, that the trustworthiness of his estimate of the Earth's weight may be fully recognised.

(683.) The rod *r r′*, fig. 201, 6½ feet long, was of light wood (in nearly all the experiments), and was suspended in various ways in the different experiments, viz. on

single copper wires ·0178 inch and ·0219 inch in diameter; on two parallel iron wires 0·177 inch, 0·367 inch, and 0·415 inch apart; on two brass wires 0·380 inch and 0·415 inch apart; and on two silk fibres 0·177 inch, 0·367 inch, ·0380 inch, and 0·415 inch apart. At the ends of the rod were attached balls of different material and size in different experiments, viz. 1½-inch platinum, 2-inch ivory, 2-inch glass, 2-inch zinc, 2-inch lead, 2½-inch lead, and 2½-inch brass. In a small number of experiments (56) the wooden rod was replaced by a brass rod without balls. The torsion rod and its suspension were enclosed in a case with a glass at one end. The devices by which the effects of electricity, magnetism, radiation, and other disturbing influences were as far as possible eliminated need not be described, nor the multitudinous experiments considered by which explanations of irregular discordances were sought for, or corrections introduced. Nor is it necessary to describe or picture Baily's instrument either as a whole or in detail—the general principle of his method, already sufficiently explained, being all that is required and all that the student really needs to understand. Let it suffice to note that the experiments for the correction and explanation of discordances were more numerous than those actually employed for the determination of the Earth's mean density.

(684.) The experiments thus used amounted in all to 753. Of these, however, 56, above mentioned, when only a brass rod was used, were not seriously intended for the determination of the Earth's mean density. They are well described by De Morgan as 'a defiance to the apparatus to fail if it could.' Yet they indicated a mean density below 6—a result much nearer the truth than the Harton Colliery experiments had given, costly and complicated though they were. The remaining 697 experiments gave results ranging from 5·5 to 5·847. The mean value deduced by Baily (due weight being given to each set of experiments) was 5·66.

(685.) Cornu, in 1872, applying the same method with improvements suggested by recent scientific developments, obtained the value 5·56. He did still better work in applying to the more numerous experiments of Baily, as recorded, corrections justified by recent physical discoveries. He found that as thus corrected Baily's elaborate and beautiful experiments indicated a mean Earth-density of 5·55.

(686.) The following table presents the results of the application of Michell's method (it is interesting to compare it with the table in Art. 675):—

Cavendish	5·48
,, (revised by Hutton)	5·32
Reich	5·44
Baily	5·66
,, (revised by Cornu)	5·55
Cornu	5.56
Mean	5.51

(687.) The true mean value of the experiments made by this method,

due weight being given to each result, and Cornu's revision of Baily's experiments being accepted, is so near 5·55, that this may fairly be taken to represent the most probable mean density of the Earth, the error being probably not more than 0·5; in other words, the density of the Earth probably lies between 5·5 and 5·6.

(688.) Taking the Earth's mean density at 5·55 times the density of water, the Earth's mass = 590,654,000,000,000,000,000 tons.[1]

[1] The Earth's equatorial radius contains in round numbers 20,926,200 feet, the polar radius being $\frac{1}{289}$ less, and 35·943 cubic feet of water weigh one ton. Hence, the Earth's mass, expressed in tons,

$$= \frac{5·55}{35·943}\left(\frac{4}{3}\right)(20{,}926{,}200)^3\left(\frac{288}{289}\right)(3·14159)$$

which when duly worked out (the use of logarithms will greatly help the reader who cares—as every reader should to test the calculation) gives the above value.

There is a method by which, I think, the mass of the Earth might be directly compared with that of a known mass of lead or other heavy metal, without the difficulty arising from the varying torsion, under varying conditions, in the Michell method. The plan, suggested (but in an unworkable form) by Professors Richer and Mayer, of the Berlin University, will be readily understood from figs. 202 and 203. A B D is a globe of lead, which should be three or four feet in diameter, whose centre is at C. At M, the highest point of this globe, a small steel block is set, on which rests the knife-edge c of a balance $a\,c\,b$. The large globe A B D is pierced along the vertical directions A D and B E, immediately under the extremities a and b of the balance-arms $c\,a$ and $c\,b$, in such sort that the balance can be used to weigh bodies either above A B or below D E, or one above A B and the other below D E, as in the case illustrated in fig. 202, where a weight w above A is weighed against a weight w' below E. Fig. 203 shows the balance on a larger scale, and illustrates the arrangement suggested as best for applying the weights. Immediately above the centre-piece c (the knife-edge of which rests on the horizontal surface $k\,l$ at e) is a thin plate of polished steel which, when the balance is level, has its plane at right angles to the then horizontal direction $a\,b$. A horizontal beam of light from a distant source, situate either on the left or on the right of the apparatus, as pictured, falls on this steel mirror, and when the balance-beam is level returns after reflexion upon its horizontal track, but when the beam is inclined the return ray is inclined to the horizon. Thus the reflected ray practically serves as a very long index by which to measure the deflection of the balance. [In the actual experiments the light could be sent out and received as in the

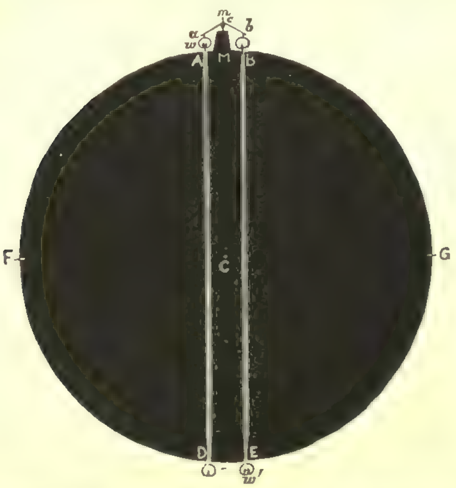


Fig. 202.—Illustrating a plan suggested for weighing the Earth directly against a globe of heavy metal.

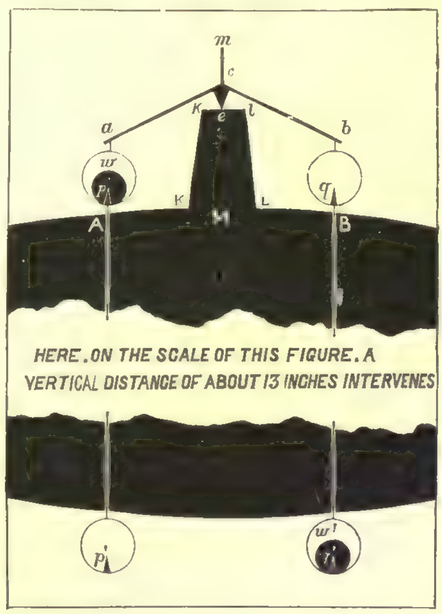


Fig. 203.

(689.) The determination of the moon's mass may be taken next in order, though not next in importance, to the determination of the mass of the Earth.

observations for determining the velocity of light by Fizeau's and Wheatstone's methods.] Two weights exactly equal and similar suffice for all the experiments, but preferably four should be provided, so that in any set of experiments there need be no occasion to transfer a weight from above the large globe to below, or *vice versâ*. It would be desirable also that sets of weights of different materials should be employed, so that diversities depending on the physical qualities or inter-relations of different substances might be eliminated. The weight-holders are shown in fig. 203 at p, q, p', and q'. Their construction should be such that with the least amount of disturbance a weight, such as is shown at p and q', may be added, removed, or transferred, as the experiments proceed. The scale-pans and weights should depend from knife-edges at a and b, in the usual way where delicate weighing is required.

The method of the experiments is as follows in a case where a full set of four weights is employed: –

All the four weights to be used in a given set of experiments are to be first carefully weighed against each other, in pairs, above A B and below D E, every pair being thus tested, each weight in each test being put on one side of M in one trial and on the other side in another, and the indications of the long light-pointer carefully noted in each of the twenty-four trials thus made. [Six pairs of weights can be selected from the four; and taking any pair, w and w', these can be weighed against each other in two ways in the upper weight-holders, and in two ways in the lower, or four ways in all. Thus twenty-four weighings must be made to eliminate all errors arising from differences—however minute—between the weights.] Next, each pair of weights must be balanced against each other, one being above and the other below. One such trial is illustrated in fig. 202. The weight w above A, which should exactly balance the weight w' set in the scale-holder above B, will not balance w' set below E. For while each is drawn downwards with equal force by the Earth's mass (or with only such minute and calculable difference as results from the distance of w from the Earth's centre being greater than the distance of w'), w is drawn towards C, or *downwards*, by the attraction of the great globe A B D, while w' is drawn towards C, or *upwards*, by the same attraction. The amount of deflection, as indicated by the light-pointer, is to be carefully noted, and then the weight w transferred to B and the weight w' to D, when an opposite deflection will take place, which is to be similarly noted. Next the two experiments are to be repeated, the weight w' being above and the weight w below. Corresponding experiments are to be made with each of the six pairs of weights which can be formed out of the four. The mean of the observed deflections compared with the mean of the twenty-four preceding observations (in all of which the beam is approximately horizontal) will supply the means of comparing the attraction of the Earth with the attraction of the globe A G D. Of course many details must be taken into account: as, the weight of the wires or cords along A D and B E; the angles such as C w D and C w'B when w and w' are in equilibrium; the difference of the distances of w and w' from the Earth's centre; and the like. But all such matters are readily calculable from the original data and the indications of the ray-index.

Here is a rough calculation of the effects we may expect to recognise in such experiments:

Supposing the globe A B D to be of lead, and therefore of twice the mean density of the Earth, and 4 feet in diameter, its mass compared with that of the Earth is as $2(2)^3$ to $(10{,}500{,}000)^3$; but the weights w and w', neglecting their distance from the surface of the large globe and from the vertical diameter, are only 2 feet from the centre of the globe, while they are 10,500,000 feet from the centre of the Earth. On this account the attraction of the globe is greater than the attraction of the Earth as $(10{,}500{,}000)^2$ is greater than $(2)^2$; and since the attraction of the globe is exerted upwards on one weight and downwards on the other, we must regard it as doubled, and this doubled attraction as practically again doubled by the interchange of the ends of the balance from which the upper and lower weights are severally suspended. Hence, finally, while the attraction of the upper globe, so far as it depends on mass, is less than the Earth's in the ratio

$$2\ (2^3) \text{ to } (10{,}500{,}000)^3$$

it is increased, through the diminution of distance and the adjustment and interchange of the weights, in the ratio

$$4\ (10{,}500{,}000)^2 \text{ to } (2)^2$$

Hence, finally, the effect of the leaden globe's pull, quadrupled as in the experiments, bears to the effect of the Earth's pull the ratio compounded of these two, viz. the ratio

$$16 \text{ to } 10{,}500{,}000$$

So that if our balance, with its ray-index, is delicate enough to measure a difference of weights amounting to about $\frac{1}{656000}$ of the weight used, we may expect this plan to afford a direct, and in

Since in reality the relation between the distance and period of the moon's relative motions round the Earth depends in part on the moon's mass, we have at once a means of determining the moon's mass from her known mean distance and time of circling round the Earth—due account being taken of the perturbations she experiences. For, whereas a *particle* travelling round the Earth at the moon's distance would have a period depending on the attraction of the Earth at that distance in the manner considered in Arts. 455 to 461, the moon, travelling at that distance, has a period shorter than this in the degree in which the square root of the Earth's mass is less than the square root of the sum of the masses of the Earth and moon. Hence, since this period is known and the former hypothetical period is calculable from the moon's measured distance, we can infer the moon's mass with an accuracy proportioned to the accuracy of the determination of the moon's distance. The accuracy of this method, however, depends not only on the accuracy of such measurements, but also on the exact determination of the effects of solar perturbations, without which we cannot tell precisely what the moon's period would be were she undisturbed.

(690.) We can also determine the moon's mass theoretically by comparing spring tides, when her attraction and the sun's are combined, with neap tides, when her attraction is diminished by the sun's, for we can thus compare her attraction with the sun's. But this method is not capable of giving results of any considerable accuracy.

(691.) A third method results from the circumstance that the Earth revolves once a month around the common centre of gravity of the Earth and moon. For the effect of this motion is that the Earth is sometimes in advance of her mean place (in longitude) by the radius of this small orbit, and sometimes as much behind her mean place, the advance of the sun in his apparent motion along the ecliptic being correspondingly affected. If such effects are accurately measured, and the sun's distance is known, we can infer the diameter of the Earth's small orbit round the common centre of gravity of the Earth and moon; and having thus determined the position of that centre of gravity, we can deduce at once the proportion which the moon's mass bears to the Earth's.

In recent times the meridional observations of the sun have been so numerous and exact, that the means of determining the moon's mass by this method are much more satisfactory. Thus we can place very great reliance on Leverrier's estimate of the parallactic inequality, viz. $6''\cdot50$. Professor Newcomb, of Washington, deduces from a yet wider range of observations the value $6''\cdot52$. These values lie so close together as to show that the observations on which they have been based suffice for the very accurate determination of this quantity.

(692.) The value of the moon's mass which we should infer from the mean

that respect satisfactory, comparison between the mass of the Earth and that of a globe of known size and density.

[It might seem as though the slight difference of distance of the two small weights from the earth's centre might be neglected. Lord Brougham incurred some just ridicule for the rather strange mistake of asserting that a man can carry a weight more easily over his shoulder than in his hand, because in the former position the weight is farther from the Earth's centre. But in delicate experiments such as we are considering in the text, the difference due even to a few feet of distance must be taken into account. Thus, suppose A C to be two feet, or w rather more than four feet farther from the Earth's centre than w'; then, since the Earth's radius contains in round numbers 21 millions of feet, we may take w' to be $\frac{1}{5000000}$ nearer the Earth's centre than w, by which difference the attraction of the Earth on w' is increased by $\frac{1}{2500000}$ part. This, though minute, is appreciable, even considerable, in relation to the quantities we are dealing with in these experiments.]

(6″·51) of these two estimates will depend on the value we assign to the solar parallax. The mean equatorial horizontal solar parallax assumed throughout this volume is 8″·81; it would follow that the distance of the centre of gravity of the Earth and moon from the Earth's centre is $\frac{651}{881}$ of the Earth's equatorial semi-diameter, or $\frac{651}{881}$ of 3,963 miles; that is, about 2,928 miles. Thence it follows that the moon's mass is to the sum of the masses of the Earth and moon as 2,928 to 238,830, or

$$\text{Moon's mass} : \text{Earth's mass} :: 2928 : 235{,}902$$
$$:: 1 : 80{\cdot}6$$

that is, with these data the Earth's mass is found to exceed the moon's $80\frac{3}{5}$ times.

(693.) It will be observed that, if the moon's mass has been otherwise satisfactorily determined, this calculation enables us to obtain an estimate of the distance of the sun. It was, indeed, first employed for this purpose.

(694.) Since the Earth's nutation is solely dependent on the moon's mass, it follows that when its observed value is compared with the formula deduced by theory, a means of determining the moon's mass is obtained. This is the best method available.

(695.) Laplace, adopting Maskelyne's value of the maximum nutation—namely, 9″·6—inferred for the moon's mass $\frac{1}{72}$ (the Earth's being regarded as unity). Professor Newcomb, adopting 9″·223 for the lunar nutation, and 50″·378 for the annual luni-solar precession, deduced the value $\frac{1}{81\cdot 68}$. Leverrier with the same value deduced $\frac{1}{[illegible]}$. With the estimate of the sun's distance here adopted, the moon's mass, as deduced from nutation, would be about $\frac{1}{80\cdot 75}$ of the Earth's. This is the value employed here.

(696.) Taking the moon's volume as $\frac{100}{4944}$ of the Earth's, it follows that the moon's mass bears a smaller proportion to the Earth's than her volume bears to the Earth's volume, in the ratio of 4,944 to 8,075. Hence the moon's mean density must be less than the Earth's in this ratio. So that if we express the Earth's density by unity, the moon's will be expressed by 0·611. If the Earth's mean density be held to be 5·55 times that of water, the moon's mean density is 3·39 times the density of water.

Fig. 204. Illustrating the Sun's pull on the Earth.

(697.) For the determination of the sun's mass two methods are available. We may compare the sun's observed attractive action on the Earth (or any other planet) either with the attraction of the Earth on bodies at her surface, or with the Earth's attraction on the moon.

(698.) The former method gives the following calculation:—

Let $e\,e'$E, fig. 204, represent the Earth's orbit (regarded as a circle) around the sun at S. Then, if $e\,e'$ represent the distance traversed by the Earth in one second, we know that

$$e\,e' = \frac{2\,\pi \times 92{,}780{,}000}{365{\cdot}256 \times 24 \times 60 \times 60}\ \text{miles}.$$

Now, if e'L is drawn perpendicular to eE, eL represents the amount by which the Earth has been drawn sunwards in passing from e to e', and we know that $e\mathrm{E}.e\mathrm{L} = (e\,e')^2$. Hence

$$e\mathrm{L} = \frac{2\,(\pi)^2 \times 92{,}780{,}000}{(365{,}256 \times 24 \times 3{,}600)^2}\ \text{miles} = {\cdot}0097\ \text{feet}.$$

Comparing this space traversed under the influence of the sun's attraction at a distance of 92,780,000 miles with the fall of 16·100 feet in a second[1] at the Earth's surface, or at a distance of 3,963 miles from the point where the whole mass of the Earth may be supposed to be collected, we see that

$$\text{Sun's mass} : \text{Earth's mass} :: \cdot 0097 \times (92{,}780{,}000)^2 : 16\cdot 10 \times (3{,}963)^2$$

whence

$$\text{Sun's mass} = 330{,}500 \text{ times Earth's mass.}$$

(699.) The other method of determining the sun's mass, by considering the moon's distance and period under the Earth's attraction, is complicated by the circumstance that the sun perturbs the moon in such sort as somewhat to diminish her period of revolution round the Earth. Regarding this as a correction to be taken into account after the sun's mass has been provisionally determined in this way (a fresh calculation being then entered upon), the method depends on the following considerations:—

(700.) The moon and the Earth are constantly deflected from the direction in which they are at each moment travelling, the former by the Earth's attraction, the latter by the sun's. Now we have seen (Arts. 446, 447) that the angular deflection of a body tending to advance in a straight line is inversely proportional to the body's velocity and directly proportional to the time. Hence the force producing a given deflection in a given time on a body moving with given velocity is directly proportional to the velocity and inversely proportional to the time. Combining this with the fact that the velocity in a circular orbit is proportional to the radius of the orbit divided by the period, we see that

$$\left.\begin{array}{l}\text{Sun's attraction}\\ \text{on the Earth}\end{array}\right\} : \left\{\begin{array}{l}\text{Earth's attrac-}\\ \text{tion on moon}\end{array}\right\} :: \frac{\text{Sun's distance}}{(\text{sidereal year})^2} : \frac{\text{Moon's distance}}{(\text{sidereal month})^2}$$
$$:: \frac{92{,}780{,}000}{(365\cdot 256)^2} : \frac{238{,}830}{(27\cdot 322)^2}$$

Whence the sun's attraction on the Earth (or moon) is found to exceed that of the Earth on the moon (diminished by the sun's perturbing influence) 2·1737 times. Since the sun's force is exerted at a distance of 92,780,000 miles and the Earth's at a distance of 238,830 miles, we find finally the sun's attraction exceeds the Earth's (diminished in the slight degree mentioned) at equal distances

$$2\cdot 1737 \times (92{,}780{,}000)^2 \div (238{,}830)^2 \text{ times}$$
$$= 328{,}050 \text{ times.}$$

Whence it would follow, since it is the sum of the masses of the Earth and moon[2] which determines the period of the moon's revolution, that the sun's mass exceeds the Earth's mass

$$328{,}050 \times 81\cdot 75 \div 80\cdot 75, \text{ or } 332{,}100 \text{ times.}$$

This is too great, but diminished in the proportion which the Earth's attraction on the moon bears to that attraction diminished by the mean effects of the sun's perturbing

[1] The actual attraction of gravity at the equator, unreduced by the effect of the centrifugal tendency, is 32·19926; by which is meant that under the influence of that attraction a body would in one second acquire a velocity of 32·19926 feet per second. The fall in a second would be half this, or 16·09963, which to the third decimal place is correctly represented by 16·100.

[2] The period of a body of mass m round a body of mass M is inversely proportional to the square root of $(m + \text{M})$: hence, since the mass determined in the manner shown above depends on the square of the period of the moon, the result represents the ratio of the sun's mass to the sum of the masses of the Earth and moon.

action, it indicates the result already obtained, viz. that the sun's mass exceeds the Earth's 330,500 times.

(701.) The mass of any planet which has a satellite, or a system of satellites, can obviously be determined in either of the ways here employed. Thus, in applying the first to Jupiter, we take the sidereal period of any one of his moons instead of the sidereal year, and the distance of that moon from Jupiter's centre instead of the sun's distance from the Earth. In all other respects the calculation proceeds precisely as in Art. 700. In applying the other method, which depends on the comparison between the orbital motions of two bodies, one travelling round a body of known mass, the other round the body whose mass we wish to ascertain, we now have a choice between two comparisons: we may compare the motion of a satellite round the planet whose mass we require either with the motion of the moon round the Earth (suitably corrected, as in Art. 700), or with the motion of the Earth round the sun. Indeed, after our first computation for determining a planet's mass, we may determine the next by three comparisons on this second plan, the next again by four, the next by five, and so on; the results of all these comparisons necessarily agreeing if each has been correctly computed.

(702.) The masses of the several planets can also be computed by a third method, not applicable as a distinct method to the sun, viz. by their effects in perturbing other bodies, whether planets or comets, which come for awhile under their disturbing influence. This, indeed, is the only method available for determining the mass of a planet which has no satellite; so that it becomes a matter of some importance, in connection with the general problem we are considering, to inquire how far this method may be trusted.

(703.) In the first place, it is to be noticed that the application of the method is more satisfactory when the perturbed body is known to be certainly small in mass compared with the perturbing planet. For if the mass of the disturbed body is comparable with that of the disturbing planet, we must take their comparative masses into account, and this we are not able to do exactly until we know just what our inquiry is directed to ascertain, the mass of the planet as yet unweighed.[1]

[1] There seems to be singular liability to error on this point, even the keenest mathematicians erring sometimes in the general discussion of such problems, though of course when they come to deal with them in detail they get right. For instance, Sir John Herschel wrote to me in 1870 correcting what he regarded as an error in my *Other Worlds than Ours*, where I had stated that the asteroids are more perturbed by Jupiter and Saturn than planets of greater mass would be. For, said he, a planet or a peppercorn would behave in precisely the same way under the attraction of Jupiter were Jupiter alone, and therefore must be equally perturbed by his influence when it is combined with that of other planets and is subordinate to that of the sun. In my reply I thought it sufficient to call his attention to the long inequality of Saturn and Jupiter, by which the smaller planet is the more affected. This presently set him on the track of the error which had misled him. So far as the relative orbits of two bodies around each other are concerned, it matters not how the masses are distributed: the sum of the masses remaining unchanged, the bodies might be equal, or one might be a grain, and the other many trillions of trillions of tons. But as regards the actual orbits of the two the distribution of the masses is all-important: in the latter case one body would be appreciably at rest, the other would travel in a wide orbit; in the former the two bodies would travel in equal orbits, each orbit having half the dimensions of the wide orbit described by the minute body in the other case. The perturbations of small and large bodies are perfectly analogous to their relative orbital motions. When an asteroid is acted on by Jupiter, the asteroid's perturbation takes place appreciably about Jupiter as a centre—because the common centre of gravity of the immense mass of Jupiter and the very small mass of the asteroid is appreciably the same as the centre of gravity of Jupiter. When Saturn

(704.) The relative values of these different methods of determining the mass of a planet are well indicated by the results obtained in the case of Jupiter, whose mass has been more exactly determined than that of any of the other planets.

Comparing Jupiter's mass with the sun's by the two first general methods (practically identical in this case), the following results have been obtained in past and recent times:—

Triesnecker	made Jupiter's mass .	$\frac{1}{1048\cdot55}$	of the sun's.
Santini	,, ,, .	$\frac{1}{1051\cdot09}$	,,
Airy	,, ,, .	$\frac{1}{1047\cdot81}$	,,
Bessel	,, ,, .	$\frac{1}{1047\cdot91}$	,,
Schur	,, ,, .	$\frac{1}{1047\cdot23}$	,,
Vogel (3rd and 4th Sats.)	,, ,, .	$\frac{1}{1047\cdot76}$	,,

The mean of the last four values (the two first were obtained from comparatively unsatisfactory data) is $\frac{1}{1047\cdot65}$, and this is undoubtedly not far from the true value. It will be observed that the difference between the highest and the lowest of the last four values, 0·58, is very small indeed, compared with either, or with the mean.

Taking next the estimates of Jupiter's mass from the perturbations of other bodies, we may range the principal results in the following order:—

Bouvard, from the perturbations of	Saturn,	made Jupiter's mass .	$\frac{1}{1070\cdot5}$	of the sun's.
Nicolai, ,, ,, ,,	Juno,	,, ,, .	$\frac{1}{1053\cdot92}$	,,
Encke, ,, ,, ,,	Vesta,	,, ,, .	$\frac{1}{1050\cdot36}$	,,
Schubert, ,, ,, ,,	Thalia,	,, ,, .	$\frac{1}{1048\cdot23}$	,,
Krüger ,, ,, ,,	Themis,	,, ,, .	$\frac{1}{1047\cdot54}$	,,
Hansen, ,, ,, ,,	Egeria,	,, ,, .	$\frac{1}{1051\cdot12}$	,,
Becker, ,, ,, ,,	Amphitrite,	,, ,, .	$\frac{1}{1047\cdot37}$	,,
Moller, ,, ,, ,,	Faye's Comet,	,, ,, .	$\frac{1}{1047\cdot79}$	,,
Von Asten, ,, ,, ,,	Encke's Comet	,, ,, .	$\frac{1}{1050\cdot48}$	,,

It will be observed that while the value obtained from Saturn's perturbations is decidedly too small, being farther from the mean than any value in the whole series,

is acted on by Jupiter, the perturbation of the smaller planet takes place (momentarily) around a point on the line joining the two planets, and nearer to Jupiter than to Saturn. The perturbation of a body as massive as Jupiter disturbed by him would take place (momentarily) about a point midway between the two equal bodies. It need hardly be said that the longer the arm—as it were—at the end of which perturbations of given relative amount take place, the greater the absolute perturbation. Thus is it, though in one sense two bodies perturb each other equally, that,—the orbital motion of the small asteroid is largely affected by the action of Jupiter, while the motion of Jupiter is not appreciably affected by the action of the asteroid; the orbital motion of Saturn is more affected by the action of Jupiter than the motion of Jupiter by the action of Saturn; and the orbital motions of equal planets are equally affected by their mutual perturbations.

the determination from several of the asteroids is very satisfactory—especially in those cases where the orbit of the disturbed asteroid passes relatively near to the orbit of Jupiter. The determination from Encke's comet is only fairly good; but that from Faye's comet, whose orbit passes much nearer than Encke's to Jupiter's and has already brought the comet under very powerful action by Jupiter, is very near indeed to the value which we know, by unmistakably trustworthy methods, to be correct.

(705.) From the best recent estimates of the periods of the satellites and the dimensions of their orbits, the mass of Jupiter may be taken at $\frac{1}{1047\cdot88}$ of the sun's, or 315·393 times the mass of the Earth.

(706.) The other planets which have satellites have had their masses similarly determined as follows: Saturn's is 94·384 times the Earth's, Neptune's 17,053 times, and Uranus's 14,624 times the Earth's; while the mass of Mars is but $\frac{112}{1000}$ths of the Earth's.

(707.) The determination of the mass of Mars from the motions of his satellites came after astronomers had long learned to regard all the determinations based on his perturbative action as unsatisfactory. These determinations had shown by their differences among themselves that they could not be trusted. Of course Laplace's estimate, based on a fancy of his that the densities of the planets vary inversely as their distance from the sun, had no scientific value: it is only mentioned here, and is not to be weighed against any of the others; it indicated $\frac{1}{1846082}$ of the sun's mass for the mass of Mars. Delambre, comparing Laplace's general formulas for the Earth's perturbations with the observations made by divers Astronomers Royal at Greenwich, assigned to Mars $\frac{1}{2546320}$ of the sun's mass. Burckhardt, by a similar process, deduced the smaller value, $\frac{1}{2680337}$; and Airy reduced this value to $\frac{1}{3734600}$ of the sun's. We are not able to test the value of the method depending on the perturbation of a comet by a planet, because this method had never been applied under favourable conditions to Mars.

(708.) So soon as the satellites of Mars were discovered, their distances carefully measured, and their periods of circuit accurately timed, it became possible to determine the mass of Mars satisfactorily. The mass mentioned above, as resulting from these data, corresponds to $\frac{1}{2950000}$ of the mass of the sun—a value very much larger than Airy's estimate, but considerably smaller than the estimates of Delambre and Burckhardt, which had been generally regarded as the most trustworthy available.

(709.) The mass of Venus plays a more important part than that of Mars in the mutual perturbations of the four inner planets; and therefore we may place more reliance on the determination of her mass from the observed perturbations of the Earth (on which we necessarily place chief reliance because they have been most satisfactorily observed). Passing over the guesses of Laplace and Lagrange, based on mere fancies as to the possible variations of the planetary densities according to some law depending on planetary distances, we find that Delambre, using the observations made by Bradley and Maskelyne at Greenwich, deduced for Venus a mass equal almost exactly to the Earth's, or $\frac{1}{330500}$ of the sun's. Burckhardt obtained $\frac{1}{401211}$ and Airy $\frac{1}{401847}$ as the proportion of Venus's mass to the sun's, both from Greenwich observations. Leverrier, from researches on the motion of Mercury, obtained in 1847 the proportion $\frac{1}{390000}$. Later estimates based on more exact observations of the sun's apparent motions—that is, on more exact determinations of the Earth's real motions—and a more thorough mastery of the theory of the solar system, have tended

to reduce our estimate of the mass of Venus, which is now regarded as not more than $\frac{1}{425000}$ of the mass of the sun.

(710.) The mass of Mercury was guessed by Laplace and Lagrange to be about $\frac{1}{2000000}$ of the sun's; but their guess, based as it was on a mere fancy, had no scientific value. Judged by the very slight perturbative action Mercury exerts on Venus and the Earth, his mass appeared to lie between $\frac{1}{3000000}$ and $\frac{1}{3200000}$ of the sun's. But from the perturbation of Encke's comet in the neighbourhood of the planet it appears probable that the mass of Mercury is much smaller than this. Encke himself estimated the mass of Mercury in this way at $\frac{1}{4866571}$ of the sun's. Later observations of Encke's comet suggest a still smaller value. Probably the mass of Mercury is less than $\frac{1}{5000000}$ of the sun's. This would make the density of Mercury about one-fifth greater than the Earth's; and if this result is accepted, Mercury is the only member of the solar system whose density surpasses that of the planet on which we live.

(711.) Of the total mass of the asteroids I shall presently have more to say. It may be regarded as certainly much less than the mass of Mercury.

(712.) The masses of the chief members of the solar system are tabulated a little farther on. It may be conveniently noted here, however, that, taking the Earth's mass at 1,000, the four interior planets together weigh 1,956, the four outer planets together weigh 441,534, while the mass of the sun alone is no less than 330,500,000. The total mass of the eight chief planets—viz. 443,410—is outweighed $745\frac{1}{3}$ times by the mass of the sun. Calling the Earth's mass 1,000, the total mass of the solar system is no less than 330,943,500.

(713.) The sun's mass so enormously exceeds that of all the planets taken together, that he is capable of swaying their motion without being himself disturbed. He is not indeed quite fixed. We know from Newton's third law that whatever force the sun exerts on any planet, the planet exerts precisely the same force on him; but then he is so massive that the pull which compels a planet to circle round the sun displaces him very slightly.[1]

[1] Not, however, quite so slightly as Sir John Herschel asserts in the following oft-quoted passage:—

'If he pulls the planets, they pull him and each other; but such family struggles affect him but little. *They amuse them*,' he proceeds quaintly, '*but don't disturb him.* As all the gods in the ancient mythology hung dangling from and tugging at the golden chain which linked them to the throne of Jove, but without power to draw him from his seat, so, if all the planets were in one straight line and exerting their joint attractions, the sun—leaning a little back as it were to resist their force—would not be disturbed by a space equal to his own radius; and the fixed centre, or, as an engineer would call it, the centre of gravity of our system, would still lie far within the sun's globe.'

The distance of the centre of gravity of the whole row of bodies from the sun's centre can, of course, be easily determined with precision in the case imagined by Sir John Herschel (all the planets in one straight line on the same side of the sun). But in such an inquiry we can neglect minutiæ, and need consider only the four primary planets, while we may regard the distance of any one of these planets from the centre of gravity of the whole system as appreciably equal to the distance from the sun's centre, the difference of these distances being exceedingly small compared with either. Calling the sun's mass 1, and the distance of the centre of gravity of the sun from the sun's centre x, we find, taking moments about the common centre of gravity,

Sun's moment = Jupiter's + Saturn's + Uranus's + Neptune's, or

$$1 \times x = \frac{482,700,000}{1,048} + \frac{885,000\,000}{3,500} + \frac{1,779,830,000}{22,600} + \frac{2,788,500,000}{19,380}$$

$$\text{i.e. } x = 460,000 + 253,000 + 79,000 + 145,000$$
$$= 937,000 \text{ in round numbers.}$$

Showing that the centre of gravity of the whole solar system would, in the case supposed, lie more than half a million, more exactly 505,000 (937,000 − 432,000) miles from the sun's surface.

(714.) After the amazing superiority of the sun's mass over the combined mass of all the planets, the most remarkable feature of the distribution of the masses within the solar system is the difference between the four outer and the four inner planets of the system. The Giant Planets, as they may fairly be called, together surpass the Minor Planets nearly 226 times. This cannot but be regarded as a distinction which, of itself, indicates a difference of kind, not merely of degree.

(715.) Next in importance is the superiority of Jupiter, not only over every other planet, but over all the other planets taken together. Removing his mass, 315,393, from the total mass, 443,410, of the planets, we have left but 128,017; so that Jupiter's mass surpasses that of all the other planets together nearly $2\frac{1}{2}$ times.

(716.) Of the planets which remain after Jupiter has been counted out, Saturn, with his mass of 94,384, leaves only 33,633 to the remaining six, whose combined mass his surpasses not far from three times. Removing Neptune, 17,053, we have left but 16,580, so that Neptune's mass slightly surpasses that of all the rest of the planets together; though in reality the relation between Neptune and Uranus is more correctly indicated by saying they are nearly equal: small as their difference is, however, compared with either, it amounts to more than the whole mass of the four smaller (or, as they have been called, *terrestrial*) planets. This includes the statement that the mass of Uranus alone, 14,624, surpasses that of the four minor planets in much greater degree. We see that, taking it from 16,580, we have left but 1,956, so that Uranus surpasses in mass the whole family of terrestrial planets about $7\frac{1}{2}$ times; and Uranus, the least of the giant planets, surpasses our Earth, the chief of the smaller planets, more than $14\frac{1}{2}$ times in mass.

(717.) Within the family of the smaller planets the differences of mass are scarcely less striking, relatively, than those within the family of the giant planets. Our Earth's mass, 1,000, is more than half the combined mass, 1,956, of the four terrestrial planets; taking the mass of Venus, 778, from the 956 remaining, leaves only 178 for Mars and Mercury, whose combined mass the mass of Venus surpasses nearly $4\frac{1}{2}$ times; and Mars is nearly twice as massive as Mercury.

(718.) It appears to me that the solar system may be described as comprising five very unequal orbs—the Sun, Jupiter, Saturn, Uranus, and Neptune, each with its family of dependent bodies; the Sun's special family of attendants comprising Mercury, Venus, the Earth, and Mars. Saturn and the Sun have also each of them a system of rings, Saturn's being the complex appendage long known as the 'ring system,' the Sun's being the zone of asteroids, which we have begun lately to recognise as divisible into several

zones separated by vacant gaps resembling the divisions in the Saturnian rings.

(719.) I may consider here certain relations which, so far as I know, have not as yet been dealt with in astronomical treatises. Although the sun is supreme ruler over the whole planetary system, yet each planet has a special domain of its own, within which its immediate sway is greater than that exerted by the sun.

(720.) The boundary of the domain of each planet may be regarded as limited by a surface such that a particle placed at any point of it would be equally attracted to the sun and to the planet. It may be readily shown that this surface is spherical, the planet's centre being very nearly at the centre of the planet's spherical domain.

Fig. 205.—Determination of the boundary of a Planet's Domain.

(721.) Thus, let P be a planet, S the sun, Q a point such that a particle at Q is equally attracted to the sun at S and to the planet at P. Then, if M is the mass of the sun, m the mass of the planet, we know that

$$PQ : QS :: \sqrt{m} : \sqrt{M}$$

This at once shows that P Q is almost constant, for Q S can change very little relatively, while M and m are both constant. We shall be as near to exact accuracy as in such a matter is desirable if we take the domain of any planet of mass m, travelling at a distance D from the sun, as a sphere having its centre at D, the centre of the planet, and a radius equal to $D\sqrt{m} \div \sqrt{M}$.[1]

[1] It is not difficult, however, to determine how far the centre of the domain is displaced from the planet's centre on the side away from the sun, or to prove that the radius just mentioned is the true radius of the spherical domain. Thus, produce S Q to F, and draw Q B and Q A bisecting the supplementary angles at Q, and therefore at right angles to each other, to cut the straight line S P produced; we obtain two fixed points, A and B, both on the surface we require (*Euclid*, Book VI. props. 3 and A), and Q is a point on a semicircle upon B A as diameter (the angle B Q A being a right angle). Thus we see that the surface which limits the domain of the planet P is a sphere having B A as diameter. Further, noting that

$$\frac{PA}{AS} = \frac{\sqrt{m}}{\sqrt{M}} = \frac{PB}{BS}$$

whence

$$\frac{PA}{PS} = \frac{\sqrt{m}}{\sqrt{M} + \sqrt{m}} \text{ and } \frac{PB}{PS} = \frac{\sqrt{m}}{\sqrt{M} - \sqrt{m}}$$

we see that, representing P S, the planet's distance from the sun, by D,

$$PB - PA = \sqrt{m}\left\{\frac{1}{\sqrt{M} - \sqrt{m}} - \frac{1}{\sqrt{M} + \sqrt{m}}\right\}D$$
$$= \frac{2m}{M - m}D$$

and

$$AB = \sqrt{m}\left\{\frac{1}{\sqrt{M} - \sqrt{m}} + \frac{1}{\sqrt{M} + \sqrt{m}}\right\}D$$
$$= \frac{2\sqrt{M}\,m}{M - m}D$$

The length P B − P A may therefore be neglected, m being always small compared with M; while the radius ($\frac{1}{2}$ A B) of the planet's spherical domain may be taken equal to $\sqrt{\frac{m}{M}}\,D$ without sensible error.

It is easy from this result to calculate the radius of the spherical domain of each planet.

(722.) In the following table the domains of the planets as calculated from this formula are indicated, with several other details, some of which have not hitherto been discussed in astronomical treatises but are necessary for the due recognition of the relative importance of the various members of the solar system :—

	Distance from sun in miles	Diameter in miles	Mass	Domain within which Planet's attraction surpasses Sun's: Radius in miles	Domain within which Planet's attraction surpasses Sun's: Volume in billions of cubic miles	Orbital Velocity (miles per second) at this distance: Circular orbit	Orbital Velocity (miles per second) at this distance: Parabolic orbit	Velocities (miles per second) with which bodies supposed to be drawn from an indefinitely great distance towards each several member of the solar system, under its sole attraction, would reach the surface of the orbs so attracting them.
The Sun . .	—	865,240	330,500,000			270·52	382·57	382·57
						At sun's surface		
Mercury . .	35,915,000	3,010	66	16,060	4·144	29·690	41·988	2·901
Venus . . .	67,110,000	7,707	778	102,940	1090·870	21·720	30·717	6·218
The Earth . .	92,780,000	7,927	1,000	161,390	1200·533	18·472	26·124	6·953
Mars	141,368,000	4,247	112	82,310	557·598	14·965	21·164	3·179
The Asteroids (mean)	259,780,000	-	50		—	11·039	15·612	
Jupiter . . .	482,716,000	86,520	315,393	14,912,000	3,315,939,000	8·098	11·453	37·373
Saturn . . .	885,015,000	70,930	94,384	14,956,000	3,345,427,000	5·981	8·458	22·580
Uranus . . .	1,779,834,000	31,900	14,624	11,839,000	1,659,495,000	4·218	5·965	13·275
Neptune . .	2,788,115,000	34,700	17,053	20,030,000	8,026,290,000	3·370	4·765	13·721
The Moon . .	92,780,000	2,160	12·38	17,960	5·793	18·472	26·124	1·482

These are also the velocities with which bodies drawn to the sun from indefinitely great distances under the sole attraction, would travel when at the distances of the several planets from him.

(723.) It is interesting to notice in this table how, with increase of distance from the sun, the planetary domains widen, as compared, at least, with what the mere mass of each planet might lead us to expect. The domains of the four interior planets are indeed arranged in the order of their masses, though differently proportioned. But the domain of Saturn is greater than that of Jupiter, though Jupiter's mass is so much the larger ; the domain of Uranus is not so greatly inferior to the domain of either of the two chief giants of the solar system as from his much smaller mass we might have expected ; while the domain of Neptune is nearly equal in extent to those of all the other planets taken together.

(724.) It will be observed (see Art. 666) that the satellite-systems of Jupiter, Saturn, Uranus, and Neptune all fall far within the boundaries of the spherical domains of these several planets. They therefore have supreme sway over their satellites. It is otherwise (as indeed we have already seen) with the Earth : the moon is ruled chiefly by the sun. Mars, however, has supreme sway over his small family.

(725.) If we consider the above relations in connection with the results collected in the two columns following those relating to the planetary domains, we perceive still more clearly how distance from the sun increases the relative

power of a planet over the region around it. For instance, Jupiter has supreme rule within his domain of nearly $3\frac{1}{3}$ trillions of cubic miles—in this sense, that when matter passes into this domain from without, Jupiter sways such matter more strongly than even the chief ruler of the solar system, the central sun. We see further that (apart from influences external to our system) matter thus entering Jupiter's domain may have velocities ranging from 11·453 miles per second, the greatest the sun can impart at Jupiter's distance, to 8·098, the velocity in a circular orbit at that distance, down to velocities which may be much smaller in the case of bodies having the aphelia of their highly eccentric orbits near the orbit of Jupiter. Saturn, with a domain greater in extent, has supreme rule over bodies whose velocities, as they cross his domain, range only up to a maximum 8·458 miles per second, so far as solar influence is concerned, and amount only to 5·981 miles per second for circular orbits round the sun. It follows that, on the whole, Saturn has better opportunities than Jupiter for influencing the motions of bodies which, travelling under the sun's rule, chance to cross his domain—seeing that, *cæteris paribus*, such bodies cross that domain more slowly, and therefore remain longer under his paramount influence. Uranus, again, though he has a much smaller domain than either Jupiter or Saturn, has more time to perturb bodies passing under his influence, since their maximum velocity (so far as solar influence has been concerned) is but 5·965 miles per second within the domain of Uranus, and their velocity in circular orbits round the sun only 4·218 miles per second. And lastly, Neptune, with the widest domain of all, has sway over bodies passing into that domain with velocities ranging no higher (so far as solar influence is concerned) than 4·765 miles per second, in circular orbits only 3·370 miles per second, and much less even than this in orbits of considerable eccentricity having their aphelia near the orbit of Neptune.

(726.) The last column of the table affords an entirely different measure of the power of the several members of the solar system. It indicates the velocity which each orb would be able to communicate to a body travelling to it from rest at an indefinitely great distance under its sole influence. The body thus attracted is supposed to be very small compared with the attracting orb—a mere particle, in fact; and the velocity tabulated is that with which the attracted body would impinge upon or (in certain cases) pass through the surface of the body attracting it—meaning by the 'surface' that visible surface from which astronomers measure in determining the diameter and volume of each member of the solar system. The power of an orb, *as thus estimated*, depends not only on the absolute mass of the orb, though that alone is assumed to be at work in communicating to the approaching particle

its gradually increasing velocity of approach, but also on the density [1] of the attracting orb, since the greater the density is the smaller the orb must be, and the nearer the approaching body can approach before it reaches the bounding surface. Probably in the case of a giant planet (as we shall see hereafter), and certainly in the case of the sun, the visible surface lies far outside the real surface or region where a body approaching from without would be brought to rest. But in the case of any orb which, like the Earth, Moon, Mars, &c., has a definite solid surface, we obtain the absolute limit of the velocity which such an orb can give to other bodies by considering the velocity with which a body drawn from an indefinitely great distance under its sole influence would impinge upon that surface.

(727.) It will be obvious, from a consideration of the way in which the masses of the various members of the solar system have been determined, that—if, first any masses of any importance have been overlooked, or secondly the dimensions of the solar system have not yet been correctly determined, the observed movements within the solar system cannot accord exactly with those deduced from the estimated masses and distances. In the first case, perturbations not yet accounted for must arise, which, though they may be at present insensible, will yet, in the fulness of time, assert their influence and (probably) indicate their origin. In the second case, since any change in the estimated scale of the solar system changes our estimate of the masses of all the members of the solar system except the Earth and Moon, and even our estimate of the Moon in some degree, it is evident that our Earth's unchanging mass bears a different proportion to the masses of the other members of the solar system with every change in the estimated dimensions of that system. Thus with each such change different relative perturbative action must be attributed to the Earth with her yearly periodic motion; and as time passes the discrepancy between her annual perturbative actions and those assigned to her on a given estimate of the dimensions of the solar system cannot but be suspected, and in the fulness of time recognised.

(728.) To appreciate the bearing of such considerations as these on the future of astronomy (so far as the measuring and weighing of the solar system are concerned) we need only consider what has happened in the past.

We owe more to French astronomers than to those of any other country in this particular matter. From the time of its establishment in 1795, the French Board of Longitude has attached special importance to the construction of correct tables of the sun, moon, and planets; and although the astro-

[1] If V and v be the velocities of particles drawn (under the conditions we are considering) to the surfaces of bodies having masses M and m, radii R and r, and of uniform specific gravities Σ and S, it can be shown that

$$V^2 : v^2 :: R^2 \Sigma : r^2 S :: M\sqrt[3]{\Sigma} : m\sqrt[3]{S}.$$

nomers of France have largely trusted to observations made at Greenwich and elsewhere for their material, the investigations depending on such material have been altogether their own. The best English work in this direction has not been official work at all—nay, was only impeded and obstructed by officials when they put their hands to it. I refer, I need hardly say, to Adams's independent work in inferring the existence of Neptune from perturbations unaccounted for by the distribution of the masses within the solar system as before known. But this noteworthy achievement, which was the ultimate object of Adams's labours, was with Leverrier merely a step in a series of investigations; while that series was but a part of a much larger series initiated by the French Board of Longitude nearly a century ago, and still in progress.

(729.) Passing over the earlier labours of the French astronomers, we find Leverrier presenting to the Academy of Sciences, on December 21, 1874, a paper 'completing,' as he said, 'the *ensemble* of work, the first piece of which extends back to September 16, 1839.'

Taking first the seven chief planets known when his labours began, he set himself to inquire into their motions. He found before long that the tables hitherto in use did not accord satisfactorily with observation. If every discrepancy had had a single cause, it would then have been a work of great labour to determine each such cause; but the chief difficulty which the astronomer has to deal with in considering planetary perturbations resides in the fact that multitudinous causes are in operation, the effects of which are intermingled. The perturbations are as the waves on the troubled surface of a storm-swept ocean. Watch such a surface, and notice how every wave differs from its fellows in one respect or another, usually in many. Suppose now that the task were assigned of analysing the causes of these varieties of form. How difficult would it be to distinguish one effect from another, when so many were manifestly in operation! A sudden gust of wind blowing against the sloping side of a great wave may aid to heap up or to depress the mass of water which at the moment forms the wave, and thenceforth through many oscillations the effect of that accident will remain. A wave under observation may have been affected by many gusts, acting in various ways. Again, a wave may be increased or diminished by combining with a cross-wave belonging to another series than the first, and such causes of change may have operated over and over again. Peculiarities of the sea-bottom act to modify the shape and size of waves, and a wave observed in one place may have been affected by such peculiarities in regions many miles away from the observer's station. Thus, though the observer might find it an easy task to give a general explanation of the sea-waves before him, he would have a task

of enormous difficulty—in fact, an altogether hopeless task—if he were asked to ascertain from the varieties of form presented by the waves the peculiarities of all the modes of disturbance operative in giving to the waves their actual forms. Somewhat similar, though not altogether hopeless, as will soon appear, is the task of the astronomer called upon to assign to their several causes, *not* the observed perturbations—*that* would correspond only to explaining the general nature of the wave-motion—but the peculiarities recognised in these perturbations, the various ways in which these differ from what may be described as their normal character.

(730.) In this inquiry, the astronomer has not merely to consider the motion of the Earth in her orbit round the sun, and the disturbances which affect that motion, but to regard these as the chief factors in his analysis. The Earth is riding on the waves of perturbation. Her movement on these waves must be most carefully considered ; for not merely will that movement indicate directly the nature of those waves which particularly affect herself, but also, unless that movement is taken into account, the Earth-borne observer will form an incorrect estimate of the waves by which the other planets are perturbed.

(731.) To this work, then, of determining exactly the characteristics of the Earth's motion round the sun, Leverrier from the very outset of his inquiry devoted close attention. For this purpose it was necessary to study very carefully the sun's apparent motion from day to day, for this motion precisely corresponds with the real motion of the Earth. It will give some idea of the extent of Leverrier's field of research, though but a faint idea of the nature of his work therein, to mention that, in dealing only with this one part of his subject, he reviewed and discussed nine thousand distinct observations of the sun, made since Bradley's time at Greenwich, Paris, and Königsberg.

(732.) The first result which attracted Leverrier's attention was rather unsatisfactory. It is commonly supposed that the observations of the sun at those three observatories, and especially at Greenwich, have been so exceedingly precise as to leave nothing to be desired on that score. Bessel, of Königsberg, was led to remark, many years since, with some degree of surprise, that the theory of the sun (or, which is the same thing, the theory of the Earth's motion) had not made the progress which might have been expected from so many and such accurate observations. Leverrier's opinion, which must be accepted as final, owing to the enormous number of observations he has examined and his unsurpassed skill as a mathematician, is very different. 'Our conclusion is,' he says, 'that the observations of the sun leave much to be desired, on account of systematic errors affecting them; and

there is no discordance between theory and observation which cannot be attributed to errors in observing.'

(733.) Yet Leverrier dealt so successfully with these observations, though thus imperfect, that he early deduced from them a noteworthy result. We have seen (Art. 690) that in every lunar month the Earth circuits around the common centre of gravity of her mass and the moon's. The diameter of this monthly orbit amounts to about six thousand miles, and as a result of this motion she is about three thousand miles in advance of the centre of gravity just named when the moon is in her first quarter, and as far behind when the moon is in her third quarter. It is that centre of gravity which alone follows the true orbit around the sun commonly attributed to the Earth herself. The two bodies circle round their common centre of gravity while the pair are swung round the sun. This peculiarity of the Earth's real motion is reflected in the sun's apparent motion. He seems at the time of the moon's first quarter to be in advance, and at the time of her third quarter to be behind, his mean place. But it is clear that, if we can tell how large this apparent monthly orbit looks as seen from the Earth, we shall know how far off the sun is. For the real size of this orbit is a matter depending only on the Earth and moon, and can be inferred independently of the sun's distance—if we can independently determine the moon's mass. Knowing how large the path really is, and how much the sun seems displaced in traversing it, we have learned the apparent span presented by a range of six thousand miles at the sun's distance. This is equivalent to determining the sun's distance. Accordingly, Leverrier, having carefully estimated the sun's apparent monthly displacements, deduced thence an estimate of the distance of the sun, and announced in 1865 that the accepted estimate of the sun's distance was too large by between three and four millions of miles.

(734.) Before this, however (though there are reasons for setting this result first), Leverrier recognised that the system of seven great planets was incomplete. He found that some planet, as yet unseen and unknown, travelling beyond the path of Uranus, disturbs by its attraction the movements of that planet, which for sixty years had been regarded as the remotest member of the sun's family.

(735.) Leverrier first satisfied himself by the most careful analysis of all available observations that Uranus really is disturbed by an unknown body (and in passing we may remark that in this respect Leverrier's work differed from that of Adams, who assumed this particular point). It luckily happened that at this time the displacement of Uranus had reached its maximum, and was beginning slowly to decrease. This showed that the disturbing planet had made its nearest approach to Uranus, and was now slowly drawing

away.[1] Thus, though the hypothetical Neptunes of Adams and Leverrier moved quite differently from each other, and departed still more widely from the path of the real Neptune, yet under the actual conditions both astronomers were led to point to a place very near to that occupied by the real Neptune at that particular time. The planet was found almost at the first cast of the telescopic line.

(736.) In passing to the next result of Leverrier's researches, we have to turn from the outermost planets of the solar system to the innermost. The motions of Mercury have been determined with a great degree of accuracy, because Mercury often passes across the face of the sun, and can at those times be observed very exactly. Leverrier found that the observed movements of this planet did not accord with those calculated, 'a result,' he quaintly said, which 'naturally filled us with inquietude. Had we not allowed some error in the theory to escape us? New researches, in which every circumstance was taken into account by different methods, ended only in the conclusion that the theory was correct, but that it did not agree with the observations. Many years passed, and it was only in 1859 that we succeeded in unravelling the cause of the peculiarities recognised.' They are all included under a simple law—the mean motion of the perihelion of the orbit of Mercury is more rapid than the known masses affecting the movements of Mercury will account for; but the motion of the nodes of Mercury's orbit agrees well with the known perturbing influences. The unexplained part of the motion of the perihelion cannot be accounted for by any assumption as to an increased relative mass for the sun, were such assumption for other reasons admissible. The agreement of the node's motions with theory precludes the supposition that unknown masses are travelling anywhere except near the plane of the orbit of Mercury. Hence, we must conclude that, as Leverrier says, 'there exists in the neighbourhood of Mercury, doubtless between that planet and the sun' [though this is not certain, and quite probably part of the disturbing matter may be outside the orbit of Mercury], 'some matter as yet undiscovered. Does it consist,' he proceeds to

[1] When Adams and Leverrier began to angle for the unknown planet, it had become quite certain that that body had been lately in conjunction with Uranus. If these astronomers had not known when this happened within a few years either way, they would have found it a very difficult task to calculate the orbit and movements of Neptune by mathematically analysing the disturbance affecting the movements of Uranus. They were fortunate in this, that the conjunction had opportunely occurred at about the time when the motions of Uranus were sufficiently observed to satisfy astronomers that there was an external planet. The evidence was striking enough, and had been available long enough, to have enabled astronomers, had they been as zealous as they should have been, to have discovered Neptune at least ten years earlier. The discovery of Neptune was a triumph for the Newtonian theory; but it can scarcely be regarded by those acquainted with the circumstances as a triumph for astronomy. Indeed, it involved official astronomy in a certain degree of discredit. This will be shown in the chapter on Neptune.

inquire, 'of one or more small planets, or other more minute asteroids, or even of cosmical dust? Theory tells us nothing on this point. On numerous occasions trustworthy observers have declared that they have witnessed the passage of a small planet over the sun; but nothing has been established in this matter.'

(737.) Such were Leverrier's latest utterances on this interesting question. He took no notice, on the one hand, of the discoveries which had been recently effected in meteoric astronomy, demonstrating the existence of at least some matter in the sun's neighbourhood. Nor did he speak of the statements made by Dr. Lescarbault, which he had himself once to some degree sanctioned, respecting the transit of a small black disc across the face of the sun on March 26, in the very year, 1859, when Leverrier first laid his results respecting Mercury before the scientific world. At present, it is accepted as to all intents and purposes demonstrated that there are no planets nearer to the sun than Mercury. Probably it is to the zodiacal matter existing in the sun's neighbourhood that the outstanding perturbation of Mercury is to be attributed. If so, the character of the perturbation as above described, shows (1) that the density of distribution of the cosmical particles forming the Zodiacal is much greater near the sun than at a distance much exceeding Mercury's, and (2) that the mean plane of the Zodiacal's disc is nearly coincident in position with the plane of the orbit of Mercury.

(738.) Leverrier's analysis of the movements of Venus brought to light evidence tending to show that the sun's distance had been overestimated.[1]

(739.) We come, finally, to Mars, for the planets Jupiter and Saturn follow the motions which theory ascribes to them without any recognisable error :—

Leverrier's discussion of the motions of Mars showed how by means of mathematical analysis we can deal with matter apart from all direct evidence as to its existence. Suppose no telescopic search had been made for the planet which astronomers of old time supposed to be travelling between the paths of Mars and Jupiter. Leverrier's analysis of the motions of Mars would in that case afford decisive evidence of the question whether a large but as yet undetected planet is really travelling in that region or not. It shows that there can be no such planet, simply because Mars shows no traces of the disturbing influence of any considerable planet. But Mars does show

[1] An interesting confirmation of the accuracy of Leverrier's theory of Venus is that it enabled him to form much more accurate tables of the planet's motion than had before been in use. For instance, Dr. Hind, the superintendent of the *Nautical Almanac*, found that, using the old tables of the sun and Venus to calculate the time of the egress of Venus from the sun's face during the transit of 1874, there was an error of 13½ minutes as compared with the observed time; whereas when Leverrier's new tables were used the calculated time was only five seconds in error.

the influence of disturbing matter, not giving him a strong pull in this direction at one time and in that direction at another, as a single planet would, but exerting a more equally distributed action. This is the influence of the zone of asteroids, and in this action we have a means of weighing that zone.

(740.) But here a difficulty arises. Leverrier long since pointed out that the peculiar form of disturbance thus affecting Mars might be explained either by ascribing to the whole family of asteroids, when taken together, a weight equal to one-eighth of the Earth's, or else by adding so much to the estimate of the Earth's weight. This last result corresponds almost exactly with the effect of decreasing the estimate of the sun's distance to the degree indicated by Leverrier's other researches.[1] But then, if we ascribe the whole effect to the original erroneous estimate of the sun's distance, we are left in this predicament—that we can assign *no mass at all* to the whole family of asteroids.

(741.) Leverrier seems to have favoured this view, since he regarded the diminished distance of the sun suggested by his study of the motions of Venus as the most probable value. Yet this interpretation of his researches was on the face of it altogether improbable: as actually expressed in the last article absolutely impossible. Now that we know the sun's distance had been reduced in too great degree by Leverrier, we may take the more probable estimate resulting from recent inquiries, and infer thence what mass may be assigned to the zone of asteroids. Reducing the sun's mass from 355,000 times the Earth's (the old estimate) to 316,000 times the Earth's (Hansen's and Leverrier's estimate), the motions of Mars could be explained without taking the asteroids into account at all. The best modern measurements of the sun's distance seem to show that we cannot reduce the sun's mass below 330,000 times the Earth's. This corresponds to an increase of the Earth's mass in the proportion of 355 to 330 (or 71 to 66) instead of 9 to 8 (or $74\frac{1}{4}$ to 66), which is about the increase required to explain (in this way

[1] Some of our text-books, with a happy freedom of manner, combine these two results (stated by Leverrier in 1861), and assign to the asteroids a total mass equal to one-eighth part of the Earth's, while *also* asserting that Leverrier's researches on Mars, like those on Venus, proved that the Earth's mass must be increased by an eighth. But we cannot assign the observed effects fully to both causes at once, though we may assign part of the observed effects to one cause and part to the other. Leverrier's words are as follows: 'Only two hypotheses were possible, as we explained on June 3, 1861: either the hitherto neglected matter resides in the totality of the ring of small planets, or else it must be added to the Earth itself. In the second case, and as a consequence, the distance of the sun must be diminished by about a twenty-fourth part of the' [then] 'received value—that is, we are led to the result already obtained from the theories of the sun and Venus.' He does not seem to have thought it necessary to explain that neither explanation is likely to present the whole truth. The asteroids must have some mass, and it is quite reasonable to suppose that their mass is not a very minute fraction of the Earth's. On the other hand, the old estimate of the sun's distance certainly has to be diminished in some degree, so that a part, and possibly a considerable part, of the discrepancy must be assigned to this cause.

alone) the motion of the perihelion of Mars. Hence the perturbation remaining unexplained, or $\frac{13}{264}$ of the whole, must be attributed to the influence of the zone of asteroids ; and we shall not be far from the truth in assigning to that zone a total mass equal to about $\frac{13}{264}$, or say one-twentieth, of the Earth's mass.

(742.) It is obvious that, if we can ascertain by independent observations precisely how much must be added to the old estimate of the Earth's weight (or, which is exactly the same thing, how much must be taken from the old estimate of the sun's distance), we shall know how much is left, on the one hand for intra-Mercurial matter, and on the other for the asteroidal family. It is somewhat strange that, this being so—Leverrier's own results pointing to the importance of the direct measurement of the sun's distance by transit observations, or in any other available manner—he nevertheless spoke quite disdainfully of all direct methods of measurement. Because in weighing the planets in his analytical balance, poised and adjusted with marvellous skill, he found clear evidence that the old measurements of the sun's distance were erroneous, he deprecated new measurements! 'Here I have,' he said in effect, 'a way of testing such measurements so delicate that in itself it is preferable to them all. The balance I have used is one which will improve with advancing years ; and as in 1861 it had detected the error in measurements of the sun's distance effected in 1769, so it will determine the sun's distance much more accurately than any of the other methods which have been applied at the cost of so much labour and expense.' This is well ; but Leverrier's own results leave something to be accounted for, which (at present) direct measurements of the sun's distance are alone competent to explain satisfactorily.

(743.) But this in no sense affects the value of Leverrier's own labours. Beyond question he deduced from the observed motions of the planets all that at present can be deduced as to the masses of the different known and unknown parts of that complex system—containing bodies of all orders of size, density, and structure—which occupies the domain of space ruled over by the sun.

CHAPTER VI.

THE SUN.

(744.) The sun, recognised for ages as the source of light and heat, and therefore of all forms of life upon this Earth, worshipped in the youth of every race and nation as a god—in such sort that the religion of every known race and nation bears to this day the traces of the old ideas of the sun-worshippers—appears to the eye as a glowing circular disc, whose diameter has an apparent span of rather more than half a degree. More exactly, the sun's apparent diameter measures $\frac{8}{15}$ths of a degree, about $\frac{1}{169}$th of the distance from the horizon to the zenith, or about $\frac{1}{676}$th of the horizon's circuit.

(745.) The real size of the sun can be inferred from his apparent size only when his distance is known.[1] The ancient astronomers, though they discovered that the sun lies much farther away than the moon, and is a much larger globe than the Earth (for they had a fair notion of the size both of the Earth and of the moon), had very inadequate ideas of the sun's real size. The notion which Greek philosophers found surprising, that the sun may be as large as the Peloponnesus, suggested an idea of the sun's dimensions far short of what was positively known to Hipparchus and Ptolemy, and probably still farther within the estimates of Chaldean and Egyptian astronomers. But even these would have learned with surprise that the sun lies at such a distance as 92,780,000 miles, and so has a diameter of more than 865,000 miles, or about $109\frac{1}{3}$ times the diameter of the Earth.

(746.) And indeed, though we know certainly that our sun lies at this vast distance and has these enormous dimensions, we are quite unable to conceive distances and dimensions of such orders. Nor do we gain much, so far as clearness of conception is concerned, when we translate the sun's distance into time-measured velocities; since either we must speak of periods of time beyond our power of imagining (as they really are) or else we must speak of inconceivable velocities. Thus, if we speak of 60 miles per hour or 1,440 miles

[1] An apparent span of 32′ indicates that the object presenting that span has a real span equal to less than $\frac{1}{107}$ (about $\frac{100}{10743}$) of its distance, whether that object be the sun or the moon or a distant balloon or the dome of a building or a ball of any sort close at hand.

per day, we have indeed a velocity we can appreciate, since an express train often attains it; and we can, in a sense, appreciate distances traversed with this velocity in short periods of time, as a few hours or even days. But this velocity continued for $176\frac{2}{5}$ years, the time necessary to traverse the sun's distance, we are utterly unable to conceive, because no man can form a correct idea of the duration of his own life, or even of twenty or thirty years of it, far less of a period of time ranging over six generations. If we take more rapid motions, as that of a cannon-ball, we get more manageable periods of time. Nine years, for example, would suffice to carry a body from the Earth to the sun with the motion which can now be given to a cannon-ball as it leaves the cannon's mouth; but the velocity of a ball so moving is far too great for us to be able to conceive it, since it signifies motion which the eye cannot follow. Sound, as it travels in air, would take more than fourteen years in reaching the sun—a thought which would have been rather startling to the sun-worshippers, who raised their voices in prayer to their glowing god. Sensation travels along the nerves of the human body at an estimated rate by which the sun's distance would be traversed in about 130 years.

(747.) Even light, though, as we have seen, it travels at the rate of 186,500 miles in a second, yet takes 8 mins. $17\frac{1}{2}$ secs. in traversing the distance which separates us from the sun. It follows that we do not see him as he is at any moment, but as he was about $8\frac{1}{4}$ minutes before. We do not even see the sun where he actually is, but, since our Earth's motion of revolution is referred to a motion of the sun from west to east, we see the sun apparently to the west of his real position by a distance equal to that which the Earth has traversed in $497\frac{1}{2}$ secs., or by 9,190 miles. This, at the sun's distance, is but a small apparent displacement, being only $\frac{1}{94}$th part of the sun's apparent diameter. But by this distance the eye must be directed east of the sun's apparent centre (along the ecliptic) to give the true direction of the sun's centre at the moment.[1]

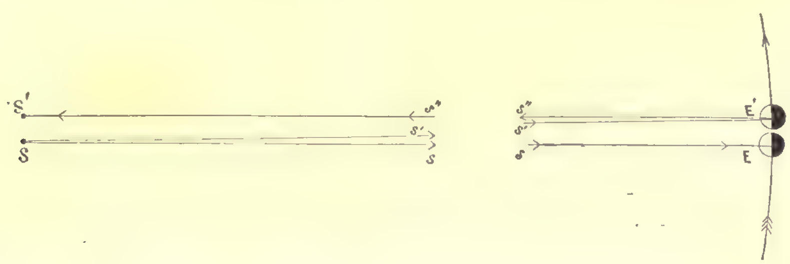


FIG. 206.—Illustrating the apparent displacement (or aberration) of the Sun, due to the measurable velocity of light.

[1] Let S, fig. 206, be the centre of the sun (whose disc on the scale of this picture would be greater than the page would contain), E the Earth at any moment, S E the course of light rays emitted from the sun towards the Earth at E [the real distance S E being so great that in the picture an

(748.) It is as impossible to conceive the sun's size as his distance; and perhaps in the former case the contrast between what we have learned and what we seem to see is even more striking than in the latter. The sun looks as though he were not more than a yard at the utmost in diameter. But to traverse his diameter a point moving at the rate of 60 miles an hour would require more than 600 days. To complete the circuit of the sun's globe, which amounts to 2,718,200 miles, at this rate of a mile per minute, would require 1,885 days, or more than five years. The sun exceeds our Earth about 1,305,000 times in volume, and about 11,940 times in surface.

(749.) Such is the size and surface of the sun as judged from his apparent disc. We shall see presently that he is at once larger and smaller than this: much larger if the extent of all his appendages is taken into account; much smaller if the dimensions of his real globe are considered. But, in whatever way we estimate his volume, the sun is a giant in size. His globe is compared with the Earth's in fig. 207 (from my 'Lessons in Elementary Astronomy,' 1870).[1]

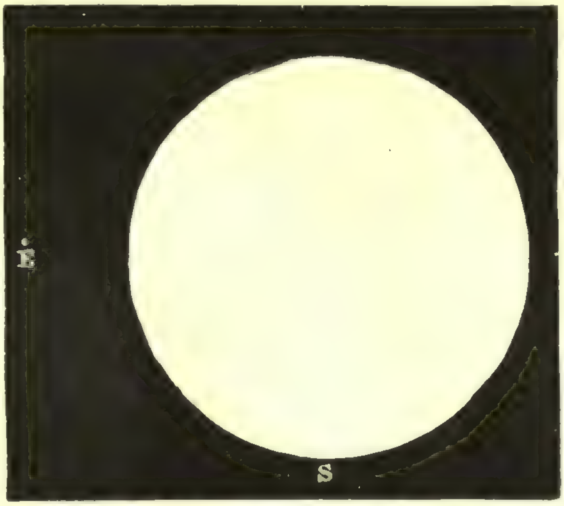


FIG. 207.—Comparative dimensions of the Sun and Earth.

(750.) Fig. 208 presents the sun's globe as a white disc, on which are shown the several members of his system as black discs, the terrestrial planets on the right, the giant planets on the left, and the asteroids between. These, however, are too small to be correctly shown on the scale of fig. 208. The planets are arranged in the order of their distance from the sun from Mercury on the extreme left to Neptune on the extreme right.

immense gap must be supposed to intervene between the points *ss*″ and *ss*″]. Then these rays fail altogether to reach the Earth, since when they arrive at E the Earth has passed on to E′, 9,190 miles from E. But the Earth at E′ receives the rays which had been emitted 8 mins. 17 secs. before in the direction S E′. The velocity of light so arriving, combined with the much smaller velocity of the Earth in her orbit, causes the light to seem to reach the Earth in the direction S′ E′—or the sun's centre seems to lie in the direction E′ S′—the angle S′ E′ S being measured by the ratio of the arc E E′ traversed by the Earth in 8 mins. 17 secs. to the radial distance S E traversed by the rays of light in the same time. This angle, as we have already seen, is one of 20″·45 (Art. 653).

[1] This is the first of some ten or twelve figures which I have occasion to use in the present volume, whereof electros seem to have been taken without my sanction before the stock of my *Elementary Astronomy* was transferred to Messrs. Longmans. It is a matter of slight importance in itself; but my blocks (or electros from them) have been used in a work on Astronomy recently issued, without any acknowledgment, and I am obliged to protect myself from the suspicion of being the borrower, when I alone had the right of lending.

(751.) Mass is a more important quality in the comparison of the celestial orbs than size, though size appeals more directly to the mind. For the mass of each orb determines its power—one may almost say its vitality, since, by virtue of the power of attraction, matter, which men fondly call inert, is in reality the source of every form of motion, and so, directly or indirectly, of every form of life.

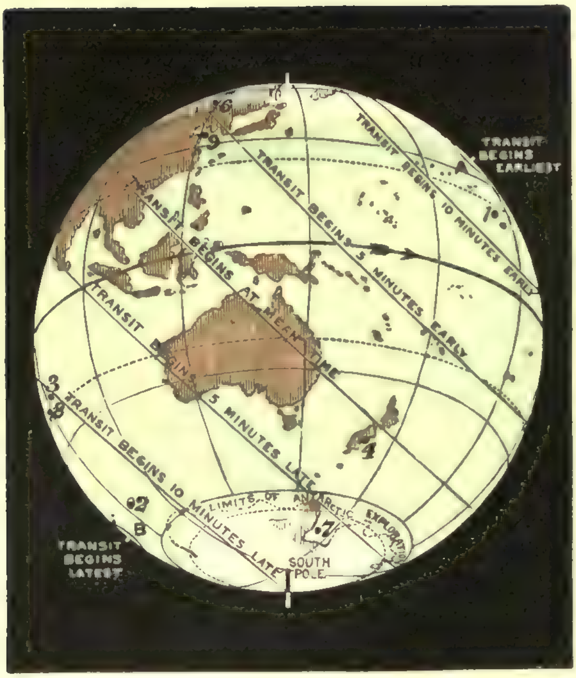
(752.) The sun does not surpass the Earth so many times in mass as in size. His orb, which is 1,305,200 times as large as the Earth's, contains but

FIG. 208.—The Sun and his family of Planets, with their Satellites.

330,500 times the Earth's quantity of matter. If we regard his apparent disc as indicating his real orb, we should infer that his mean density is little more than one fourth of the Earth's (the exact value would be ·2532 where the Earth's mean density is 1). But we shall see as we proceed that probably the real orb of the sun—the working orb, as it might be called—is much smaller than the globe we see and measure.

(753.) The attractive might represented by the sun's mass may best be shown by considering what would be the force with which bodies would be drawn to the Earth's surface were her mass equal to the sun's, her size being

PLATE XI.

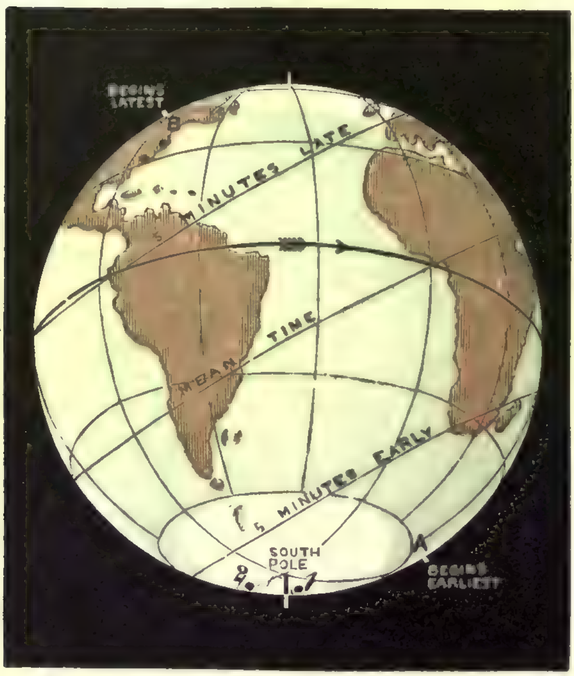


SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1874.

1. Hawaii
2. Kerguelen Land
3. Rodriguez
4. New Zealand
6. Nertschinsk
7. Possession I.
8. Mauritius
9. Station in North China

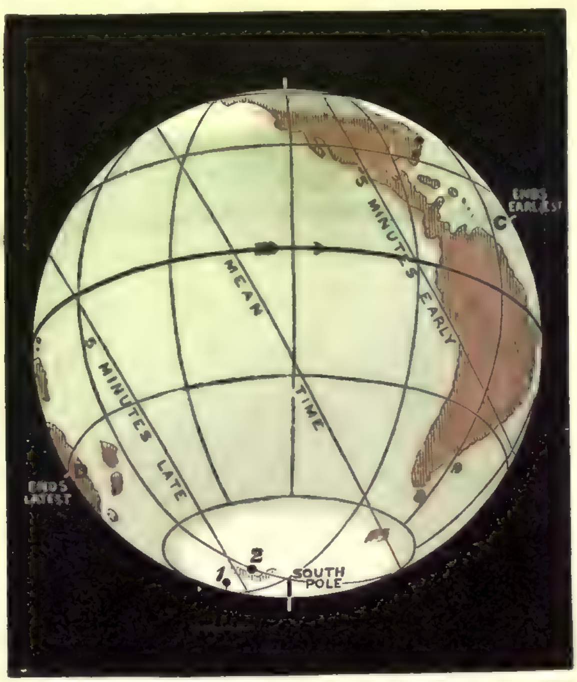
SUN-VIEW OF THE EARTH AT THE END OF THE TRANSIT OF 1874.

2. Kerguelen Land
3. Rodriguez
4. New Zealand.
5. Alexandria
6. Nertschinsk
7. Possession I.
8. Mauritius
9. North China
10. North India



SUN-VIEW OF THE EARTH AT THE BEGINNING OF THE TRANSIT OF 1882.

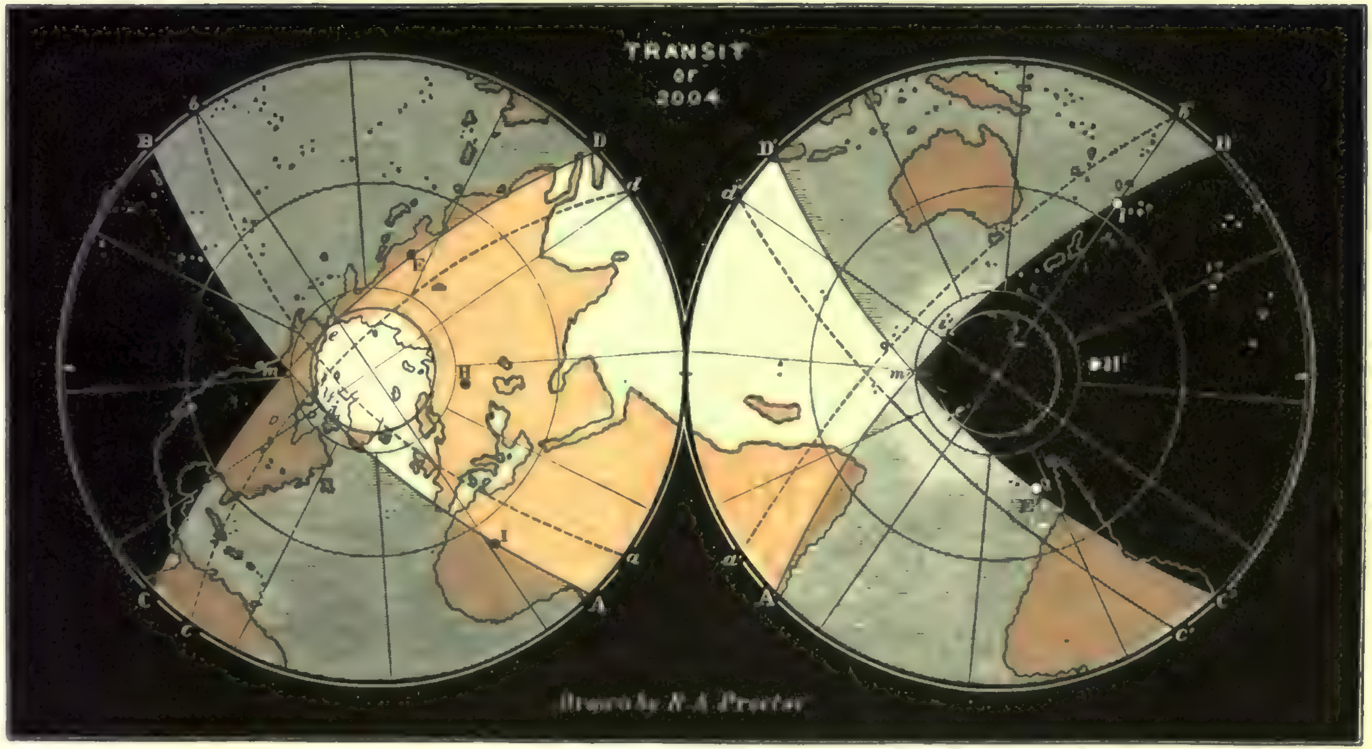
1. Repulse Bay.
2. Possession Island.



SUN-VIEW OF THE EARTH AT THE END OF THE TRANSIT OF 1882.

1. Repulse Bay.
2. Possession Island.

PLATE XII.



SHOWING WHERE THE TRANSIT OF 2004 WILL BE WHOLLY OR PARTIALLY VISIBLE.

A B and A′ B′ separate sunlit and darkened hemispheres at ingress; along *a b*, *a′ b′*, sun 10° high at ingress.
C D and C′ D′ separate sunlit and darkened hemispheres at egress; along *c d*, *c′ d′*, sun 10° high at egress.
I, I′, are the Delislean poles for ingress; E, E′, those for egress; H, H′, the Halleyan poles.

unchanged ; in other words, by supposing her mean density increased 330,500 times. In this case, the quantity of matter in a one-ounce weight would be drawn downwards with as much force as a ten-ton weight on the actual surface of the Earth. In falling through a single inch a body would acquire a velocity of about 200 miles per hour. So that, were our Earth as massive as the sun, with her present size, a one-ounce weight drawn down from rest one inch would acquire all the energy which resides in a ten-ton mass rushing along at a rate four times greater than that of an express train ; an energy which, converted into heat, would represent a considerable amount of life-supporting power.

(754.) At the sun's visible surface gravity is much less than this, because that surface is more than 109 times farther from the sun's centre than is the Earth's surface from her centre. Yet even at his visible surface solar gravity exceeds terrestrial gravity at the Earth's surface $27\frac{2}{3}$ (or more exactly 27·673) times.

(755.) At the Earth's distance, solar gravity is much smaller than terrestrial gravity, the reduction of the force as the square of the distance being much more than sufficient to compensate for the great superiority of solar over terrestrial gravity at equal distances. A mass weighing 1 lb. at the Earth's surface is drawn towards the sun with a force corresponding to the weight of only $\frac{6}{10000}$ths of a pound, terrestrial gravity at the Earth's surface exceeding solar gravity at the Earth's distance nearly 1,665 times.

(756.) We are led to inquire here how long the pull of the sun on the Earth takes to traverse the distance separating the two bodies. We have seen that the sun's light requires 8 mins. 17 secs. Gravity ranges much more swiftly over the distance ; it may even be said that the velocity of the action of gravity—a mutual action, be it remembered—is infinite, seeing that computation indicates no interval of time, however short, which is occupied in the passage of the attractive influence over even the greatest of the measured distances in the solar system. The full proof of the practical instantaneity of the action of gravity over all distances, however great, cannot conveniently be presented in a popular form; but sufficient evidence may be deduced from a consideration of what has been already noted (Art. 747) in regard to light. The direction in which the sun's light seems to reach the Earth is not the true direction of a straight line joining the centres of the Earth and sun, but is inclined to this direction so as to be directed from the Earth to a point nearly 9,200 miles from the sun's centre. If gravity acting outwards from the sun[1] traversed the interplanetary spaces with the same velocity as light, the pull of gravity would not be directed towards the sun's centre (which we have seen, Art. 465, is essential to the fulfilment of Kepler's second law), but at an angle of about 20″·445 to that direction. And precisely as the rays of light reach the Earth with this slight slant directed towards her in her advance

[1] Of course the nature of gravity (a force of interaction) precludes any just comparison between the action of gravity and the emission of light or heat; but the inference as to the velocity of intercommunication is none the less sound.

(as rain falling vertically seems to meet with a slight slant, directed towards him, a swiftly advancing traveller), so the direction of the action of gravity on the Earth would be inclined with a slant in the direction of the Earth's motion. In fact, as light coming really from the sun at S, fig. 206, to the Earth at E′, appears to reach the Earth in the direction S′ E′, so would gravity really acting in the direction E′ S tend to pull the Earth in the direction E′ S′ if the velocity with which gravity ranged over distance were the same as that of light.[1]

(757.) If we imagine gravity as it really acts changed to a force acting in this way, the effect of the change would at first be slight. The Earth would still be drawn to the sun by a force not appreciably differing from solar gravity, and would only be affected by a relatively minute component of the sun's pull in the transverse direction, this pull bearing to the radial pull the same proportion that E E′ bears to E S, being therefore about $\frac{1}{10000}$ part of the sun's direct action, or $\frac{1}{16650000}$ of terrestrial gravity (see Art. 755). Thus in one second this transverse action would communicate to the Earth an increase of velocity in her orbit amounting only to $\frac{1}{16650000}$ of 32·2 feet, or about half a millionth of a foot per second. But the extra advance made by the Earth in her orbit on account of this transverse pull would increase as the square of the time, and be very quickly rendered sensible; while even the increase of velocity, though it would be but about a foot per second in six days, and about twenty-one yards per second in a year, would amount in eighty-four years to one mile per second, or in the life of an octogenarian to a twentieth part of the Earth's present velocity in her orbit. This would involve a measurable change in the orbit of the Earth.[2] And manifestly, in the long periods of time belonging to the history of astronomy, and still more in the vast cycles belonging to the history of a solar system, the orbits of the planets would all be entirely changed, had solar gravity no greater rapidity of action than have light and heat on their course outwards from the sun's orb. The whole mechanism of the solar system would go astray and be ruined by such a change.

Even with much greater velocity than light, the sun's action would be unable to act as solar gravity really acts in ruling the solar system. In fact, it has been shown that the observed movements of the planets can only be explained by regarding the action of gravity over all distances as practically instantaneous.

But what the real nature of that force of gravity may be which thus acts over vast distances in times immeasurably short science has as yet no inkling, perhaps may never have any.[3]

[1] The action of gravity is in itself so utterly inexplicable that any attempt to deal with it in the way of comparison with radiant forces, whose manner of action is known, must be to some degree delusive. The Earth comes into regions traversed by the sun's light and heat independently of the Earth's being there; but the pull of gravity on the Earth depends as much on the Earth as on the sun. In the imaginary case considered in the text we must imagine the pull of the Earth (exerted on the Earth when at E, but arriving when tho Earth is at E′), communicated to the Earth in this new position with unchanged direction, that is, in the direction E′ S′.

[2] It is worthy of notice, and affords a striking illustration of the mistakes into which, at a first view, we are apt to fall in dealing with such problems, that if the Earth were thus continually hastened on her course by a tangential force, she would continually travel more and more slowly, the sun's direct action in constantly tending to retard the Earth, thus hastening on her course, having greater effect in the long run than the pull tending to accelerate the Earth's motion.

[3] It has been suggested (as a way of escape from the absolutely inexplicable) that the transmission of etherial waves due to movements in the direction of wave-transmission may be conceived as the cause of that tendency of material particles to approach each other which we call 'gravity.' The

(758.) The action of gravity being mutual in such sort that each planet pulls the sun as strongly as the sun pulls the planet, it follows that whatever motions or changes of motion take place among the planets on account of the sun's action, corresponding motions and changes of motion take place in the sun's own mass. It is only because his mass is so great, surpassing the combined mass of all the planets, as we have seen (Art. 712), $745\frac{1}{3}$ times, that these movements affect the sun's position but little; for a small motion of his very large mass corresponds to a large motion of the mass of any disturbing planet. Still the sun does move within the solar system, apart from that motion through space which the solar system shares with him; indeed his orbit, considered absolutely, has considerable extent, though it is very small compared with the orbit even of Mercury. The sun's motion in this orbit, though at times rapid (considered absolutely), is very slow compared with the motion even of Neptune.

(759.) The orbit of the sun is complex in shape, since it is compounded of the circling motions which would severally result from the action of the different planets. We may neglect the movements due to the four inner planets as insignificant in range, though of course in any exact computation they would have to be taken into account. Taking the four giant planets separately, we find from what is shown in the note to Art. 713 that the sun would describe, (1) if Jupiter alone were considered, a circle (slightly eccentric but not appreciably elliptical) round the centre of gravity of Jupiter's mass and his own, once in Jupiter's period, the radius of the orbit being about 460,000 miles; (2) considering Saturn alone, a circle round the centre of gravity of Saturn's mass and his own, once in Saturn's period, the radius of the orbit being 253,000 miles; (3) considering Uranus alone, a circle 79,000 miles in radius, round the centre of gravity of Uranus's mass and his own, once in Uranus's period; and (4) considering only Neptune, a circle 145,000 miles in radius round the common centre of gravity of his own mass and Neptune's, in Neptune's period. The actual motion of the sun would be that compounded of these four circling motions with their different periods, and such smaller motions as would result from the disturbing actions of the several smaller planets, satellites, asteroids, &c. The curve would be exceedingly complicated even if we considered only the motions due to the four giant planets. Here we need only note that the greatest range of the sun from the common centre of gravity of the solar system can never exceed 940,000 miles, and very seldom approaches that amount.

transmission of such waves would enormously exceed in velocity the transmission of the wave-motions due to thwart vibrations which produce light and heat, somewhat in the same way that the transmission of sound-waves in water exceeds in velocity the transmission of waves along the water's surface. But we have no evidence (unless it be the evidence of gravity itself) as to the transmission of such ether-waves. We should have to imagine qualities in the ether to account for the supposed action of such waves, which though not perhaps more inexplicable than some of the qualities already attributed—*ex necessitate*—to the ether, are utterly outside all that is known respecting matter. In particular, we should have to imagine such ether-waves acting directly, not on matter as we know it, not even on the particles or molecules of such matter, but on the ultimate individual atoms—since otherwise the property of gravity in being proportional to the masses would not be explained. In this respect, however, if once the initial assumptions be admitted (and assumptions equally outside the range of physical experience must be accepted in any theory for the explanation of gravity, and have been already accepted with the theory of the ether itself), the wave-theory of gravity has a marked superiority over the theory of Le Sage, according to which gravity is due to the actual transmission of etherial particles with infinite velocity in all directions through space. This, however, is not saying much, since Le Sage's theory has long been recognised as altogether inadmissible.

(760.) The student must carefully avoid the mistake of supposing that the sun, in moving about the centre of gravity of the solar system on this complicated orbit, has the kind of motion which he would have if his whole mass were swayed about that point as around a fixed axis. Every point in his orb describes an orbit of the same size and shape, so that no differences of velocity or centrifugal tendency exist among the different parts of his mass.

(761.) Thus, suppose that the sun's globe is so moving that its centre passes from S to S′, fig. 209, part of a circular arc around G, the centre of gravity of the solar system (usually, of course, the centre would not be moving so as to keep its distance from G unchanged); then G A S B, the diameter through G at the beginning of this motion, would not be swayed to the position G *a* S′ *b* at the end of it, but to the position A′S′B′ parallel to its former position, the points A and B moving each in precisely the same way as S,—A as if round a centre at *c*, B as if round a centre at *c′* (A *c*, S G, and B *c′* being equal). Any particle as P moves in like manner along the arc P P′ round a centre as C, such that P C is equal and parallel to S G.[1] To suppose otherwise would be to attribute to the action of the planets different forces at A, at S, and at B, whereas, owing to the vastness of the planets' distances, their action is appreciably the same on all parts of the sun's mass. Such slight differences as exist do not even approach what would be required to account for the motion of the point B over the arc B *b* while S moved only over the arc S S′ and A over the still smaller arc A *a*.

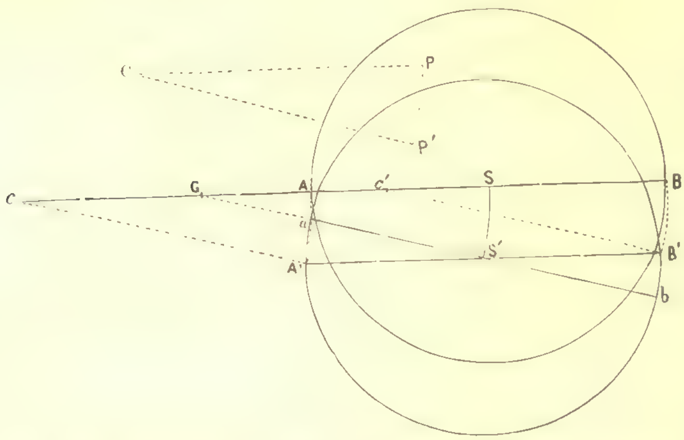


Fig. 209.—Illustrating the Sun's motion round the centre of gravity of the Solar System.

(762.) Even more important than the sun's might as a ruling orb is his beneficence as a source of light and heat :—

[1] From a misapprehension of the nature of the moon's motion round the centre of gravity of the Earth and moon, Mr. Henry Perigal and others were led to deny the moon's rotation. From a similar misapprehension of the sun's motion about the centre of gravity of the solar system, Mr. Mattieu Williams has been led to suppose that *differences* of motion exist in the sun's mass, which result in the stirring up of what he regards as the fuel of the sun. Even mathematicians have been led (through inadvertence merely, of course, and only for a short time) into a similar mistake. For instance, Professor Newcomb, in the first edition of his *Popular Astronomy*, explained the tides as due to *differences* of centrifugal tendency in parts of the Earth at different distances from the centre of gravity of the Earth and moon (a mistake which he promptly corrected in a new edition), and the precession of the equinoxes in a similarly faulty manner. (I mention the mistake only lest it should confuse readers of this work who possess the first edition of Professor Newcomb's book; for they would naturally suppose any statement by him on a matter of that nature necessarily correct, and be disturbed by finding a statement here inconsistent with his : I was indeed gravely taken to task by persons connected with Johnson's *Cyclopædia*—American—for retaining in the *American Cyclopædia* (Appleton's) the old and correct explanation of the precession of the equinoxes, when this much more readily grasped explanation was available—the only objection to the new explanation being that it is wrong.)

The distinction which we draw between light and heat is in some degree artificial. Indeed, the limitation of certain rays of the sun as light-rays relates only to human vision, not to any inherent quality distinguishing either those emissions from others or the specific quality which we call light from other qualities possessed by the self-same rays. Light is the property by which certain solar emissions affect human vision; probably other emissions which do not affect our vision affect the visual organs of other creatures, as certain tones which some men cannot hear affect the auditory organs of other men, while yet others are heard by other creatures which are not audible by human ears. And again, the emissions which possess the property we call light—that is, which affect human vision—produce also other effects, as heat and chemical action, even as waves which rock boats sway also large ships, or falling on the sea-beach roll the sands into ridges of various forms. To distinguish between heat-rays, light-rays, and chemical rays, as some do, is in reality as unphilosophical as it would be to distinguish between ship-swaying, boat-rocking, and sand-rolling waves. Yet it must be noted that while some solar emissions are chiefly effective in generating heat, others are chiefly effective in producing light, and others in developing certain chemical processes or changes—precisely as some waves sway large ships effectively but only heave small ones gently, others act most effectively in rocking small vessels, while others are chiefly active when they roll in upon the sands of the sea-shore.

(763.) As regards the light he emits, we may say that, apart from absorption, the sun is just as luminous as he looks, distance having no effect in diminishing his *brightness*, though of course it effectively diminishes the *amount* of light we receive from him. We have only to determine what the sun's actual brightness is, and to consider that degree of luminosity as belonging to his whole surface, to determine his actual emission of light.

(764.) But it is manifest that, apart from the absorptive action of our air, which is effective in diminishing the sun's brilliancy when he is not high above the horizon though slight on the sun when near the zenith, the atmosphere of the sun largely absorbs his light. For without telescopic or photometric appliances the eye readily recognises a diminution of the sun's light near the edge when he is observed under suitable conditions, as through smoked glass, or by receiving on a smooth white surface the solar image formed by admitting his rays through a distant small aperture. Making more exact measurements of the luminosity of the sun's disc, we find that it is nearly three times as great at the middle of the disc as near the edge. The proportion determined by Herr Vögel for the yellow rays is about 100 to 25.

(765.) Although we cannot from this observed relation determine the actual absorptive action of the solar atmosphere, and thence the actual emission of light before such absorptive action took place, yet, since other considerations tend to show that the absorption at the middle of the sun's disc does not greatly reduce the sun's apparent luminosity, we may safely conclude that near the edge a much larger proportion of the light is absorbed than near the middle.[1] A probable assumption, judging from the spectroscopic evidence, is that not more than one-hundredth part of the light emitted from the sun's visible surface is absorbed at the centre of the sun's disc. The observed brightness here being called 100, the real brightness would thus be about 101; and since the absorption at the edge reduces the brightness to 25, it follows that the absorption there is 101 − 25, or 76. Regarding the absorption at the edge as about seventy-six times the absorption at the middle of the disc, it would follow that the atmosphere producing the absorption is relatively shallow. Were it relatively deep, the absorption would be everywhere nearly equal; but if shallow, the absorption will be much greater near the edge than near the middle. Thus, if A B, fig. 210, represents part of the sun's surface, *a b c* the outer surface of an absorptive atmospheric envelope, the arrows indicating the direction towards which the Earth lies, the absorption at A, which will be the middle of the sun's disc for observers on the Earth, will be proportional to the line A *a*, while the absorption at the edge B will be proportional to the much longer line B *b*. That B *b* should be seventy-six times A *a*, A *a* should be about $\frac{1}{2880}$th part of A S. But we cannot deal with the problem thus simply, because it would be assuming what is impossible, viz. a homogeneous absorptive atmosphere. Let it suffice that the observed absorption of the sun's light near the edge indicates the existence of a relatively shallow and effectively absorbing atmosphere round the glowing photosphere of the sun. This atmosphere must be cooler than the surface it encloses or it would emit as much light as it absorbs, or more, and we should recognise no darkening near the edge.

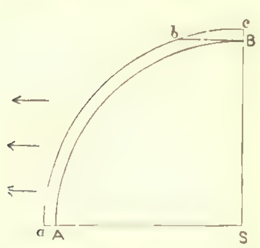


FIG. 210.—Illustrating the effect of a shallow Solar Atmosphere in absorbing the Sun's light.

(766.) The absolute or intrinsic lustre of the sun's disc near the middle is about 150 times as great as that of the glowing lime in the calcium light, and between three and four times as great as that of the glowing carbon of the electric light, when the lime light and the electric light respectively are at their greatest obtainable brilliancy.[2] Professor Langley, in 1878, comparing the light of the sun with the brilliant surface of the molten metal in a Bessemer

[1] If the absorption at the middle of the disc were large, the absorption near the edge would not seem larger in a much greater proportion. For instance, if the unabsorbed light were 500, so that a luminosity of 100 near the middle of the disc implied an absorption of 400, then a luminosity of 25 near the edge would imply an absorption of 475, which is not relatively much greater.

[2] These statements are based on the results obtained by Foucault and Fizeau in 1844. They found the sun's intrinsic lustre 146 times greater than that of the incandescent lime, and 3·4 times greater than that of the positive carbon of the electric arc. Later experiments seem to show that a brilliancy equal to half that of the sun can be given to the positive carbon, but the difference probably depends on the way in which comparison is made with the sun's light, all the methods yet employed for this purpose being indirect, and in some degree unsatisfactory.

'converter' (so much brighter than the melted iron poured into it that this looks brown by comparison), found the sun's light 5,300 times more brilliant.

(767.) An average square inch of the sun's surface gives out probably about as much light as 250 of the most powerful electric lights; whence a simple computation shows that from the sun's surface there is emitted as much light as would come from 2,500000,000000,000000,000000—that is, from two quadrillions five hundred thousand trillions of the best electric lights.[1] Measured by 'candle' power (the 'candle' being the sperm-candle, six to the pound, and burning 120 grains per hour), the sun's light has been estimated as equal to 6300,000000,000000,000000,000000 'candles.' This result, based on the estimates of Bouguer in 1725 and Wollaston in 1799, has been confirmed by later experiments. It would make sunlight equal to about 530000,000000,000000,000000 of the most powerful electric lights. Since the sun's disc appears as a circle equal in surface to one fourth the actual surface of the sun, and the light is largely absorbed near the edge of the disc, it follows that the actual amount of light emitted by the sun is considerably more than four times as great as the amount we receive as sunlight. Hence this measurement by 'candles' agrees with the estimate (in terms of electric light) for the whole surface of the sun.

(768.) This computation, so far as it relates to sunlight as actually received, does not take into account the whole absorption of the light emitted from the photosphere, only the excess of the whole absorption over what would be the absorption if the whole disc were uniformly as bright as the central parts actually are. How much light the sun would give if the atmosphere were removed cannot readily be determined. A very unsatisfactory estimate by Laplace indicated that the sun if stripped of his atmosphere would give out about twelve times as much light as he actually does; but Laplace shared the ignorance of his contemporaries in regard to many physical laws now established; and the assumptions on which he based his calculations were not merely doubtful, they were absolutely erroneous. Professor Pickering, of Harvard College, estimating the sun's light as reduced from 100 at the centre of the disc to 37 at the edge, and assuming that the absorptive atmosphere has a height equal to the sun's radius and capable of reducing the sun's light 74 per cent. at the centre of the disc, infers that were there no atmosphere the sun's light would be $4\frac{2}{3}$ times as great as now. But Herr Vögel's estimate of the reduction of the light towards the edge is probably more trustworthy; and there is no reason for supposing that the sun has an atmosphere approaching in range to the enormous height imagined by Professor Pickering. Vögel, assuming the solar atmosphere to be relatively shallow, as physical considerations and observation agree in showing, and employing the result of his own measurements of the diminution of the luminosity of the sun's disc towards the edge, concludes that the sun's light would be rather more than doubled if the atmosphere

[1] I use the words 'quadrillion' and 'trillion' here according to the English custom, which understands by a billion a million raised to the second power, by a trillion a million raised to the third power, by a quadrillion a million raised to the fourth power, and so on. This system of numeration appears far more sensible and logical than the French system (which, being non-English, our American cousins have naturally adopted), according to which a billion is a thousand millions, a trillion a million millions, a quadrillion a thousand million millions, and so on—a usage which does not in any way account for the bi-, tri-, quadri-, and other numerical relations indicated in the several names billion, trillion, quadrillion, &c. Moreover these names, which have been specially invented for the convenient expression of large numbers, often become inconveniently numerous in the expression of such numbers by the French-American method.

were removed. This result appears to be about as near the truth as any which has been yet obtained.

(769.) Vögel's observations on the luminosity of the different parts of the solar disc are the best at present available. He employed an instrument which he called the spectro-photometer, by means of which he could deal separately with the different colours of the sun's light. The following table is a summary of his results, the first column giving the distances from the sun's centre in hundredths of the radius, the others the amount of light of the various colours named at the head of the several columns in thousandths of the light at the middle of the disc:—

Distances	Violet	Blue	Green	Yellow	Red
0	1000	1000	1000	1000	1000
10	996	997	997	998	999
20	985	988	987	992	995
30	963	972	969	982	989
40	934	941	943	967	980
50	887	913	907	945	967
60	824	870	862	909	948
70	744	808	800	845	910
75	694	767	759	801	881
80	637	717	709	746	843
85	567	655	647	677	790
90	477	576	566	590	710
95	347	456	440	460	580
100	130	160	180	250	300

(770.) It follows from these observations that the violet light is more effectively absorbed than the blue, the blue more effectively than the green, the green than the yellow, and the yellow than the red—as is the case with the absorptive action of our own air.

(771.) Since smaller proportions of the light belonging to the red end of the spectrum than of light belonging to the violet end are absorbed, the light actually received from the sun is redder than it would be if the sun had no atmosphere. This indeed is obvious when a large image of the sun, formed either with or without telescopic magnifying, is received on a screen in a darkened room, for then the ruddiness of the parts near the edge as compared with the parts near the middle is a very striking feature. It is also shown in another way in photographs of the sun; for the parts of the disc near the edge are much more markedly darkened than the parts near the middle, showing how much more effectively the violet and blue rays (chiefly active in producing the chemical changes required in photography) are absorbed than the red and orange rays, which, though they give light, produce no chemical effect of that particular kind.[1]

[1] It is a mistake to speak of the blue and violet rays, or rather light-waves, as if they alone produced chemical effects. In ordinary photography certain chemical effects are sought, which are produced chiefly by the blue and violet parts of the sun's light. But there are other chemical effects which the yellow, orange, and red rays produce more readily than the green, blue, and violet; and photographs have been obtained by the action of these rays from the red end of the

(772.) If then the sun's atmosphere were removed, he would be changed in colour. The blue and violet rays now more largely absorbed would reach us in their due proportion, and would make the sun look somewhat more bluish than he actually appears. To eyes accustomed to receive actual sunlight as white light, the sunlight received then would not only be about twice as great in amount, but would be slightly tinged with bluish indigo or with greenish blue. The change, however, under the conditions imagined (which are, of course, utterly impossible, and if for an instant possible could not be maintained for a second) would be by no means so marked as some students of the sun have supposed, who have spoken of the sun as becoming violet if exposed to such a change. The alteration of tint would be very slight. A violet tint could only be produced by the entire absorption of the red, orange, and yellow rays, whereas there would be an increased emission of these, and only a somewhat greater relative increase in the emission of blue, indigo, and violet rays. The increased relative emission, moreover, since it would affect rays belonging to half the spectrum, not the violet rays alone, would not produce a violet but a bluish-indigo or a greenish-blue tint. The actual difference between the colour of the middle of the sun's disc and that of parts near the edge is much greater [1] than that between the whole present light of the sun and the light if the absorptive atmosphere were removed. Of course, if the sun's light slowly underwent a change of the kind imagined, we should remain utterly unconscious of it. We do not even recognise any change in the *colour of daylight* when the setting sun has changed in apparent colour from white to strong red. If the sun changed suddenly either from midday whiteness to the strongest redness of sunset or to full blue, the change of the colour of daylight would only be noticed for a few seconds.[2]

spectrum. It has even been found possible to employ in photography the rays beyond the red end—sometimes called the invisible rays (but incorrectly, because no rays or waves of solar emission can be properly said to be visible). Captain Abney, R.E., C.B., succeeded in photographing a kettle of boiling water in a dark room by the heat-rays which it emitted.

[1] Professor Langley, by bringing parts of the sun's edge into direct comparison with parts near the middle, has shown that the contrast is marked, as indeed might be expected from Herr Vögel's tabular statement of the relative absorptions of violet and blue light on the one hand, and of red and yellow light on the other. Yet to ordinary vision the sun's disc appears of uniform colour; and it is ordinary vision we are dealing with.

[2] The ordinary assumption has been that the absorption of the light emitted from the photosphere is due to the vapours which are present in (or rather which form) the solar atmosphere; but this can hardly be regarded as a sufficient explanation. That these vapours absorb large portions of the sun's light is proved by the dark lines in the solar spectrum, which indicate the selective absorptive action of specific vapours. And it is certain that whatever absorptive action is thus produced is not so strong for the middle of the disc as for parts near the edge. For, although in the case of some particular rays the selective absorption may be already complete near the middle of the disc, and therefore cannot be increased near the edge, yet as respects the great majority of the absorbed tints the absorption can be but partial, and must therefore be so much the greater as more of the absorbing medium is called into play—that is, as the part of the sun's surface considered lies nearer the apparent edge of the disc. Still, as the rainbow-tinted background of

(773.) The most important part of the solar radiation—for us, at any rate—is that by which the solar system is warmed: for the sun's emission of heat is the source of every form of energy existing upon this Earth, excepting only the action of the tides and those activities which are due to the Earth's internal heat. From the cyclone and the hurricane to the gentlest breeze of summer, all the movements in our air are due to the sun's heat; oceanic as well as atmospheric currents are due to the same cause; thunder and lightning, rain, snow, and hail, all forms of running water, from the vast volumes of the Nile, the Amazons, and the Missouri-Mississippi to the tiniest streamlet, from the mighty cataract to the slowly creeping brook, the glacier, the avalanche, and all forms of springs, are the products of solar energy. All orders of vegetation owe their growth to the sun's rays, not only those which clothe the Earth now, but those also which formed the forests of past ages, and now form the stores of accumulated energy existing in coal-mines and collections of natural gas and mineral oil beneath the Earth's crust. Without his life-nourishing heat no forms of animal life could exist on the Earth, in the air, or in the waters. Man himself owes not only life but all his powers—including even the powers by which civilisation has been developed and man has learned both to intensify life-activities and to multiply means of life-destruction—to the sun's amazingly profuse yet steady emission of heat. In a sense never suspected by those who worshipped the Sun as a God and regarded his annual birth at the winter solstice as the birth of the World's Saviour, the year-spring of their God, the Sun as the beneficent source of life-nourishing heat is the Saviour of the World, the veritable Life of Man, aptest (nay, the one only apt) emblem of Almighty Power, even as his Light is the aptest emblem of All-seeing Wisdom.

the spectrum itself appears darker for light obtained from near the edge than for light from near the middle of the disc, it is manifest that a portion of the absorption cannot be selective, unless we assume the existence of multitudinous absorption-lines too fine and too faint to be individually discernible. We appear forced, then, to the conclusion that a considerable part of the so-called absorption of the light emitted by the photosphere is due to the actual interposition of less luminous and non-transparent bodies. Professor Hastings, of Baltimore, has suggested that such interposed matter may be dust-like or smoke-like, an idea which does not commend itself to acceptance as probably, or even possibly, sound. But it must be remembered that at the sun's distance clouds as large as those which form in our own air would be individually invisible even with telescopes much more powerful than any yet constructed by man. Apart from any direct evidence, it is probable that such clouds must form in the lower levels of that portion of the solar atmosphere which lies outside the photosphere, and that they are at a lower temperature than the larger masses of aggregated cloud forming the photosphere itself. Such clouds would produce a very marked darkening near the edge if they were merely less brilliant than the photosphere, and a marked colouring if, being cooler, they were differently tinted. The case is akin to that of the whitish edge of the disc of Mars illustrated further on. If we prefer to regard the colouring as due to the specific absorptive action of finely divided matter suspended in the solar atmosphere, it is more reasonable to regard such matter as akin to the minute globular or vesicular aqueous particles forming mist or vapour, than to particles of dust or soot, where no such particles can conceivably exist.

(774.) The actual amount of heat poured forth by the sun is measurable by the effects produced, under carefully regulated conditions, on terrestrial substances. The description of the various methods which have been employed to measure the solar emission would here be somewhat out of place, belonging rather to the domain of physics than of astronomy. Let it suffice to state that Sir John Herschel determined the solar radiation by examining its effect in raising the temperature of water alternately exposed to the solar rays (then nearly overhead)[1] and shielded from them by an umbrella. Pouillet's method was similar in principle; but he brought the vessel containing water squarely under the sun's rays, and turned it altogether away from them, alternately. Crova used mercury. Waterston, Ericsson, Violle, Secchi, and others, noted how much the sun's direct heat will raise the temperature of a body enclosed within a space maintained at an unchanging temperature.

(775.) From the researches of Sir John Herschel it appears that the direct heat of the sun, 'if received on a surface capable of absorbing it and retaining it, would suffice to melt an inch of ice in thickness in 2 hours 13 mins.;' and he thence calculated that no less than 26,000 tons of ice would be melted per hour by the heat actually thrown on a square mile exposed at noon under the equator. This amount must be multiplied fifty million times to correspond to the heat actually received by the Earth's globe during a single hour. Pouillet obtained results not differing very greatly from these. He calculated that 2 hours 27 mins. would be required to melt a layer of ice one inch thick. Expressing his result according to a somewhat different method, Sir John Herschel calculates that if the sun's heat were distributed uniformly over the Earth's surface, 'it would in one year suffice to liquefy a layer of ice 100 feet thick, or to heat an ocean of fresh water sixty-six miles deep from the temperature of melting ice to the boiling point.' Sir John Herschel deduced 43·39 feet as the thickness of ice which the sun is capable of melting per minute, supposing the ice continually applied to the sun's surface (and the water produced by its fusion continually carried off). Pouillet deduced 38·7 feet per second. Sir John Herschel suggested, with characteristic modesty, that 40 feet may be regarded as a probable mean; but other estimates show that in all probability the solar radiation is greater than Herschel's estimate, and that the sun would be capable of melting from his surface per minute a shell of ice about 48½ inches in thickness.

(776.) This enormous annual supply of heat is but one 2,128,000,000th part of that which the sun actually radiates into space in the course of a year. All the planets of the solar system are able to intercept but about one 234-millionth part of the heat actually emitted by the sun. Sir John Herschel thus presents the enormous amount of heat deduced by increasing in the above ratio the supply actually received by the Earth from the sun:—'Supposing a cylinder of ice forty-five miles in diameter (according to recent measures, correcting both the radiation and our estimate of the sun's distance, it should be 45⅓ miles) to be continually darted into the sun *with the velocity of light*, the heat

[1] The observations were made near Cape Town on December 31, when the sun was about 12° from the zenith.

now given off constantly by radiation would then be wholly expended in its liquefaction on the one hand, while on the other the actual temperature of the sun would undergo no alteration.'

(777.) The heat emitted by the sun in a single second is as much as would come from the complete combustion of a cube of coal about 126 miles in the edge. About 16,400 millions of millions of tons of coal (anthracite) would have to be consumed per second to correspond with the solar emission of heat during that brief interval of time. The rate of emission corresponds to the consumption of about nine times as much coal per unit of surface as in the most powerful blast-furnace yet constructed by man.

(778.) The emission of heat from the sun's surface does not enable us to determine the actual mean temperature of that surface, and still less the mean temperature of the sun's real globe within the surface we see. Formerly, when Newton's law of cooling was supposed to be general, according to which the radiation is proportional to the temperature, the former temperature would have been regarded as determinable by comparing the emission of heat from the sun's visible surface with that from surfaces of known area, distance, and temperature. So estimated, the sun's temperature would be enormously high, only to be expressed in fact by millions of degrees Fahrenheit. Thus Secchi, adopting this law, estimated the sun's temperature at 18,000,000° Fahr., while Ericsson placed it at between 4,000,000° and 5,000,000° Fahr. The later estimate of Secchi, still based on improbable assumptions, indicated 250,000° Fahr. for the sun's temperature. Zöllner inferred a temperature of 100,000° Fahr.; Spörer and Lane deduced temperatures ranging from 80,000° to 50,000°. It has been shown by the experiments of MM. Dulong and Petit that Newton's law is only true when the radiant body is at a temperature not exceeding by more than 20° that of the body receiving its radiations. For very high temperatures the radiation is much greater than it would be according to Newton's law;[1] so that the radiation of great quantities of heat from an intensely hot body implies no such intensity of intrinsic heat as would be inferred from the application of Newton's law. On the other hand, however, the law according to which Dulong and Petit concluded that radiation varies with temperature seems to indicate too low a temperature for bodies emitting very large quantities of heat.[2] Pouillet deduced for the sun's temperature, by applying this law, values ranging

[1] Comparing Newton's experiments with the case of an intensely hot body like the sun, it may be said that he determined the law of cooling only for the final stage of approximation to equality of temperature as cooling proceeded, during which the hotter body parts much more slowly, relatively, with its heat than when still intensely hot. And even thus viewed his experiments were inexact, as the physics of heat were little understood in his day.

[2] The law of Dulong and Petit may be compared with Newton's law as follows (but it must be remembered that no complete account of either can here be given, or would indeed be suitable for these pages):—

If t be the temperature indicated by a thermometer exposed to the radiation of a surface which, as supposed to be seen from the thermometer-bulb, would occupy an nth part of the surrounding sphere, and τ the temperature of the surrounding medium (air in all such experiments), then, assuming the mean temperature of the radiating surface to be T, we should have—

1. According to Newton's law—

$$\mathrm{T} = \frac{1}{n}(t-\tau).$$

2. According to the law of Dulong and Petit—

$$n^t - n^\tau = (1{\cdot}0077)n^{\mathrm{T}}$$

or, using Naperian logarithms,

$$\mathrm{T} = \frac{\log(n^t - n^\tau) + \log(0{\cdot}99235)}{\log n}$$

the temperature being expressed in degrees of the centigrade thermometer.

If the surface dealt with is that of the sun $n = \frac{1}{183960}$; and according to Secchi's experiments, $t-\tau = 29{\cdot}02$. Putting $\tau = 0$ in both formulæ, we get from the former

$$\mathrm{T} = 29{\cdot}02 \times 183{,}960$$
$$= 5{,}338{,}577^\circ\ \mathrm{C}.$$

between 1,760° and 1,460° Cent., or 3,240° and 2,692° Fahr. Vicaire confirmed Pouillet's calculations, and although admitting the possibility that Pouillet's estimate might nevertheless be far too low, agreed with St. Claire Deville in assigning to the sun's visible surface a temperature below 10,000° Fahr. The law of Dulong and Petit has recently been shown by Rosetti and others to be probably inexact for very high temperatures, and Rosetti assigns 10,000° Cent., or about 18,000° Fahr., as the probable mean effective temperature of the sun's surface. Professor Langley considers this estimate far too low. Comparing the solar radiation of heat with that from the molten metal in a Bessemer converter, he finds the former at least 87, and probably more than 100 (even perhaps as much as 150) times greater—a result corresponding with the experiments of Ericsson in 1872. Moreover Professor Langley considers Ericsson underrated the temperature of the molten metal in estimating it at about 3,000° Fahr. But Langley's estimate of this temperature is doubtful, being based on the melting of platinum wire in the hot vapour of the iron, a process due partly to chemical action. Ericsson's and Langley's estimates of the solar radiation as compared with that from the metal may be accepted, but not the inferences of either physicist; for Ericsson adopted in effect Newton's law, and Langley manifests in combination with notable experimental skill a singular inability to weigh the quantitative significance of his results.[1]

but from the latter

$$T = 1{,}398° \text{ C.}$$

The difference between these results is very striking. Moreover, it is to be noted that since

$$\log n = 300, \text{ almost exactly,}$$

or

$$n^{300} = 10,$$

it follows that increasing our estimate of the solar temperature by 300° C. increases the factor n^T of the second formula tenfold, so that we may admit a wide range of corrections in the observed value of t and the estimated value of the constant multiplier (1·0077 in the formula of Dulong and Petit) without greatly increasing the deduced value of the solar temperature.

Such is the reasoning by which Vicaire and St. Claire Deville were led to adopt Pouillet's method of dealing with the solar temperature as much nearer (Vicaire said 'infinitely nearer') to the truth than Secchi's. And although the estimates of the three first-named physicists may be short of the truth, yet we may confidently accept the general inference by Vicaire that, whatever correction we may wish to apply to the estimate of about 1,400°C.—if we double it, triple it, or increase it in yet greater degree—we must admit that '*the temperature of the sun's surface is altogether comparable with the temperature of terrestrial flames.*'

[1] The mathematical interpretation of such a law as that of Dulong and Petit seems perplexing to Professor Langley, who has indeed publicly expressed his objection to mathematical methods. In this respect he shares the views of not a few, possessing like himself marked aptitude for experimental research, but seeming to become actually irritated when physical matters are dealt with in the only way in which they can be satisfactorily analysed. That mathematical methods are often applied by able mathematicians (perhaps oftenest by those ablest mathematicians who expend all their powers on the development of mathematics) in an unsatisfactory way must be admitted. As that master of common-sense reasoning, Professor Huxley, neatly said, 'Grinding peascods in a mill of super-excellent grinding qualities will not give us wheaten flour' or to that effect. But no physicist, certainly no astronomical physicist, can successfully deal with the problems which come before him, without a mastery of at least the elementary methods of mathematical analysis—as the differential and integral calculus, the treatment of differential equations, the calculus of variations, and the like. Geometrical power is also essential to really effective research; for as the corona controversy showed in 1870 and 1871, the interpretation of physical experiments and observations may be obvious to the geometrician when it is wholly missed by the very observers and experimenters themselves if not possessed of adequate geometrical *aperçu*. To complete Professor Huxley's apt illustration, the finest wheat will not give pure flour till it has been passed through the mill, and though even the worst mill will grind infinitely better meal from grain than the finest mill will grind from peascods, yet the better the mill the better the meal.

There are indeed experimenters and observers of the non-mathematical sort, who are not only unable to work raw material of their own col-

(779.) Probably a temperature of from 12,000° to 15,000° Fahr. may be most safely assumed (which is not saying much) as the mean effective temperature of the sun's surface. It is unlikely that the temperature can be below 10,000° or above 20,000° Fahr. That the mean temperature of the sun's surface is above that of our hottest reverberatory furnaces is shown by the way in which under a lens whose effect (and that not produced without absorption and consequent loss of heat) is equivalent to bringing the sun to about the moon's distance, the most stubborn terrestrial substances are melted or resolved into vapour. But this indicates the increased relative radiation at temperatures higher than those of our hottest flames, *not* that the temperature is in such high degree greater as Secchi, Ericsson, Zöllner, and Langley have supposed. The same remark applies to the spectroscopic researches by Langley, and to Soret's experiments on the penetrating power of the sun's rays, which agree in showing so high a proportion between the light-waves and the total radiation as to indicate a temperature much higher than that of the intensest artificial heat. The temperature of the sun's effective heating surface is undoubtedly much higher than that of the hottest known flame, but not in such degree as the higher estimates considered above would imply.

(780.) The heat of the sun, so far as it belongs to the same range of emission as the light, is absorbed in the same degree. The heat corresponding to radiation beyond the red end of the spectrum is also absorbed, but in less degree (as might be expected) than even the red rays, which as we have seen are less effectively absorbed than those belonging to the violet end of the spectrum. Even the total heat is less affected quantitatively by the absorption of the solar atmosphere than the light from the red end of the spectrum, as the following table shows :—

Distance from centre of sun's disc	Red Rays (Vögel)	Heat Radiation (Langley)
0	1000	1000
25	992	990
50	967	950
75	881	860
95	580	620
98	350	500

(The interpretation of the table is the same as that of the table at p. 328, Art. 769.)

(781.) Of the chemical activity of the solar rays it is not in our power to speak with so much confidence, since we have not as yet measured the sun's

lecting into the manufactured article alone worth aught, but are troubled when others, seeing the worth of that raw material, seek to obtain properly wrought products from it. The full misfortune of this way of viewing matters will be perceived when we consider that often the observing and experimenting capacity is most strongly developed in minds which want the power of calculating, weighing, and generalising—and *vice versâ* ; while, even were it otherwise, yet in these days of rapidly advancing and extending science it can be given to few men to be able to work effectively both in observational research and in the investigation of the significance of observed results.

It is because I have observed that the progress of science in nearly all departments is apt to be much hampered by such mistaken views that I have here touched on them. I do not know which fault has done more injury—the fault of basing elaborate and imposing processes of calculation and cogitation on an imperfect or insecure foundation, or that of leaving unworked the gathered products of long and careful observation.

Worse faults are, of course, not unknown, such as observing and experimenting without aim or purpose, and (at the opposite extreme) working at mathematics to develop only mathematical products. But such faults are beyond the range of criticism.

power in this respect in a way which enables us to pronounce on its real extent. We can compare the intensity of the sun's chemical action with that of terrestrial lights ; but we have not yet found a means of determining its value as compared with those forces which the chemist more ordinarily employs to produce chemical changes.

(782.) It is worthy of notice, as respects the last two forms of solar activity, how large a share of the force we derive from the sun is obtained through their action. This will be apparent when we remember the important bearing of the processes of vegetation on the wants of the human race. 'Nature,' says Mayer, 'has proposed to herself the task of storing up the light which streams earthwards from the sun—of converting the most volatile of all powers into a rigid form, and thus preserving it for her purposes. To this end she has overspread the Earth with organisms, which, living, take into them the solar light, and by the consumption of its energy incessantly generate chemical forces. These organisms are *plants. The vegetable world constitutes the reservoir in which the fugitive solar rays are fixed, suitably deposited, and rendered ready for useful application. With this process the existence of the human race is inseparably connected.*'

(783.) Among the most interesting problems in modern physics is the inquiry whence the sun derives the immense stores of heat which he is continually pouring forth, apparently in even wasteful profusion.[1] The problem cannot be regarded as solved ; yet certain general inferences of great interest have been deduced from its study :—

[1] We cannot regard as belonging to exact science the suggestion that the sun does not really pour forth in all directions the heat which we recognise as falling on the Earth. It is a favourite speculation of the ill-informed (as I know by multitudinous communications) that the sun only sends out heat where there is a body to receive his rays—or that the action, like that of gravity, is a form of interaction taking place only between bodies. On this point Professor Young, in his excellent little book on the sun, says 'all scientific investigation so far shows that this is not the case'; but the doubt suggested (perhaps not really intended) by the expression 'so far' may be dismissed definitely and absolutely. Not one of the solar rays of heat and light which fall upon the Earth (to seek no farther) set out towards the Earth, and not one of the rays setting out from the sun towards the Earth falls upon her. She comes at each moment into the course of light-waves travelling radially from the sun, each of these having originally started on a course which would have carried it more than 9,190 miles from the point on the Earth on which it actually falls, or would have taken it (since the Earth's diameter is but 7,924 miles) clear of the Earth's globe by from 9,190 to 1,266 miles. It would be as reasonable to imagine that an umbrella, swiftly carried forwards in a rainstorm, draws to it the raindrops which fall on it (though these would have fallen in front of it but for its advance, while those actually falling towards it when they were high fall behind it when nearing the ground) as to imagine that the Earth has any part in drawing from the sun the rays of light and heat which reach her surface.

The coldness of the upper air, and especially of mountain tops, has also been urged as an argument to show that the sun does not warm the Earth as a mass of heated matter warms bodies in its presence by its radiation of various orders of heat-waves. But, in reality, while the expansion of ascending currents of air, especially as they slant upwards along mountain slopes, is a cooling (and often most actively cooling) process constantly in operation, the sun's rays, though even more effective in heating bodies high above the sea-level than in heating bodies at lower levels, pass through the dry and tenuous air there without perceptibly heating it. The solar rays pass equally through dry air at the sea-level without warming it.

It is certain that the sun's heat is not due to combustion. A globe of coal, equal in mass to the sun, would have to be entirely consumed by combustion in less than 6,000 years to radiate during that time the amount of heat which the sun has been emitting during many millions of years—as the Earth's record shows us—in the past.

(784.) Were the sun simply an intensely hot mass, parting with his heat by radiation, it is certain that, at the present rate of emission, the entire mass of the sun would cool down to darkness in less than 6,000 years.

(785.) In changing from the vaporous to the liquid condition, and from the liquid condition to the solid, matter gives forth heat; restoring, in fact, the amount of heat employed in liquefying what had been solid or in vaporising what had been liquid. Changes of both kinds are constantly going on in the sun, and possibly processes of the former or heat-generating kind may be taking place so much in excess of processes of the latter or heat-employing kind as to account for a measurable proportion of the solar radiation. But even if we imagined the whole emission thus produced at the present time, only a very small proportion of the sun's emission of heat during past ages would thus be accounted for; and if the sun had no other store of heat on which to draw, he would cool to darkness in a few thousand years. The suggestion made many years since by Professor Clarke, of Cincinnati, that the sun's radiation may be in part due to the combination of the really elementary constituents of his mass into the substances which we regard as elementary, is open to the objection of being as yet unsupported by trustworthy evidence respecting the really compound nature of the so-called elements. Even if accepted, it would go but a little way towards the explanation of the emission of solar heat during millions of years in the past, or towards the indication of a store of solar heat available for any long period in the future.

(786.) The theory for a time found favour among physicists (though no astronomer accepted it) that the sun's heat is due to the impact of meteors drawn from outer space by the sun's attractive energy, and striking his surface with velocities so great as to generate intense heat. This theory was for a while accepted by Messrs. Mayer, Joule, and Sir William Thomson, who showed that the sun's growth in mass and volume, under this process of meteoric indraught, would be so slow as to be barely measurable even in tens of thousands of years. One consideration, however, which they overlooked, opposes a fatal objection to this theory, regarded at least as an attempt to account for the whole or any large portion of the sun's emission of heat. We know the outside limit of the amount of matter which we may suppose as yet free to be gathered up by the sun. Even if the whole of this were steadily flowing in year after year upon his surface, the supply, if the whole of the

sun's annual emission of heat had to be explained by it, would last but for a few decades of years. But even this supply is not wholly available in that way. For, a large proportion of the meteoric systems existing within the sun's domain travel on paths not carrying them near the surface of the sun, nor capable of being altered by any of the perturbing forces at work within the solar system into paths intersecting that surface; so that the meteors of those systems cannot at any time fall into the sun. Considering those meteoric systems only, known or unknown (nearly all unknown), which might help to maintain the sun's heat in the way imagined by Mayer and the rest, there probably is not enough material for ten years' emission of solar heat, while the actual meteoric indraught per annum probably does not generate one ten-millionth part of the sun's annual emission of heat.

(787.) Yet all the evidence tends to show that the sun's heat is due to the work done by his mass. The power of attraction possessed by what we fondly call inert matter has drawn inwards towards the ever-growing mass of the sun matter which had been strewn through immense regions of space all around him, and has steadily brought the matter so drawn in towards the centre of the mighty aggregation, immense heat being produced by these purely mechanical processes. It may be shown that, if we regard the sun's mass now as uniformly distributed through the globe limited by the visible light-surface, and as formerly uniformly distributed throughout a sphere indefinitely larger in diameter than the orbit of Neptune, the heat generated in the process of contraction would have corresponded in total amount with the emission at the present rate continued during rather less than twenty millions of years.[1]

(788.) The crust of the Earth, however, speaks unmistakably of a hundred millions of years of sun-work (for some details of the evidences chiefly gathered by Mr. Jas. Croll, see Chapter IX., and an essay, 'The Age of the Sun and Earth,' in my 'Poetry of Astronomy'). During that time, at least, the sun has poured his rays upon the ocean, raising up their waters by evaporation to be carried by winds (also products of the sun's rays) over the

[1] The total amount of heat radiated from the surface of the sun per annum amounts, according to the best recent estimates, to about $8{,}500 \times 10^{30}$ foot-pounds [the *foot-pound* being the quantity of heat necessary to raise one pound one foot against terrestrial gravity]. Now it has been shown by Helmholtz (*Phil. Mag.* vol. xi. § 4, p. 516, 1856) that the process of contraction under gravity from uniform distribution of the sun's mass throughout an indefinitely large sphere to uniform distribution through the present apparent globe of the sun, would have corresponded to a total amount of work represented in foot-pounds by the expression—

$$\frac{3}{5}\frac{r^2 M^2}{R\,m} g$$

where M is the mass of the sun, R his radius, m the mass of the Earth, r her radius, and g the fall of a body under terrestrial gravity in feet per second. Dividing the value of this expression when the proper values in feet and pounds are given to M, m, &c., by the number representing the sun's annual emission, we get 19,000,000 as about the number of years of emission corresponding to the work done during the process of contraction.

continents, and in rain and snow, river, cataract, and glacier, to do the work of denudation of which the Earth's crust gives us such abundant evidence.

(789.) Even if we reject the Earth's evidence, or if we endeavour to show that the rainfalls by which the Earth's surface has been again and again denuded were not always due to solar heat, but may have been generated by the Earth's own heat, we scarcely find our difficulty removed. For it seems utterly absurd to suppose that the mighty central orb of the solar system only attained its present activity during the comparatively recent years of the history of our Earth, one of the smaller and shorter-lived members of the sun's family. Sir W. Thomson has shown, from the observed underground temperature of our Earth, that the consolidation of the Earth's crust must have taken place not less than 20 million years nor more than 400 million years ago. Now the time when the Earth's crust was formed certainly followed by many millions of years the actual genesis of the Earth as a gaseous mass. Many physicists reject even the 400 million years given by Thomson as the superior limit, doubting whether the formulæ and data he employed can be relied upon as confidently as the various processes of mathematical calculation which he applied to them. But even if we accept his minimum result—certainly the very least which science can accept—it would still follow that the sun's present emission of light and heat could not have continued throughout the time of our Earth's existence as a planet, *if*, as appears certain, the sun's heat had its origin chiefly in those processes of contraction combined with meteoric indraught in which astronomers and physicists at present believe, *and if the space into which the sun's mass has contracted is really that which the sun we see appears to occupy.*

(790.) Mr. Croll, who passes over the latter consideration with the remark that if the sun's density increases towards the centre the supply of solar heat might be somewhat greater, suggests as an explanation of the difficulty, that the sun may have derived a portion of his energies by the collision of bodies moving originally (that is, apart from attraction) with immense velocities through space, a theory which appears too improbable to need consideration—interstellar space being so vast compared with the space occupied by bodies moving through it.

(791.) The discrepancy between the Earth's record and the evidence afforded by the sun's apparent size may be removed if we consider that the sun's size as estimated by astronomers is apparent only. The surface of glowing clouds which forms the sun's disc lies certainly thousands, and may possibly lie tens of thousands, of miles above the real surface. If the sun's real globe is very much smaller than the globe we see and measure, he has contracted in

greater degree than we had before supposed, and has therefore generated and emitted more heat.

(792.) If the sun, being thus smaller than he looks, has done much more of his life-work, he has much less left to do. The duration of the sun as a stable source of light and heat hereafter must be much shorter than we had estimated it to be when we considered his apparent size to be his real size. Where, before, we might recognise the possibility of his working as an active and beneficent sun during tens of millions of years, we can now only look, perhaps, for a few millions of years of sun-work.

(793.) Let us consider now the evidence obtained from telescopic and physical research respecting the condition and structure of that mighty central engine, that great heart, whose pulsations are the life of the solar system.

(794.) We may regard the discovery of the spots on the sun as the commencement of that long series of telescopic researches to which we owe our present knowledge of the solar orb.

(795.) It is highly probable that spots on the sun had been seen and even watched for long intervals, when as yet astronomers were not aided by the powers of the telescope. Kepler supposed that in lines 441 and 451 of Virgil's first 'Georgic' the solar spots were referred to. For 'if anyone,' he reasons, 'should refuse to see anything else than an allusion to our clouds in the words

Ille ubi nascentem maculis variaverit ortum,

I shall oppose to the interpretation this other verse:

Sin maculæ incipient rutilo immiscerier igni.'

But the latter verse is quite as applicable to clouds as the former. As regards the occasional recognition of spots by the ancients, however, there seems less room for doubt. We learn from Father Mailla that the Chinese recorded the appearance of spots on the sun in the year 321 A.D., and Acosta tells us that the natives of Peru told the Spanish invaders that the sun's face had in former times been marked with spots. In the year 807 a large spot was seen on the sun for eight successive days. Astronomers did not recognise, however, or even suspect, the cause of these phenomena. We must turn to the telescopic discovery of the spots for the real commencement of astronomical researches into the sun's physical condition.

(796.) Fabricius states ('*De Maculis in Sole observatis*,' *&c.*) that early in the year 1611, while observing the sun just after sunrise with a telescope of inconsiderable power, he noticed a black spot upon its disc which he supposed to be a terrestrial cloud. He found, however, that the object, whatever it was, belonged to the sun. As the sun rose he had to discontinue his observations, for he possessed no means of

mitigating the brilliancy of the sun's light. 'My father and I,' he says, 'passed the rest of the day and the whole night in great impatience, trying to think what this spot might be. "If it is in the sun," I said, "we shall no doubt see it again; if it is not, its motion will have carried it away from the sun's disc, and so we shall be unable to see it." On the next morning, however, to my delight, I saw the spot again. But it was not in the same place—a peculiarity which increased our perplexity. We determined to obtain an image of the sun on a sheet of paper by permitting his rays to pass through a small hole in a darkened chamber, and in this way we saw the spot quite clearly in the form of an elongated cloud. For three days we were prevented by bad weather from continuing our observations; but at the end of that period we again saw the spot, which had crossed obliquely towards the western side of the sun's disc. Another smaller one had made its appearance near the eastern edge, and in a few days this second spot reached the middle of the disc. Lastly, a third spot appeared. The three spots vanished in the order of their appearance. I was hopeful that they would be seen again, but yet perplexed by doubts and fears; however, ten days afterwards the first reappeared on the eastern side of the disc. I knew then that it had revolved completely round (the sun), and since then I have convinced myself that this is really the case.' Fabricius studied the import of his observations, and came to the conclusion that the spots are probably upon the body of the sun itself. 'We invite the students of science,' he says, 'to profit by our description; they will doubtless conclude that the sun has a motion of rotation, as Giordanus Bruno has asserted, and more lately Kepler. Indeed, I do not know what we could make of these spots on any other supposition.'

(797.) Galileo, at Florence, and Father Scheiner, a German Jesuit, discovered the sun spots independently, and investigated the laws which regulate their apparent motion. Scheiner was at first disposed to regard the spots as due to the existence of planets travelling round the sun close to its surface. Indeed for a while these imagined planets were admitted as true members of the solar family under the title of the Bourbonian stars. But Galileo pointed out that the spots move as parts of the sun's surface and show that the sun rotates on his axis in about a month. Scheiner re-examined his hypothesis, and presently admitted that Galileo was in the right. He then made a long and elaborate series of observations in order to determine the true period of rotation and the actual position of the solar axis of rotation. He published the results of these labours in his 'Rosa Ursina.' He assigned to the sun a rotation-period of between twenty-six and twenty-seven days. He also stated that the plane of the sun's equator is inclined between 6° and 8° to the plane of the ecliptic.

(798.) Scheiner, Galileo, and Hevelius would seem to have independently recognised the fact that the solar spots are not of uniform brightness, but commonly

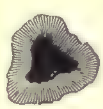


Fig. 211.—A Sun-Spot, showing the *umbra* (black) and the *penumbra* (shaded).

surrounded by a fringe less dark than the central part, as shown in fig. 211.[1] The outer fringe Hevelius called the *penumbra*, the central dark part he named the *umbra*.

[1] From my *Lessons in Elementary Astronomy* (1870); see note on p. 319.

Hevelius also recognised the existence of certain bright streaks in the neighbourhood of the spots. He called these the *faculæ* (or torchlets). These must not be confounded with the irregular tracts somewhat brighter than the general surface, which with less luminous tracts (as seen by telescopes of moderate power) produce an appearance akin to mottling. The less luminous tracts were called by Hevelius the *maculæ*, or stains.

(799.) It was noticed also by these earlier observers that the sun-spots are limited to two zones of the sun's surface, corresponding to the temperate and subtropical zones on the surface of the Earth. The position of these zones will be more particularly considered farther on. Just here it will be sufficient to present them as recognised by the earlier observers, viz. as shown in fig. 212.[1]

FIG. 212. The Spot Zones on the surface of the Sun.

(800.) A long series of observations of sun-spots now began, and many hypotheses of more or less ingenuity were put forward to account for the phenomena which they present. For some time, indeed, the possibility of their existence was earnestly denied by the students of Aristotelian philosophy. It is impossible, they gravely urged, that the Eye of the Universe should suffer from ophthalmia ; and it is related that when Scheiner communicated his discovery of the solar spots to the provincial of his order, the latter, who was an earnest Aristotelian, answered, 'I have read Aristotle's writings from beginning to end many times, and I can assure you I have nowhere found in them anything similar to what you mention ; go, therefore, my son ; tranquillise yourself ; be assured that what you take for spots in the sun are the faults of your glasses or of your eyes.'

(801.) Despite the defenders of Aristotle's infallibility, however, the progress of solar research went on. Galileo continued his labours, until, from viewing the sun so often without the dark glasses now commonly employed, he lost his eyesight. Scheiner, Hevelius, and other observers added largely to the store of known facts ; and gradually the observation of solar spots began to be recognised as a regular part of the astronomer's work.

(802.) Passing over a century and a half, during which many solar phenomena were observed but little was added to exact knowledge, we come to the observations by which Dr. Wilson, of Glasgow, in 1769–70, showed that in some cases at any rate the nucleus of a spot is at a lower level than the solar photosphere.

In November 1769, Dr. Wilson began the careful study of a sun-spot which was large enough to be visible to the naked eye. When he first examined the spot (November 22) it was situated not very far from the western edge of the sun's disc.

[1] From my *Lessons in Elementary Astronomy* (1870); see note on p. 319.

Its penumbra was equally broad all round the nucleus. Next day the penumbra was unchanged except on the side towards the centre of the disc, where it was much contracted. On the 24th the spot's outer boundary was within 24″ of the sun's western edge. The penumbra had now entirely vanished on the side towards the sun's centre. The nucleus seemed also encroached upon by the photosphere on that side.

(803.) Dr. Wilson showed that these results corresponded with those which would follow if the spot was a vast saucer-shaped opening, having the nucleus at the bottom, and the penumbra forming its sloping sides. A corresponding succession of changes occurred when the spot reappeared on the eastern edge, and thence passed across the solar disc. On December 11 the spot was distant about $1\frac{1}{2}'$ from the sun's eastern edge. The side of the penumbra next the edge, the part which had been invisible on November 24, was now wholly visible, while that turned towards the sun's centre was unseen. On December 12 it came into view, but was narrower than the penumbra on the other side. On December 17, when the spot had passed the centre of the disc, the penumbra appeared to surround the nucleus equally on all sides.

(804.) In fig. 213, the upper row of spots represents the succession of changes actually presented by this spot, while the lower shows what would occur as a spot traversed the sun's disc, if the spot were simply a surface-stain with a penumbral fringe. The peculiarities shown in the upper row suggest at once that the spot was a cavity or depression. The line of reasoning followed by Dr. Wilson ran as follows :—

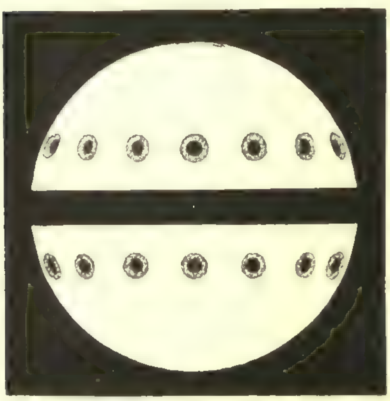


FIG. 213.—Illustrating the observed changes in regular Sun-Spots (upper row), and those which would be seen if the spots were surface-stains.

(805.) Let A, B, C (fig. 214) be supposed to represent perspective views of saucer-shaped depressions on the surface of a sphere—the depressions being all of like dimensions (the sloping sides are supposed for the moment to be transparent). Then it is obvious that to an eye viewing the depressions A, B, and C from above, the relative breadth of the black base and of the shaded sides would be indicated by the breadth of the dark and shaded spaces carried vertically upwards from the spots. Now, suppose the sphere to be rotated on a horizontal axis so that the spots are brought (their edges sliding as it were along the dotted lines) down to the positions A′, B′, and C′. Then the relative breadths of the penumbra and nucleus will be indicated by the distances separating these dotted lines, and must needs therefore be such as are shown in the figure. The shape of the depression would also be as shown. We see, then, that to an eye watching a depression of this sort as it rotated from the position A′ to the position C′, changes of shape would be shown which correspond exactly with those recognised by Dr. Wilson.[1]

[1] Dr. Wilson's observations were insufficient to demonstrate that a spot is a region which lies *as a whole* beneath the solar surface. They show only that the nucleus of the spot he observed lay at a lower level than the penumbra. It is evident from fig. 215 that appearances precisely corresponding with those observed by Wilson would be seen if spots were caused by a double layer of clouds in the solar atmosphere, the lower opaque, the upper semi-transparent and extending on all sides beyond the limits covered by the lower. Such an interpretation has indeed been put forward in recent times by Kirchhoff, and was maintained later by Spörer.

(806.) In endeavouring to form some idea of the physical cause of sun-spots, Wilson was led to suggest as possible all the several interpretations which have been discussed in recent times :—'Whether their first production and subsequent numberless changes,' he says of the spots, 'depend upon the

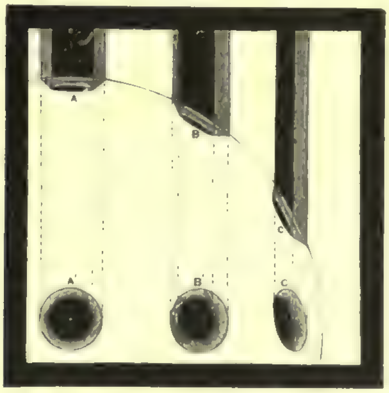


Fig. 214.—Showing that the Spots are depressions.

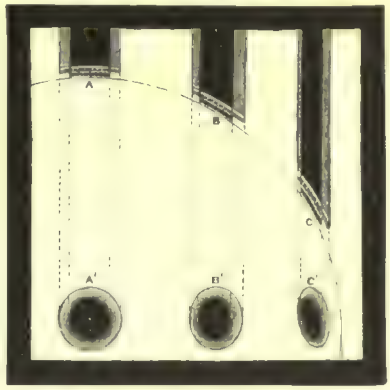


Fig. 215. Illustrating the theory that the Spots might be due to clouds.

eructation of elastic vapours from below, or *upon eddies or whirlpools commencing at the surface*, or upon the dissolving of this luminous matter in the solar atmosphere, as clouds are melted and again given out by our air, or (if the reader pleases) upon the annihilation and reproduction of parts of this resplendent covering, is left for theory to guess at.'

(807.) Sir William Herschel's observations of sun-spots from 1779 to 1792 confirmed Dr. Wilson's results in the main; though he occasionally noticed cases where it seemed to him as though part at least of the shelving sides of a spot were elevated above the surface of the sun, since 'contrary to what usually happens' (to follow his own account of such a case), 'the margin of that side of the spot which was farthest from the limb was the broadest.'

(808.) Sir William Herschel's views respecting the physical condition of the sun, though inconsistent with physical laws learned and established since his day, are interesting as at least suggesting clear ideas as to the nature of his observations. 'It appears evidently,' he remarks, 'that the black spots are the opaque ground or body of the sun; and that the luminous part is an atmosphere, which being interrupted or broken, gives us a transient glimpse of the sun itself.' He suggested that possibly even where there are no spots the real surface of the sun may now and then be perceived—'as we see the shape of the wick of a candle through its flame, or the contents of a furnace in the midst of the brightest glare of it; but this, I should suppose, will only happen where the lucid matter of the sun is not very accumulated.' Herschel's observations of the faculæ were among the earliest real studies of these phenomena since their discovery by Hevelius. In September 1792 Herschel noticed in the neighbourhood of a dark spot, pretty near the edge, 'a great number of elevated bright places, making various figures. I shall call them *faculæ*,' he says, 'with Hevelius; but without assigning to this term any other meaning than what it will hereafter appear ought to be given to it. I see these faculæ extended on the preceding' [the

western] 'side, over about one-sixth part of the sun; but so far from resembling torches, they appear to me like the shrivelled elevations upon a dried apple, extended in length; and most of them are joined together, making waves, or waving lines. . . . There is all over the sun a great unevenness in the surface, which has the appearance of a mixture of small points of an unequal light; but they are evidently an unevenness or roughness of high and low parts.' On September 22, 1792, Herschel observed few faculæ on the sun and few spots, but the whole disc very much marked with roughness, like an orange, and some of the lowest parts of the inequalities blackish. On the following day he thus associates this roughness with the faculæ:—'The faculæ are ridges of elevation above the rough surface.'

(809.) It is evident that on this occasion Sir W. Herschel saw portions of the sun's surface as delineated in figs. 216 and 217 by Secchi with the fine telescope of the Roman observatory; for the appearances depicted in these drawings correspond exactly with Herschel's description. This would very satisfactorily explain,

FIG. 216.—The Sun's surface, showing a Facula.—*Secchi.*

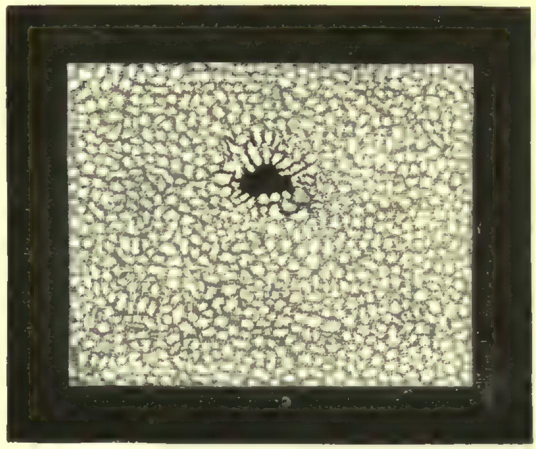


FIG. 217. The Sun's corrugated surface. *Secchi.*

he said, their disappearing towards the middle of the sun and reappearing on the other margin, 'for about the place where we lose them they begin to be edgeways to our view.'

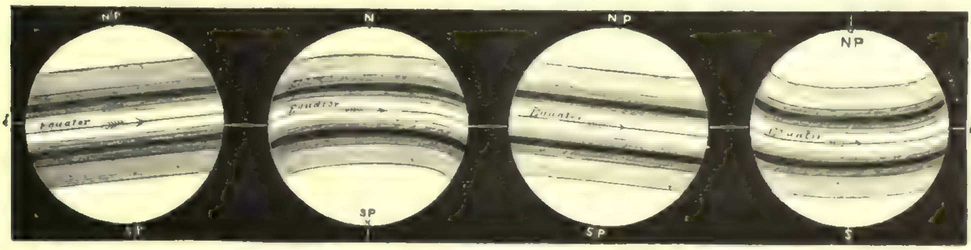
(810.) In a later paper (communicated to the Royal Society in 1801) Sir William Herschel records the results of further observations. He draws special attention to certain characteristic features of the sun's surface. These are, first, *corrugations*, which he regards as elevations and depressions causing the mottled appearance of the sun; secondly, *nodules*, or smaller elevations in the corrugations themselves over which they are distributed as bright spots; thirdly, *punctulations*, or dark spaces between the nodules; and fourthly, *pores*, or darker-coloured places in the punctulations.

(811.) We may sum up as follows the views of Sir William Herschel as to the general constitution of the solar globe and surface:—He supposed the sun to be an opaque globe surrounded by a luminous envelope. He considered that this envelope is neither fluid nor gaseous, but consists rather of luminous clouds floating in a transparent atmosphere. Beneath this layer or envelope of luminous clouds he conceived that there floats in the same atmosphere a

layer of opaque clouds, rendered luminous on the outside by the light which they receive from the outer layer. These opaque clouds protect, according to this theory, the solid and relatively unilluminated nucleus of the sun. When openings are formed in the same region in both layers of clouds, we see the body of the sun as a dark spot. If the apertures are equally large, the spot will be uniformly dark; but if, as more commonly happens, the outer aperture is the greater, the dark nucleus of the spot will seem to be surrounded by a dusky border. If the upper layer alone is perforated, a dusky spot without any dark central portion makes its appearance. Herschel supposed that those spots in which both layers are broken through are caused by an uprush of some highly elastic gas breaking its way through the lower layer, and then, after expansion, removing the upper self-luminous clouds.

(812.) We shall see that while all the facts observed by Herschel have been confirmed, and while his reasoning, so far as it relates to observed facts has been abundantly justified, some of his hypotheses have been disproved by recent observations.[1]

(813.) We have seen that the spots are confined to two definite zones. These extend about 35°[2] on each side of the equator; an intermediate zone to a distance of



I. Dec. 6. II. March 6. III. June 5. IV. Sept. 5.

FIG. 218.—Solar Spot-zones, showing their true configuration in different parts of the year. The black parts show where spots occur with greatest frequency.

some 8° on either side of the solar equator being ordinarily free from spots. Fig. 218 serves to indicate the regions where spots occur, and also (where the darkest zones are

[1] Sir Wm. Herschel's ideas of the sun's fitness to be an abode for living creatures were fanciful in the extreme:—

'The sun appears to be nothing else,' he wrote, 'than a very eminent, large, and lucid planet, evidently the first, or, in strictness of speaking, the only primary one of our system, all others being truly secondary to it. Its similarity to the other globes of the solar system with regard to its solidity, its atmosphere, and its diversified surface; the rotation upon its axis, and the fall of heavy bodies, lead us on to suppose that it is most probably also inhabited, like the rest of the planets, by beings whose organs are adapted to the peculiar circumstances of that vast globe. Whatever fanciful poets might say in making the sun the abode of blessed spirits, or angry moralists devise in pointing it out as a fit place for the punishment of the wicked, it does not appear that they had any other foundations than mere opinion and vague surmise; but now I think myself authorised *upon astronomical principles* to propose the sun as an inhabitable world, and am persuaded that the foregoing observations, with the conclusions I have drawn from them, are fully sufficient to answer every objection that may be made against it.'

[2] If we may trust an observation of La Hire's (which, however, Mr. Carrington, than whom no higher authority can be cited, is disposed to reject), a spot has been seen as far as 70° from the sun's equator. In 1846 Dr. Peters, of Altona, saw a spot 50° 55′ from the equator, while Carrington and Capocci have each seen spots about 45° from that circle.

shown) those regions in which spots occur most frequently and attain the greatest dimensions. The sun is so placed in the four views as to show the way in which the spot-belts are actually presented—I. early in December; II. early in March; III. early in June; and IV. early in September. The actual dates are those indicated under the several figures.

(814.) Mr. Richard Carrington, in a series of observations which commenced in 1853 and ended in 1861, endeavoured to detect the law of distribution of the spots, to determine the true period of the rotation of the sun's body, and to detect 'systematic movements or currents of the surface, if such exist in a definable manner.' In the course of these researches he discovered, in the first place, that discrepancies already noticed by former observers between the values deduced for the sun's rotation arise from real differences in the velocities with which the spots move in different solar latitudes. Near the equator a spot moves at a rate indicating a more rapid rotation than in higher latitudes. Further, even among spots in the same latitude proper motions may be recognised. These latter motions are to be regarded, however, as abnormal, and simply rendering unreliable such observations as are made on but a few spots. The peculiarities affecting the motions of spots in different latitudes were reduced by Carrington into a formula, but such formulæ have little scientific value.[1] The following table gives the observed rates of rotation for different latitudes (the formula being based on these values):—

	Sun's rotation-period			Rotation per day
deg.	d.	h.	m.	m.
50 N. Lat.	27	10	41	787
30 ,,	26	9	46	824
20 ,,	25	17	8	840
15 ,,	25	9	10	851
10 ,,	25	3	29	859
5 ,,	25	0	42	863
0 Equator	24	2	11	867
5 S. Lat.[2]	24	23	18	865
10 ,,	25	5	35	856
15 ,,	25	13	31	845
20 ,,	25	17	52	839
30 ,,	26	12	50	814
45 ,,	28	11	0	759

[1] This formula was as follows:—Let ξ be the angle through which a part of the sun in latitude λ rotates in one day. Then

$$\xi = 14^\circ\ 25' - 165' \sin^{\frac{7}{4}} \lambda$$

Spörer gave the formula

$$\xi = 16^\circ{\cdot}8475 - 3^\circ{\cdot}3812 (\sin \lambda + 41^\circ\ 13').$$

[2] In this table the observed daily mean rotation is less in southern latitudes than in the corresponding northern latitudes. It is doubtful whether we have in this relation any indication of the true cause of the observed variations in the rate of rotation, or merely a peculiarity which would have disappeared in a longer series of observations. In favour of the former view, we have the consideration that the determination for each southern as well as for each northern latitude was independently effected, so that the coincidence of the results indicates the existence of some real cause. If Sir John Herschel is right in considering that the more rapid rotation near the solar equator implies the action of external matter in maintaining the rotation of the photosphere, it may be suggested that the northern surface of the sun, being directed somewhat more fully towards that region whither the sun's proper motion is carrying him, would probably be more exposed to the influence of this external action—'the frictional impulse of circulating planetary matter in process of subsidence into, and absorption by, the central body'—much as our northern hemisphere is saluted with a larger number of meteoric missiles from June to December, when the northern hemisphere is in advance, than from December to June, when this hemisphere is towards the more sheltered side of the Earth.

(815.) Spörer re-examined the whole subject, taking into account later observations, and in particular those which had been made by Fr. Secchi. The following table includes the general elements of the solar rotation as deduced by Carrington and Spörer, and reduced by me to the year 1890 :—

Elements	Carrington	Spörer
	° ′	° ′
Longitude of node of solar equator	74 16	74 56
Inclination of solar equator	7 15	6 57
Diurnal rotation	14 18	14 27
Rotation-period	25d· 38	25d· 23

(816.) In fig. 218 the varying presentation of the sun on account of the inclination of his equator to the Earth's orbit is shown as exactly as possible on the scale of the figure. On or about December 6,[1] the Earth crosses the plane of the sun's equator (passing southwards), and then the sun is presented as at I., fig. 218. Three months later the Earth reaches her greatest distance south of the solar equator, so that on or about March 6 the sun is presented as at II. On about June 5 the Earth again crosses the plane of the solar equator, this time passing northwards, and the sun is presented as at III. Lastly, on about September 5 the Earth reaches her greatest distance north of the solar equator, and the sun is presented as at IV.

(817.) Sir John Herschel was the first to point out the true general significance of the spot-zones. He remarks that their very existence 'at once refers the cause of spots to fluid circulations, modified, if not produced, by the sun's rotation, by reasoning of the very same kind whereby we connect our own system of trade and anti-trade winds with the Earth's rotation. Having given any exciting cause for the circulation of atmospheric fluids from the poles to the equator and back again, or *vice versâ*, the effect of rotation will necessarily be to modify those currents as our trade-winds and monsoons are modified, and to dispose all those meteorological phenomena (on a great scale) which accompany them as their visible manifestations, in zones parallel to the equator, with a calm equatorial zone interposed.' Thus far Sir John Herschel was dealing with observed facts, and pointing to almost inevitable conclusions. In dealing with the question—Whether any cause of atmospheric circulation can be found in the economy of the sun, 'so far as we know and can comprehend it ?' he presented hypothetical considerations of a less satisfactory and convincing nature. They need not here detain us.

(818.) In the year 1826, Schwabe of Dessau began an important series of observations of the sun-spots. On the 277 days during which the weather permitted Schwabe to observe the sun in that year, there were only 22 on which no spots could be seen. In the next year there were even more spots, and only two days when none were seen. In the next two years the sun's face was not on any day seen without spots. In 1830 only one day occurred on which no spots could be seen ; in 1831, only three. But in 1832 there were no less than 49 days (out of 270 on which

[1] The date for any year can always be determined from the *Nautical Almanac*. It is only necessary to note when the sun's longitude is 180° + the longitude of the node of the sun's equator (say 180 + 74°, or 254°). In like manner, the Earth is again in the plane of the solar equator when the sun's longitude is about 74°.

observations were made) during which no spots were seen. In 1833 there were 139 such days out of 267; in 1834, 120 out of 273; in 1835, 18 out of 244; and then followed four years during which not a single observing day occurred on which no spots were visible.

The results of forty-two years of Schwabe's labours (so far as the question of the periodicity of spot-frequency is concerned) are included in the following table:—[1]

Year	Days of observation	Days without spots	New groups	Year	Days of observation	Days without spots	New groups
1826	277	22	118	1848	278	0	330
1827	273	2	161	1849	285	0	238
1828	282	0	225	1850	308	2	186
1829	244	0	199	1851	308	0	141
1830	217	1	190	1852	337	2	125
1831	239	3	149	1853	299	4	91
1832	270	49	84	1854	334	65	67
1833	267	139	33	1855	313	146	28
1834	273	120	51	1856	321	193	34
1835	244	18	173	1857	324	52	98
1836	200	0	272	1858	335	0	202
1837	168	0	333	1859	343	0	205
1838	202	0	282	1860	332	0	211
1839	205	0	162	1861	322	0	204
1840	263	3	152	1862	317	3	160
1841	283	15	102	1863	330	2	124
1842	307	64	68	1864	325	4	130
1843	312	149	34	1865	307	26	93
1844	321	111	52	1866	349	76	45
1845	332	29	114	1867	312	195	25
1846	314	1	157	1868	301	12	101
1847	276	0	257				

(819.) The careful study of Schwabe's results (combined with such scattered records as the observations of former astronomers supply) led Professor Wolf, of Zürich, to the conclusion that a period of 11·11 years (or the ninth part of a century) is indicated, rather than a ten-yearly period. He believed that he could also recognise minor periods—thus that a perceptibly greater degree of apparent activity seemed to prevail annually on the average of months of September to January than in the other months of the year; and he also found signs of variations succeeding each other at an average interval of 7·65 months, or 0·637 of a year. But the evidence was not such as to establish these minor oscillations of spot-frequency as constantly recurrent phenomena, still less to associate them, as Wolf strove to do, with the orbital motion of our Earth and Venus. A long period, estimated at about fifty-six years, has also been suspected, but the evidence for it is very slight.

(820.) The most cursory examination of the numbers in the above table suffices to indicate the peculiarity that the progression from minimum to maximum is more rapid than the progression from maximum to minimum. In other words, if we regard the periodic changes of spot-frequency in the light of a series of waves—the maxima corresponding to the crests of the waves and the minima to the 'troughs'—the front slope of each wave is more abrupt than the rear slope.

(821.) Professor Wolf, in forming his estimate of the mean period of spot-variation, endeavoured to judge, from the imperfect records of sun-spots during the eighteenth

[1] This table cannot well be combined with later statistics of the same kind, since these have been tabulated on a different scale.

century, at what times spots were most or least numerous. This part of the evidence cannot be regarded as trustworthy. Even if accepted as such, it can be interpreted in more ways than one, insomuch that epochs regarded by Wolf as marked by minor maxima or minima—the crests or hollows of mere wavelets of disturbance riding on the main waves—have by others been regarded as indicating true maxima or minima.

(822.) The following table presents Wolf's dates for maxima and minima from 1705 to 1867, with the dates of two maxima and one minimum since:—

Intervals in years	Dates of Maxima	Possible error in years	Intervals in years	Dates of Minima	Possible error in years
—	1705.0	2.0	—	1712.0	1.0
12.5	1717.5	1.0	11.0	1723.0	1.0
10.0	1727.5	1.0	10.0	1733.0	1.5
11.0	1738.5	1.5	12.0	1745.0	1.0
11.5	1750.0	1.0	10.7	1755.7	0.5
11.5	1761.5	0.5	10.8	1766.5	0.5
8.5	1770.0	0.5	9.3	1775.8	0.5
9.5	1779.5	0.5	9.0	1784.8	0.5
9.0	1788.5	0.5	13.7	1798.5	0.5
15.5	1804.0	0.1	12.0	1810.5	0.5
12.8	1816.8	0.5	12.7	1823.2	0.2
12.7	1829.5	0.5	10.6	1833.8	0.2
7.7	1837.2	0.5	10.2	1844.0	0.2
11.4	1848.6	0.5	12.2	1856.2	0.2
11.6	1860.2	0.2	10.9	1867.1	0.1
10.6	1870.8	0.2	11.4	1878.5 [1]	0.1
13.0	1883.8 [1]	0.2			

(823.) Taking this table as it stands, we have fifteen intervals from the maximum of 1705·0 to that of 1883·8, a range of 178·8 years, whence the mean spot period 11·92 years would be deduced. The minima give fourteen intervals between 1712 and 1878·5, a range of 166·5 years, whence we get the mean spot period 11·89. Wolf's estimate was 11·11 years. It has been suggested that a small maximum may have occurred in 1797, which has here been overlooked; and a period of 10·45 years has been inferred. All that can be regarded as certainly established is that the oscillation of spot-frequency is irregular, the interval between maxima having ranged from $7\frac{3}{4}$ to $15\frac{1}{2}$ years, and the interval between minima from 9 years to $13\frac{3}{4}$. No average period even can at present be regarded as established; and as for the idea that we may find in regular minor oscillations some way of making the main oscillations appear regular (as being regularly modified), we may regard all attempts in that direction as not merely hopeless but unscientific.[2]

[1] The minimum for 1878, and more exactly the maximum for 1883, can be inferred from the following table, in which the spot-surface is estimated in millionths of the surface of the solar hemisphere:—

Year	Total spot-surface
1878	24
1879	49
1880	416
1881	730
1882	1002
1883	1155
1884	1079
1885	811

[2] Such ideas belong to the infantile mode of thought which strove to show that the movements of the planets are really regular and synchronous, and failing, conceived that movements once regular had been disturbed of set purpose to punish the misdeeds of man.

(824.) It would be quite unreasonable in the presence of this evidence of irregularity both in the general periodicity of sun-spots and in the recurrence of minor oscillations, to attribute these solar changes to the planets, whose movements are regular and unchanging. For instance, the theory that the sun-spots are in some way influenced by the action of Jupiter is absolutely negatived when we notice that sun-spot phenomena are so irregular that both maxima and minima have occurred when Jupiter has been in perihelion, and also both maxima and minima have occurred when Jupiter has been in aphelion. It is the same with the minor oscillations which Wolf, Balfour Stewart, Loewy, De la Rue, and others have endeavoured to associate with the movements of the Earth, Venus, and Mercury. In assuming, as they have done, that the evidence is clear enough to indicate an exact period for some oscillation, and in noting that the exact period so determined is nearly the same as the period of a planet, they in reality have suggested that the planet has nothing to do with the disturbance : for a definite difference between two exactly determined periods proves that in the fulness of time each phase in either period will correspond in turn with every phase in the other ; whereas to prove interdependence each phase of either period should correspond with one phase, and one phase only, of the other.

(825.) It is not unreasonable to suppose that the amount of heat received by the Earth from the sun must in some degree vary according as there are more or fewer spots on the sun's surface. But if such variation exists it is very slight. Some (as Stone, then Astronomer Royal at the Cape, and Gould, at Cordoba) consider that rather less heat is given out when the sun is much spotted than at other times. Others find the reverse. Gelinck, after carefully weighing all the evidence, concludes that there is no measurable difference. Assuredly no difference has so far been measured.

(826.) As for the idea that sun-spots may exert specific influence on the weather of different parts of the Earth, it is beneath the dignity of science to discuss a notion worthy only of the first beginnings of astrology.[1]

(827.) The theory that there is some association between the sun-spot period and magnetic disturbances on the Earth stands on a very different footing. It is among the most interesting consequences of the recognition of the periodicity of sun-spot disturbances.

(828.) In every part of the Earth the magnetic needle has at any given epoch a

[1] It would never have been seriously attacked —so absurd is it—but that for a while certain charlatans held out promises that by observing sun-spots they would learn how to predict weather—'for a consideration' (for instance, what they called a physical observatory, with large salaries for observers). So little is known by the public about astronomy or physics that many believed this vain (but by no means purposeless) promise. Now, however, it may be hoped that the idea is dead—in the same sense that astrological superstition is dead, though often galvanised into the semblance of life by the pretences of astrological fortune-tellers.

certain definite position about which, under normal conditions, it would oscillate during the day. Both as regards inclination and direction with respect to the compass-points (called the magnetic declination), this position may be regarded as determinate, at least for every fixed observatory; and further, the intensity of the needle's directive power—that is, the energy with which, if slightly disturbed, it seeks to recover its position of rest—may also be regarded as determinate. From year to year all these magnetic elements undergo change; but with these changes we are not here concerned.[1] Changes of a much more minute character, and the changes affecting these changes, are what we have at present to deal with. Each day the needle oscillates gently about its position of rest, the oscillation corresponding to a very slight tendency on the part of that end of the needle which lies nearest to the sun to direct itself towards his place. The daily oscillation is itself variable in a systematic manner, not only with the progress of the year, but with that of the lunar month. The daily oscillation also varies at times in a sudden and irregular manner. Movements become perceptible which are totally distinct from the regular periodic oscillations. They are recognised simultaneously over widely-extended regions of the Earth. In some well-authenticated instances these magnetic vibrations thrill in one moment the whole frame of the Earth.

(829.) Lamont, of Munich, was the first to announce that these magnetic disturbances attain a maximum of frequency in periods of about ten years. This was in 1850. Two years later, General Sabine and (independently) Professors Wolf and Gautier, noted the coincidence of this period with that of the solar spots, not merely

[1] I may note here my belief that the recognition of the laws affecting the secular variation of the Earth's magnetism would be simplified if attention were primarily directed to the magnetic lines determined by the inclination of the needle, instead of to those which depend on the intensity of the directive action. Fully admitting the weight of General Sabine's arguments as to the importance of the intensity, and as to its being the more essentially magnetic element (north and south, or vertical and horizontal, having no direct relation to the Earth's magnetic forces), I yet cannot but regard the inclination as affording the more trustworthy means of determining the geographical features (so to speak) of terrestrial magnetism. The intensity, as well as the inclination and declination (which together form one magnetic feature—the position of rest), is doubtless affected by local circumstances; so that, superposed as it were on the normal intensity, there is an intensity depending on local conditions; and so, in like manner, as to the inclination and declination. Now, the question arises, which of these two features is likely to be most *significantly* affected by these doubtless slight peculiarities? In determining, for example, the position of the magnetic equator and poles, with reference to intensity or inclination, would the error due to some small increment or decrement of intensity cause a greater or less divergence from the true position than a correspondingly small increment or decrement of inclination? The answer is obvious. The intensity-equator is the line of minimum intensity, and the intensity poles are points of maximum intensity. Near a minimum or a maximum quantities change very slowly, and thus a very minute increment or decrement would largely shift the estimated place of minimum or maximum intensity. But a corresponding increment or decrement of inclination would have no such effect, because the inclination changes as quickly near the inclination-equator and poles as on any inclination-latitude.

The case may fairly be compared to the determination of the geographical equator and poles. Undoubtedly gravity is a far more essentially terrestrial element than the elevation of the pole-star, or of the true pole of the heavens; and also, undoubtedly, the Earth's equator is the region where gravity is least, while the poles are regions where gravity is greatest. Yet these reasons are not considered sufficient to induce us to take the force of gravity as the most satisfactory indication of latitude, or to lead us to mark down as the true equator of the Earth that line along which careful observation shows that gravity has its minimum value. We know, in fact, that however excellent the observations might be, the deduced line would diverge in marked degree from the true equator.

A similar objection may be urged against the stress laid on the position of the line of no declination; for, from the very nature of this line, minute local peculiarities must cause enormous irregularities, and (when coupled with secular variations) the most rapid and remarkable changes of figure.

in duration, but in such sort that maxima of spot-frequency coincided with maxima of magnetic disturbance, and minima with minima.

(830.) A relation so strange might well excite grave doubts. Coincidences have so often misled men of science, and indeed it is so certain in the very nature of things that misleading coincidences must occur, that physicists were justified in requiring further evidence. Such evidence fortunately was not wanting.

(831.) On September 1, 1859, Mr. Carrington when engaged in his work of portraying the sun's face, observed within the area of a great group of spots (the size of which had previously excited general remark) two patches of intensely bright and white light in the middle of the group. His first impression was that by some chance a ray of light had penetrated a hole in the screen attached to the object-glass, by which the general image is thrown into shade. Finding this not to be the case, he carefully noted down the time by the chronometer. He ran to call some one to witness the exhibition with him; but, on returning within sixty seconds, he was mortified to find that it was already much changed and enfeebled. Very shortly afterwards the last trace was gone, and although he maintained a strict watch for nearly an hour no recurrence took place. The spots had travelled considerably from their first position, and vanished as two rapidly fading dots of white light. The instant of the first outburst was not fifteen seconds different from 11h. 18m. Greenwich mean time, and 11h. 23m. was taken for the time of disappearance. In this interval of five minutes, the two spots traversed a space of about 35,000 miles, moving at a mean rate of certainly not less than 120 miles per second. On referring to the sketch of the group which he had finished before the occurrence, Mr. Carrington was unable to recognise any change whatever. The impression left upon him was that the phenomenon took place at an elevation considerably above the general surface of the sun, and accordingly altogether above and over the great group on which it was seen projected. Both in figure and position the patches of light seemed entirely independent of the configuration of the great spot, and of its parts, whether nucleus or umbra.

(832.) Mr. Hodgson witnessed the same phenomenon:—'While observing a group of spots on September 1,' he says, 'I was suddenly surprised at the appearance of a very brilliant star of light,[1] much brighter than the sun's surface, most dazzling to the protected eye, illuminating the upper edges of the adjacent spots and streaks, not unlike in effect the edging of the clouds at sunset; the rays extended in all directions; and the centre might be compared to the dazzling brilliancy of the bright star Alpha Lyræ when seen in a large telescope with low power. It lasted for some five minutes, and disappeared instantaneously, about 11h. 25m. A.M.'

(833.) At the moment when the sun was thus disturbed, the magnetic instruments at Kew exhibited those signs which indicate the occurrence of magnetic changes. A 'magnetic storm,' which affected the whole Earth, occurred in both hemispheres. Vivid auroras were seen not only in both hemispheres, but in latitudes where auroras are very seldom witnessed. Even in Cuba the sky was illuminated by the auroral radiance. Strong earth-currents were observed along telegraphic lines, and these currents continually changed their direction, while all the time the magnetic needles in fixed observatories were deflected to one side of their normal

[1] It seems probable that whereas two spots were seen by Mr. Carrington, who observed the solar image projected on a screen, these were blended, owing to their extreme brilliancy, into the semblance of a single spot when observed in the telescope itself by Mr. Hodgson.

position. Great auroras were seen not only in high latitudes but to within 18° of the equator and in both hemispheres. There were great electro-magnetic disturbances in all parts of the world. At Washington and Philadelphia, the telegraphic signal-men received severe electric shocks. At one station (in Norway), the telegraphic apparatus was fired; and at Boston, Mass., a flame of fire followed the pen of Bain's electric telegraph. Other instances of association between great solar disturbances and sudden changes in the intensity of terrestrial magnetic action have been noted during the last quarter of a century. One of the most remarkable cases on record was the intense magnetic storm which accompanied the development of the great spot of April 1882, pictured in fig. 219, by that skilful solar draftsman the Rev. F. Howlett. Another great magnetic storm occurred during the development of the still larger spot pictured by the same observer in figs. 236–238, on page 362.

(834.) The association between the disturbance of the solar photosphere and the phenomena of terrestrial magnetism has since been made the subject of special inquiry by Mr. Ellis, of Greenwich. The evidence which he has collected shows unmistakably that some association exists, though the oscillations of magnetic and solar action do not exhibit absolute parallelism in point of detail.

April 19, 1882, 3.30 P.M.

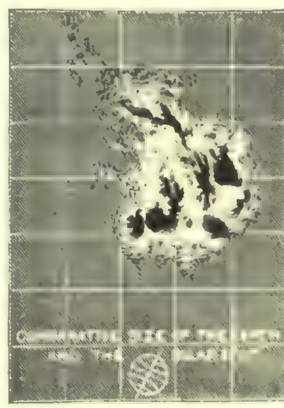


FIG. 219. The great Solar Spot of April, 1882.
Extreme length, 2' 0". Mean breadth, 0' 45".
∴ Area, about 1,093,500,000 square miles.

(835.) The study of the details of the sun's surface during the present century has enabled the astronomers of our time to understand phenomena which had been less clearly seen by their predecessors, and has revealed many features of great interest and significance.

(836.) In the first place must be noted the discovery by Mr. Dawes that the umbra of a spot, when carefully studied under conditions suitable for the detection of slight differences of luminosity, is not uniformly dark, but somewhat mottled. In all large and tolerably symmetrical spots, Mr. Dawes detected a region much darker than the rest of the umbra,—a 'perfectly black hole,' he called it, but such descriptions are unscientific and misleading. This darker tract, called the *nucleus*, has been found by Professor Langley to be not perfectly black: in fact a strong purple light seems to be emitted from it; but how much of this may be our own sky-light Professor Langley has not determined.

(837.) It was announced by Mr. Nasmyth in 1862 that the *pores* seen in the solar photosphere are 'polygonal interstices' (I quote Sir John Herschel's account) 'between certain luminous objects of an exceedingly definite shape and general uniformity of size, whose form (at least as seen in projection in the central portions of the disc) is that of the oblong leaves of a willow-tree. These cover the whole disc of the sun (except in the space occupied by the spots) in countless millions, and lie crossing each other in every imaginable direction.' The appearance of the sun, according to this view, is exhibited in fig. 220.

(838.) Mr. Dawes and other excellent observers pointed out, however, that no such interlacing as Mr. Nasmyth described is ever observable among the small bright spots which lie scattered over the general ground of the photosphere; that these spots can

in no sense be said to resemble willow-leaves, though they present every variety of figure and size; and lastly, that they had been long known to solar observers, and are,

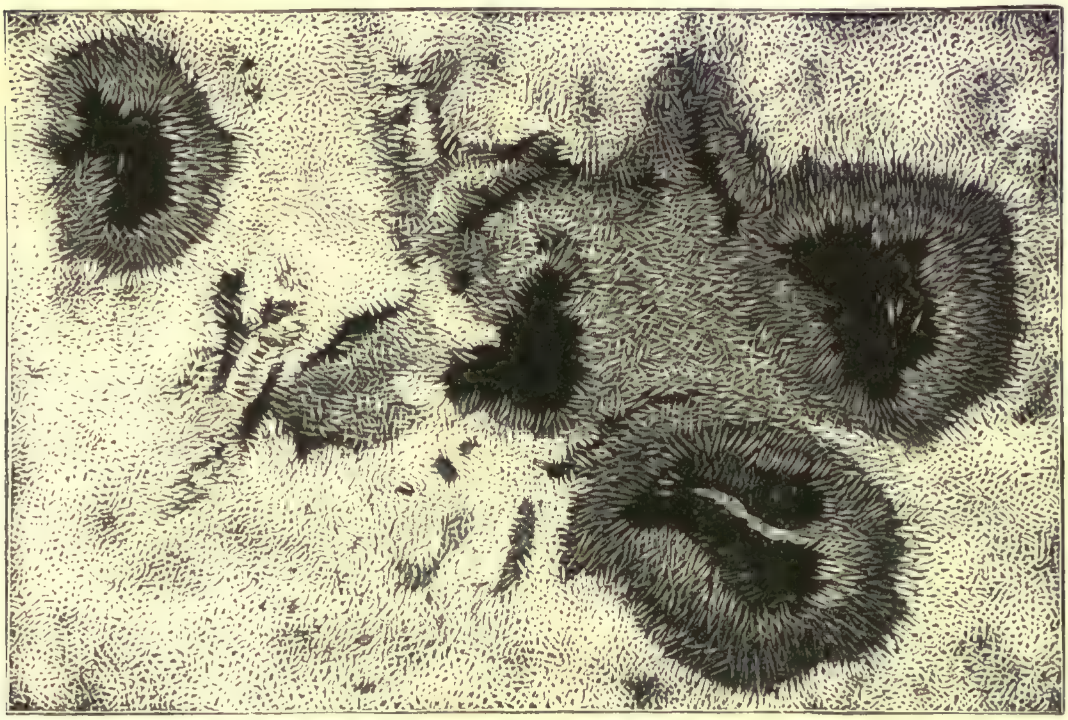


FIG. 220.—A large Sun-spot, as seen by Mr. Nasmyth, showing the interlacing willow-leaves.

FIG. 221.—Sun-spots, showing penumbral streaks, as drawn by Secchi.

in fact, no other than the *nodules* of Sir William Herschel. 'The only situation,' said Mr. Dawes, 'in which I have usually noticed them to assume anything like the shape of willow-leaves, is in the immediate vicinity of considerable spots, on their penumbræ, and frequently projecting beyond it for a small distance on to the *umbra*.

(839.) Fig. 221 presents Secchi's views of the streaks seen in the penumbræ of spots, and regarded by Nasmyth as resembling willow-leaves. Secchi called these markings 'penumbral rills.' Fr. Secchi described the appearance of the luminous spots as resembling strokes made with a camel's-hair pencil.

(840.) Dr. Huggins calls them *granules*, using that term as less definite than *rice-grains*, which they had been called by some of the observers at Greenwich. Fig. 222 represents a view of a portion of the sun's disc as seen by him. It will be noticed that if this view is held at a considerable distance from the eye the general aspect corresponds closely with the mottled appearance presented by the sun in telescopes of moderate power.

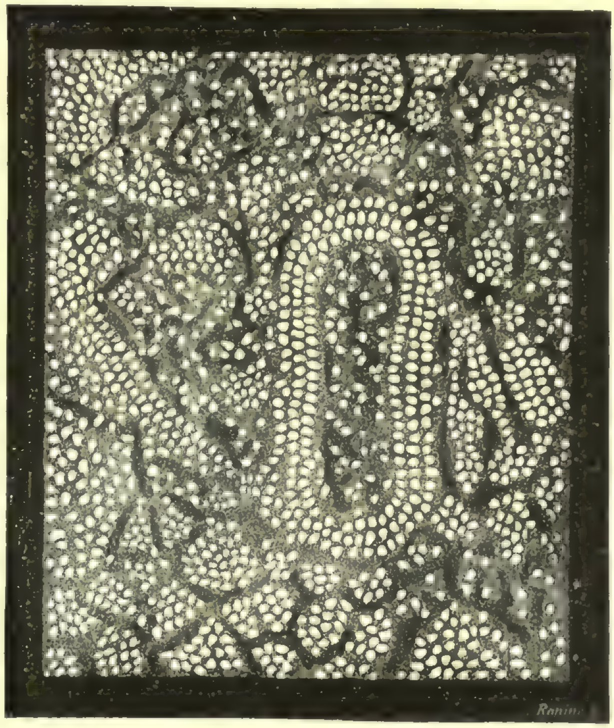


Fig. 222.—A portion of the Solar Surface, showing Granules, and illustrating the peculiarities of distribution producing the *Maculæ* (*Huggins*).

(841.) No astronomer now accepts the idea that the solar photosphere is formed of interlacing streaks of brightness as Nasmyth had supposed; but for a long time there were many who regarded Nasmyth's observations as undoubtedly new, and also as of unmistakable interest and importance.[1]

(842.) On April 13, 1869, Secchi noticed that over the whole of a large spot and its neighbourhood, there were multitudes of *leaves*, and that a bridge across the spot was formed of elongated leaves. The leaves in the neighbourhood of the spot were oval, the greater diameter about three times the less—thus,  'like the leaves of the olive and certain *salices*.' He says, 'What are these things? They are *veils*

Fig. 223.—Leaf-like Solar Formation in the neighbourhood of a Spot (*Secchi*).

[1] Sir John Herschel was one of the last to abandon the belief in Nasymth's willow-leaves. For a time he even entertained the strange fancy that the willow-leaves are organisms, and may even possess some form of vitality. 'These flakes,' he said, 'are evidently the *immediate sources of the solar light and heat*, by whatever mechanism or whatever processes they may be enabled to develope, and as it were elaborate, these elements from the bosom of the non-luminous fluid in which they appear to float. Looked at in this point of view, we cannot refuse to regard them as *organisms* of some peculiar and amazing kind; and though it would be too daring to speak of such organisation as partaking of the nature of life, yet we do know that vital action is competent to develope at once heat and light and electricity.' Regarded as solar inhabitants whose fiery constitution enables them to illuminate, warm, and vitalise the whole solar system, the solar willow-leaves would not want that evidence of might which gigantic size affords. Milton's picture of him who on the fires of Hell 'lay floating many a rood' seems tame and commonplace compared with Herschel's conception of these floating monsters, the least covering a greater space than the British Islands.

of the most intricate structure.' In another part of the spot, in a region which he describes as a third nucleus, he saw the leaves pictured in fig. 223 arranged in a radiating manner, precisely like a crystallization of sal-ammoniac seen by means of the solar microscope—attached to a stalk (*lingua*). He regarded the rest of the umbra as due to the dissolution of such leaves.

(843.) On April 14, the spot in which Secchi had observed these singular objects presented the appearance depicted in fig. 224. 'It was in a marvellous condition,' says Secchi, 'full of bridges, arcs, stalks, and leaves, like the great spot of 1866.'

FIG. 224.—Sun-spot observed on April 14, 1869, by Secchi.

(844.) From the researches of Fr. Secchi it would appear as though the umbral portion of large spots were formed of luminous matter which is undergoing a continual process of dissipation towards its interior edge. He compared the process to the gradual dissipation of cumulus clouds under the heat of a summer sun, regarding the umbra as of the nature of a veil of clouds, and the nucleus as a region where an intenser heat has caused these clouds to melt away. Within spots such as these he recognised the presence of coloured matter; and he expressed his belief as far back as 1860, that an envelope of this coloured matter surrounds the whole globe of the sun. These appearances have been noticed also by other observers.

(845.) We owe to Professor Langley, then of the Alleghany Observatory, near Pittsburg, the truest views of solar details yet made. Plate XIII. is one of the best of his drawings. It presents all the chief features of the sun. The granules or rice-grains are seen, not all equal in size, nor uniformly distributed, so that by holding the picture at some distance from the eye the parts representing the solar photosphere are seen to be mottled. Professor Langley has recognised more clearly than any other telescopist the varieties of form and size existing among the granules. He also was able in the fine clear skies of western Pennsylvania to see (with comparatively small telescopic power, for his telescope was but eleven inches in diameter) the true shapes of the filaments which form the penumbræ of spots. The picture explains itself so well that no verbal description is necessary. But I would invite special attention to the contrast between the well-defined penumbral fringe on one side of the spot and the broken outlines and irregular forms on the other, where the penumbra is in many places lost. It may be observed that where on this side large masses of luminous matter seem breaking away from the rest of the photosphere, the filamentous formation can be recognised—though it is not so distinctly obvious as within the spot. We may infer from this that the filamentous structure is present throughout the solar atmosphere, but where there is no disturbance the filaments hang vertically in the atmosphere, and only the cloud-masses from which they extend can be discerned. We

can thus understand how it is that all round a spot, where these long filamentous cloud-forms have been displaced from the vertical, there should be facular streaks, where the cloud-masses are thrust together into wave-like aggregations; for the clouds are more intensely luminous than the spaces between them. It must be remembered in studying such a drawing as Plate XIII. that objects which appear small in the picture are in reality enormously large. The cloud-granules have diameters ranging from 200 to 500 miles. Even the narrowest filaments have a cross breadth of more than fifty miles, and some of them are ten or twelve thousand miles in length.

(846.) Although photography has not yet been so employed as to show the solar details on so large a scale or so fully as they have been drawn, yet in some respects solar photographs give more satisfactory information than even the best pictures.

(847.) Fig. 225 represents a portion of the sun's surface as photographed by M. Janssen at the Meudon Observatory. It presents the granules better than in any telescopic drawing I have ever seen.[1] It will be noticed that some parts of the picture are less distinct than others. Janssen was at first disposed to attribute this to the presence of misty tracts in our own air. But taking photographs at short intervals of time he found these misty tracts repeated in successive photographs, though undergoing slow changes of position. He accordingly concluded that they are true solar phenomena. In the photograph of the whole solar disc, some eighteen inches in diameter, they appear to form a kind of network of misty bands which Janssen calls the *Réseau Photosphérique*. These tracts are regions where the bright clouds forming

Fig. 225. A portion of the Sun's surface, showing the 'Solar Granules,' photographed by Janssen, 1877.

[1] Many astronomers, in speaking of solar photography, describe as a serious difficulty what is in reality the chief advantage which photography has over ordinary vision in dealing with the solar details. In observing the sun through a telescope the astronomer is troubled by the undulations of our atmosphere, which blur and confuse the image of the sun. In the course of one-tenth of a second (the time during which, on the average, visual impressions last) a number of images, differently modified by atmospheric undulations, are all present at once on the retina, and their combination produces an indistinct picture. When the sun is photographed, each part of the photographic plate does its work in less than one-thousandth part of a second (that is, each part of the picture receives light only for this short time); and there is no time for multitudinous images to combine their impressions. Just as an instantaneous photograph of a revolving disc or wheel, on which there are printed letters, shows all the letters distinctly which the eye fails utterly to recognise, so an instantaneous photograph of the sun may show all the details distinctly which in ordinary vision are confused and indistinct because shown in so many different ways. There may be, or rather there must be, some degree of distortion in the photographic picture, since it gives one of the many views which are all included in the picture seen by ordinary vision. But though there is distortion, there need be no indistinctness (if due precautions are taken as to optical details), and whatever seeming indistinctness of outline is recognised may be attributed to a real softening of the edges of dark or luminous tracts on the surface of the sun.

the granules lie at a somewhat lower level than elsewhere; in other words, they correspond to the valleys of a series of intercrossing undulations extending over the whole surface of the sun.

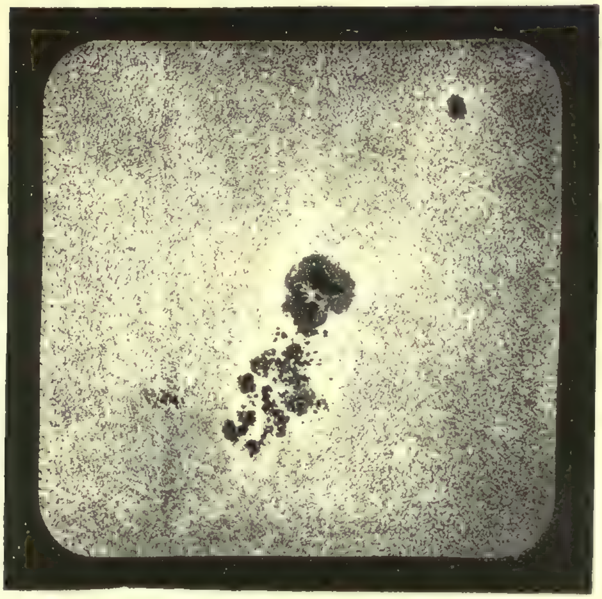


FIG. 226.—Photograph of a large Solar Spot, with surrounding Facu'æ, &c. (*Janssen*).

(848.) Fig. 226 is from another photograph of Janssen's. It presents a large spot after the later processes of change considered farther on have become well developed. Yet on one side the spot retains a regular and well defined penumbra. The little spot in the upper right hand corner presents well, though on a small scale, the appearance of a spot while still so recently formed as not to have lost its regularity of figure.

(849.) Fig. 227 is an enlarged view of the larger spot of fig. 226. It shows many peculiarities of detail which are well worth studying.

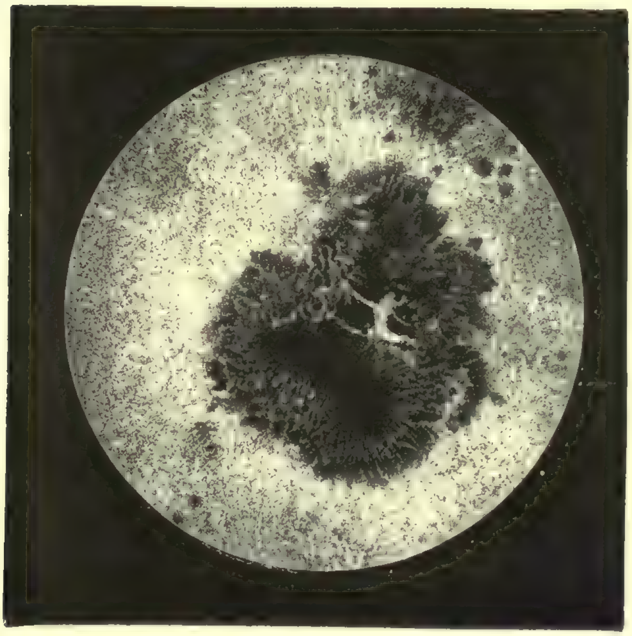


FIG. 227.—Same spot on a larger scale (inverted right and left) (*Janssen*).

(850.) The processes of formation, enlargement, and disappearance of spots are full of interest. Although no regular law has been detected in their succession, we can yet recognise certain distinctive features ordinarily belonging to each stage of development. The formation of a spot is usually preceded by the appearance of faculæ. Then a dark point makes its appearance, which increases in size, the penumbral fringe being presently recognised around it, and the distinction between the umbra and the penumbra being well defined. The same clearness of definition continues ordinarily until and after the spot has reached its greatest development. But later the edges of the penumbra seem less sharp, and an appearance is presented as though there floated over the outer part of the penumbral fringe luminous projections from the surrounding photosphere, brighter in some places than in others, and not unfrequently attaining a brightness which seems to exceed even that of the faculæ. At certain parts of the spot's circumference this bright matter

projects, hiding the whole width of the penumbra and forming a sort of cape or promontory, with sharply serrated edges, singularly well defined against the dark background of the umbra. It is usually in this manner that the formation of a bridge begins, two promontories on opposite sides of a spot, or even on the same side, joining their extremities, so as to form either a bridge of light across the umbra, or a curved streak having both its extremities on one side of the spot. But indeed no strict law or sequence has yet been assigned to these processes of change. In a large spot the wildest and most fantastic variations will take place, and often when the spot seems approaching the stage of disappearance it will renew its existence, as though fresh forces were at work in disturbing the region it belongs to.

(851.) Some of the processes of change which take place in large spots are very well exemplified in fig. 228, where the drawings 1 to 4 by the Rev. F. Howlett show the successive changes of appearance presented by the great spot of 1865, from October 7, when it was on the sun's eastern limb, until October 16, when it had passed the central part of the disc. The drawings by the same observer, illustrating Arts. 856–859, figs. 231 to 238, illustrate admirably the later stages of a sun-spot's career.

FIG. 228.—The great Sun-spot of 1865, from Oct. 7 to Oct. 16 (*Howlett*).

FIG. 229. Faculæ round a Sun-spot (*Chacornac*).

(852.) The faculæ seen round a sun-spot, and especially the characteristic forms which they present during the stages preceding the spot's disappearance, are well illustrated in fig. 229, representing a spot with the surrounding faculæ, drawn by M. Chacornac. But the student must remember that the bright and dark parts of the region around the spot present under powerful telescopic scrutiny such details as are seen in Professor Langley's picture, Plate XIII.

(853.) Mr. Dawes detected a rotatory motion in certain spots, as though these regions were the scene of some tremendous solar cyclone or tornado. A spot of enormous dimensions was observed by him to have rotated through half a complete circuit in the course of about six days. Other spots have exhibited even more rapid motion, indicating the existence of solar hurricanes or tornadoes having a velocity of forty or fifty, in some cases even 120 miles per second. Fig. 230 represents a large spot of the cyclonic kind, as drawn in 1858 by Fr. Secchi.

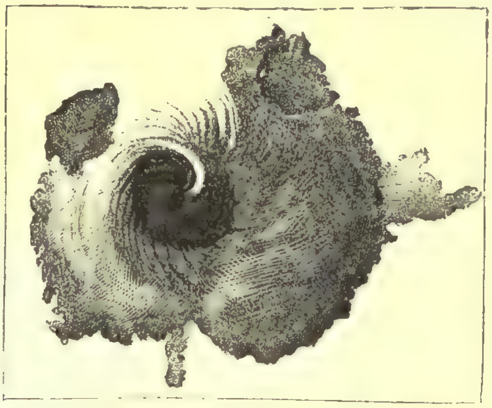


FIG. 230. The great spot of April and May 1858, presenting the appearance of Cyclonic Motion. (Drawn on May 5 by Secchi.)

(854.) Some solar spots have been of enormous dimensions. When we remember that the least spot which could be perceived with the most powerful telescope must have an area of at least 50,000 square miles, it will be understood how immense those spots must be which have been distinctly visible to the unaided eye.

(855.) Pastorff, in 1828, measured a spot whose umbra had an extent four times greater than the Earth's surface. In August 1859, a spot was measured by Newall which had a diameter of 58,000 miles—that is, exceeding more than seven times the diameter of our Earth. But spots of even greater dimensions have been observed. In June 1843 a spot was visible, which, according to Schwabe's measurements, had a length of no less than 74,816 miles. On March 15, 1858, observers of the great eclipse had the good fortune to witness the passage of the moon over a spot which had a breadth of 107,520 miles. In the same year the largest spot of any on record was visible upon the solar disc. It had a breadth of more than 143,500 miles; so that across it no less than eighteen globes as large as our Earth might have been placed side by side. At a very moderate computation of the depth of this solar cavity, it may be assumed that the mass of 100 Earths such as ours would barely have sufficed to fill it to the level of the solar photosphere.

(856.) Of two great spots visible in June 1883, the spot (A) shown in figs. 231 and 232 was of great size, and remarkable for its rapid changes during the later part of its career. But it was far surpassed in size by the great spot B, which at the time of its greatest development had an area of 2,500,000,000 square miles.

(857.) The rapidity with which some spots have changed in figure, or even wholly disappeared, would be wholly incredible were it not that astronomers of the highest repute for accuracy have supplied the records of such changes. Dr. Wollaston saw with a 12-inch reflector a spot which divided into many small spots while he was looking at it. 'I could not expect such an event,' he says, 'and therefore cannot be certain of the exact particulars; but the appearance, as it struck me at the time, was like that of a piece of ice when dashed on a frozen pond, which breaks in pieces and slides on the surface in various directions.' Spots disappear sometimes almost in a single moment, and, conversely, spots of no inconsiderable dimensions have come into existence in less than a minute of time. On one occasion a momentary distraction caused Sir William Herschel to turn away his eyes from a group of spots he was observing; when he looked again the group had vanished.

The spot represented in the figs. 236, 237, and 238 was remarkable both for its enormous size at the time of its greatest development, and for the singularly rapid

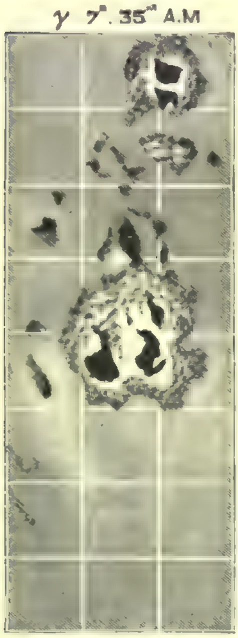


Fig. 231.—Great Sun-spot (A), June 25, 1883, 7h. 35m. A.M. (*Howlett*).

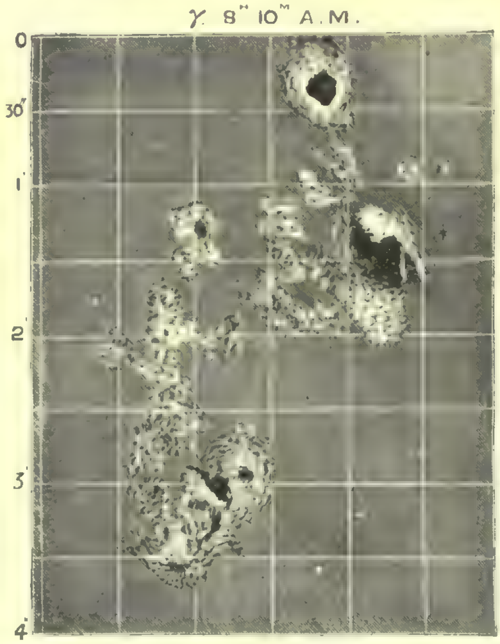


Fig. 232. Great Sun-spot (A), June 29, 1883, 8h. 10m. A.M. (*Howlett*). (This is not the spot just entered on sun's eastern edge in fig. 233.

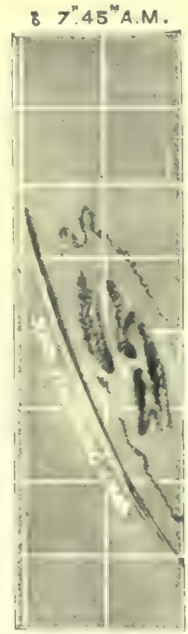


Fig. 233.- Spot (B), June 25, 1883, 7h. 45m. A.M. (*Howlett*).

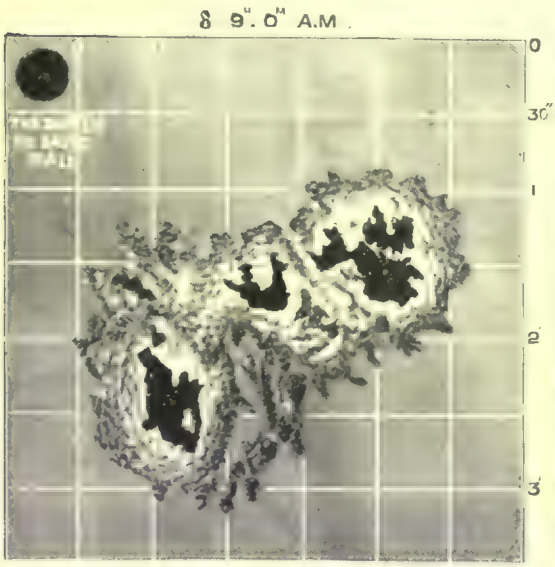


Fig. 234.
Great spot (B), June 29, 1883, 9h. 0m. A.M. (*Howlett*).
Area about 2,500,000,000 square miles.

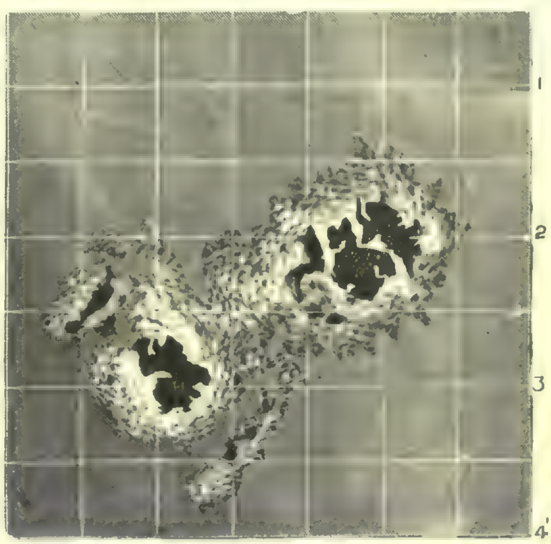


Fig. 235.
Great spot (B), June 30, 1883, 8h. 5m. A.M. (*Howlett*).
Area about 2,200,000,000 square miles.

Note.—**In figs. 231–235 the distance between successive cross-lines represents 30″ at the sun's distance, or at the end of June (when his apparent diameter is 31′ 46″) about 13,720 miles. Earth's mean diameter, 7,914 miles.**

changes which it underwent. Intense magnetic storms accompanied the more remarkable of these changes.

(858.) At this stage it becomes necessary to consider the application of *spectroscopic analysis* to the study of the sun specially, and to problems of physical astronomy generally. I propose to do this, however, only in such degree as seems necessary for the logical completeness of the astronomical matter here presented. It would be easy to fill a large section of this work with the details of spectroscopical methods and with pictures of complicated spectroscopical instruments; meaningless for those who have not worked with such instruments or carefully studied the optical and physical relations involved, but needing no explanation (and useless even as illustrations) for spectroscopists. Here, however, I deem it proper to consider only the essential principles involved in spectroscopic research.

(859.) What we call white light—such light as we get from the sun in a clear sky at midday—is produced by the combination of all those undulatory emanations from

Great Solar Spot of November, 1882 (accompanied by an intense magnetic storm).
(Drawn by the Rev. F. Howlett.)

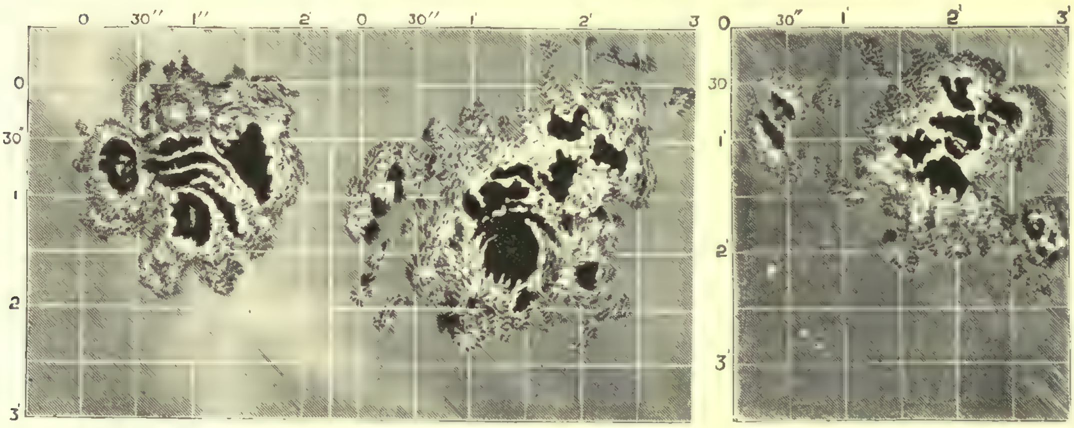


FIG. 236.—Nov. 15, 1882, 10.47 A.M. FIG. 237.—Nov. 19, 1882, 10.20 A.M. FIG. 238.—Nov. 21, 1882, 10.50 A.M

Extreme length, 2′ 12″.	Extreme length, 3′ 10″.	Extreme length, 2′ 0″.
Mean breadth, 1′ 30″.	Mean breadth, 1′ 30″.	Mean breadth, 1′ 30″.
Area about 2,405,700,000 sq. miles.	Area about 3,462,750,000 sq. miles.	Area about 2,187,000,000 sq. miles.

Note.—The distance between successive cross-lines represents 30″ at sun's distance, or in the middle of November (when his apparent diameter is 32′ 26″) about 13,300 miles. Earth's mean diameter, 7,914 miles.

the sun which affect the sense of sight. These range between certain wave-lengths. Just as sound-waves range between the short air-waves, as producing the highest notes, and the long air-waves, producing the deepest notes we can hear, so the light-waves which affect our sight range from the shortest (producing colour recognisable as deep violet in tone), to the longest (producing colour recognisable as deep red). Spectroscopic analysis enables us to sift apart light-waves of different kinds which would reach the eye together unless thus separated, and it also sifts or separates those other emanations outside the range of vision because consisting of waves either too short or too long to affect the sight.

(860.) The prismatic analysis of light (one only of the ways in which light may be sifted into its component waves) is shown in fig. 239, illustrating Newton's celebrated experiment. Here A B represents the course of a pencil of solar light[1] passing through

[1] I have purposely modified Newton's figure, because many misunderstand a figure in which the pencil AB is shown with its proper divergence. They confuse the dispersing effect of the prism with the optical effects produced on a diverging pencil of *pure* light. It need hardly be

a circular aperture in a screen S S′. The prism P is so placed as to intercept the light. It will be well to consider the prism as placed with the base E F uppermost and horizontal.[1]

(861.) If the prism were removed, the light would fall at *i* and make a small elliptical image there. And if the solar light were simple instead of being composed of rays of many different colours, it would follow such a course as is indicated by the bent bright line, and form a small elliptical image at *i′*—this image being white like that at *i*, and resembling the latter image in shape.[2]

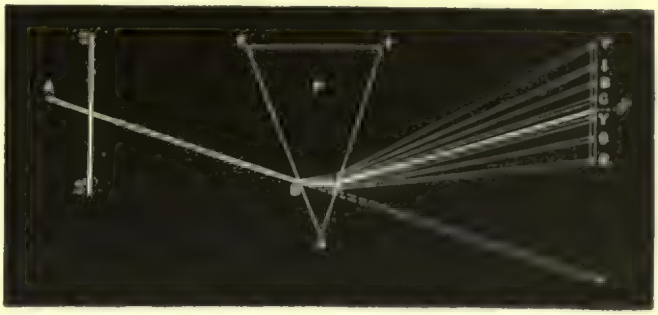


Fig. 239.—Illustrating the Prismatic Analysis of Light.

(862.) But, instead of this, Newton found, as Grimaldi had found before him, that a streak of light was formed as V R (V R is exaggerated in length), the streak being violet at the highest point and thence changing through indigo, blue, green, yellow, and orange to red at the lowest point. Neither above nor below is the streak well defined, but passes gradually into darkness. At the sides, however, the streak is well defined, and in breadth equal to the horizontal breadth of the figure at *i*. It thus forms a rainbow-tinted streak or ribbon.

(863.) It appears from this experiment that light consists of rays of all the colours of the rainbow, that the violet rays are the most bent by the action of a prism, the red rays least, the others in the order named above. This happens with prisms of all refracting angles and of whatever substance. Hence the rays forming the violet part of the spectrum are often called—without further description—the most refrangible rays, while the red rays are called in the same way the least refrangible rays. This way of speaking and the expressions arising out of it should be carefully noted.

(864.) The prism refracts the waves of different lengths in different degrees depending on the properties of the substance of which the prism is made. Thus the length of different parts of a spectrum formed by the refractive action of prisms cannot be regarded as related normally to the wave-lengths of the light. Diffraction, which may be described here (where no study of its true nature and laws can be introduced) as the power which light-waves possess (in different degrees according to their length) of passing round an obstruction, or of being deflected in passing through an opening when the dimensions of either are comparable with the wave-lengths of light of different colours, supplies a means of analysing light according to its wave-lengths. The spectrum thus

said that it would be quite as great a mistake to neglect the effect of divergence altogether. In fact, many important optical considerations are associated with the divergence (however small this may be made) of the pencil analysed by the prism. But the two matters are best kept apart, at least in the case of the beginner. In the above description the object has been to direct the reader's special attention to the dispersive action of the prism, and therefore no attention is paid to the divergence of the pencil.

E G is supposed equal to G F, so that these lines are equally inclined to the vertical.

[2] It would indeed be absolutely identical in shape with the image at *i* if the exact course indicated in the figure were followed; but if the single image fell at V or R this would not be the case. It need hardly be remarked that for pure light the course of the beam would depend on the refracting angle G of the prism.

obtained, called the *diffraction spectrum*, presents the same colours, in the same order as the prismatic spectrum, and also includes those orders of wave which produce no effect, because they are either too long or too short, on vision. As the position of each part of the diffraction spectrum depends solely on the wave-length of light of that colour, the spectrum thus obtained is called also the *normal spectrum*.[1]

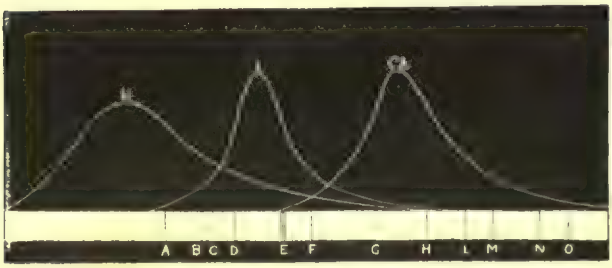


Fig. 240.—Illustrating the Distribution of Heat (H), Light (L), and Chemical (photographic) Activity (C *h*) in the Solar Spectrum (prismatic.)

(865.) When we apply either method of analysis to sunlight, we seem at first to find light of all the prismatic colours in sunlight. But when the light of the sun is admitted through a fine slit parallel to the lower edge of the prism (placed as in fig. 239), so that the image for each colour becomes a fine line, it is found that some tints are missing.

(866.) Dr. Wollaston discovered in this way ('Phil. Trans.' 1802, p. 378) two gaps in the solar spectrum. Fraunhofer, in 1814, saw and mapped no fewer than 576 lines. The positions of the chief lines seen by Fraunhofer are indicated in fig. 241, and as reference is continually made to the lettered lines, it is well that the student should carefully note their sequence and position:—

(867.) A is a well-marked line close to the limits of visibility at the red end of the spectrum. B is a well-defined line of sensible breadth.[2] Between A and B is a band of several lines called *a*. C is a dark and very well-marked line. Between B and C, Fraunhofer counted nine fine lines; between C and D about thirty. D consists of two strong lines close together. Between D and E, Fraunhofer counted eighty-four lines. E is a band of several lines, the middle line of the set being stronger than the rest. At *b* are three strong lines, the two farthest from E being close together. Between E and *b* Fraunhofer counted twenty-four lines, and between *b* and F more than fifty. F, G, and H are strong lines. Between F and G, and between G and H, Fraunhofer counted 185 and 190 lines respectively; and he found many lines also between H and I—the violet end of the spectrum.[3]

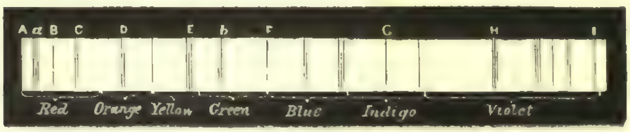


Fig. 241.—The principal Dark Lines in the Prismatic Spectrum.

[1] It is only in the prismatic spectrum that the effects separable (but only as effects) into heat, light, and chemical action (of the kind employed in photography) can be represented in quantity by curves shown in fig. 240, where the height of each curve above any part of the spectrum (for the colours of which see Art. 867, note) indicates the relative quantity of heat, or light, or chemical action in the corresponding part of the spectrum. The heat-curve comes much closer up to the light-curve in the normal spectrum. The late Dr. J. W. Draper was the first to show this.

[2] It will be understood that I am here describing the lines as seen by Fraunhofer, and as they would appear with similar spectroscopic power to that employed by him. With greater power many single lines are resolved into several, and many new lines make their appearance. For example, D is described above as consisting of two strong lines with an extremely small interval. With a powerful spectroscope numerous lines are seen between these two strong lines.

[3] Fig. 241 shows the colours of those parts of the spectrum in which the several lines occur. The reader will do well to bear in mind the posi-

(868.) Fraunhofer examined solar light received indirectly by reflection or otherwise—as from the clouds, the sky, the moon or planets, and so on; and he found in the spectrum of such light the same lines which he had seen in the spectrum of direct solar light. He studied the spectrum of the sun when that orb is near the horizon, and he found that under such circumstances the violet end of the spectrum disappears, and a number of lines make their appearance in the remainder of the spectrum.

(869.) Fraunhofer found that the spectra of the fixed stars exhibit dark lines resembling those in the sun; but none of the stars whose light he examined had a spectrum exactly the same as the sun's.

(870.) Let us next inquire into the spectra given by different terrestrial sources of light.

An incandescent solid or fluid—or, to speak more correctly,[1] a solid or fluid *glowing* with intensity of heat—gives a continuous spectrum. But the nature of the spectrum varies with the temperature of the source. If a piece of metal, for instance, be gradually heated till it is at a white heat, only the red part of the spectrum will at first be visible; then the orange part will show, then the yellow, and so on; until at length the whole range of the spectrum will be seen, from the extreme red to the extreme violet.

(871.) With glowing vapours the case is altogether different. Although there are exceptions to the rule, it may be stated as a general characteristic of the spectra of such vapours that they consist of coloured lines or bands. Sir David Brewster, Sir John Herschel, and Talbot were among the first who examined such spectra. In 1822 Sir John Herschel called attention to the importance of the study of the lines and bands seen in the spectra of the vapours of different elements. 'The pure earths,' he said, 'when violently heated, yield from their surfaces lights of extraordinary splendour, which when examined by prismatic analysis are found to possess the peculiar definite rays in excess which characterise the tints of the flames coloured by them; so that there can be no doubt that these tints arise from the molecules of the colouring matter reduced to vapour, and held in a state of violent ignition.'

(872.) The spectrum obtained from the electric spark includes the spectrum of the gas or vapour through which the discharge takes place, and also the spectrum of the vaporised substance of the conductors. Thus we can obtain not only the spectra of the metallic and other elements[2] not easily volatilised in other ways, but also the spectra of gases—such, for instance, as nitrogen and oxygen—while conditions of pressure, combination, and the like can be introduced, which would not be available under ordinary conditions.

tion of the several lines, as thus, by an easily remembered relation, he will find himself enabled to interpret readily the accounts of spectroscopic researches into astronomical and chemical subjects of inquiry. Let him remember, then, that A, B, and C are in the red portion of the spectrum: D in the orange-yellow; E in the yellowish-green; F in the greenish-blue; G in the indigo; and H in the violet.

[1] The term 'incandescent' is not properly applicable to any source of light which is not actually white. Many spectroscopists indeed go farther, and say that no luminous body ought to be described as incandescent unless its spectrum extends without dark lines or gaps of any sort from the extreme limit of visibility at the red end to the extreme limit of visibility at the violet end of the solar spectrum. Without insisting on this limitation, it certainly does seem well to point out that the term 'incandescent' is not properly applicable to solids or fluids glowing with light belonging to the red end of the spectrum, nor to vapours glowing with light of a well-marked colour.

[2] It seems unnecessary to speak of the spectra *of the vapours* of such and such elements, since in reality it is only as vapours that iron, sodium, and the rest have characteristic spectra at all.

(873.) In this way a large number of elements had their spectra assigned to them, while in several instances physicists began to recognise peculiarities in the spectrum of the same element when examined under different conditions.

(874.) Spectroscopic analysis enables us to determine the specific action of various absorptive media. Brewster found that when ordinary solar light is transmitted through the thick vapour of nitrous gas, a number of new dark lines are seen, parallel to the Fraunhofer lines, and congregated in a remarkable degree towards the violet end of the spectrum. He further proved that these lines were seen whatever the source of light might be. Professors Miller and Daniel made further researches into the effects of vapours in causing dark lines to appear in the spectrum of solar or white light.[1]

(875.) In 1861 Kirchhoff proved that (as Fraunhofer had suspected) the two bright orange-yellow lines of sodium correspond exactly with two dark lines in the orange-yellow part of the solar spectrum. In dealing with this interesting relation he made the important discovery—a discovery which may be described as leading directly to the portal by which science entered into the domain of spectroscopic analysis—that sodium vapour absorbs the same light-tints which it has the power of emitting.[2] It

[1] Some of Professor Miller's results are worth quoting, because they show how closely a physicist may approach a great discovery without actually effecting it. 'First,' he says, 'colourless gases in no case give additional lines, or lines differing from those of Fraunhofer. Secondly, the mere presence of colour is not a security that new lines will be produced; for instance, of two vapours undistinguishable by the eye, one, *bromine*, gives a great number of new lines, while the other, *chloride of tungsten*, exhibits none. Thirdly, the position of the new lines has no connection with the colour of the gas; with *green perchloride of manganese* the new lines abound in the green of the spectrum; with *red nitrous acid* they increase in number and density as we approach the spectrum's blue extremity.' Some of these results, rightly understood, contain the germ of the great discovery afterwards effected by Kirchhoff; since in some of the cases actually experimented on by Professor Miller, the absence of *new* lines meant simply that the absorption lines, corresponding to the element he was dealing with, were coincident with some of the Fraunhofer lines. Others, however, approached the solution of the great problem even more nearly, since they actually touched on the principle of the reversal of the spectral lines, which affords the explanation of the coincidences detected but unnoticed by Professor Miller. 'None of these distinguished men,' says Professor Tyndall, 'betrayed the least knowledge of the connection between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject was Ångström. In a paper translated from Poggendorff's *Annalen* by myself, and published in the *Philosophical Magazine* for 1855, he indicates that the rays which a body receives are precisely those which it can emit when rendered luminous. In another place he speaks of one of his spectra giving the general impression of reversal of the solar spectrum. Foucault, Stokes, Thomson, and Stewart have all been very close to the discovery; and for my own part the examination of the radiation and absorption of heat by gases and vapours would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation.'

Within a year of the time when the great secret was about to be revealed, an eminent physicist wrote thus: 'In quitting the mere phenomena of luminous spectra, and rising to the inquiry as to their causes, we enter a more arduous course. The phenomena defying, as we have seen, all attempts hitherto to reduce them within empirical laws, no complete explanation or theory of them is possible. All that theory can be expected to do is this—it may explain how dark lines of any sort may arise within the spectrum.' Theory soon did much more than this.

[2] The name of Bunsen must be associated with Kirchhoff's in this discovery, though we owe almost wholly to Kirchhoff the interpretation of the dark lines in the solar spectrum. It may be interesting to quote here Kirchhoff's announcement of his discovery to the Berlin Academy: 'Upon occasion of an investigation of the spectra of the coloured flames, conducted in common by Bunsen and me, whereby we were enabled to determine the qualitative composition of complex compounds from the appearance of the spectra of their blow-pipe flame, I made some observations which unexpectedly explain the origin of Fraunhofer's lines, and warrant conclusions from these as to the elemental constitution of the solar atmosphere, and perhaps to that of the brighter fixed stars.'

follows unmistakably that sunlight, which shows these same dark lines, must, before reaching us, have passed through the vapour of sodium. Either then in our own atmosphere or in the atmosphere of the sun, the metal sodium, the base of our familiar salt and of soda, must exist in quantities sufficient to cause the observed absorption-lines.

(876.) But, more than this, those countless other lines which cross the solar spectrum must each indicate some process of absorption exerted by vapours in our atmosphere or his. Kirchhoff did not long delay the inquiry thus suggested. He compared the spectrum of iron (that is, of the luminous vapour of iron) with the solar spectrum. The spectrum of iron as known to Kirchhoff contained sixty bright lines.[1] Kirchhoff found that precisely as the two sodium lines agree with the D lines of the sodium spectrum, so every one of the iron lines had its counterpart in the solar spectrum.[2] Every line of the iron spectrum appears as a dark line in the spectrum of the sun! Kirchhoff reasoned that this cannot be the result of mere chance coincidence. Taking as $\frac{1}{2}$ the chance that a bright line in the iron spectrum would *seem* to have a counterpart in the richly 'lined' solar spectrum if accident were alone in question, it follows that the chance of the observed relation is less than

$$\frac{1}{1,000,000,000,000,000,000}.$$[3]

(877.) The only assignable explanation is that the rays of light which form the solar spectrum have passed through the vapour of iron, and have thus undergone the absorption which the vapour of iron must exert. These iron vapours might be contained either in the atmosphere of the sun or in that of the Earth. But our atmosphere cannot contain such a quantity of iron vapour as would produce the very distinct absorption-lines which we see in the solar spectrum; and this supposition is further disproved by the fact that the iron lines do not appreciably alter when the sun approaches the horizon. We conclude then that, owing to the high temperature possessed by the sun's atmosphere, iron is present in it in the form of vapour, even as the vapour of water is present in the atmosphere of the Earth.

(878.) Kirchhoff found that calcium, magnesium, and chromium exist in the solar atmosphere. The presence of nickel also and cobalt was indicated by the agreement of the most conspicuous of their lines with solar dark lines. All the lines of these metals, however, could not be recognised, nor were the coincidences in the case of cobalt sufficient to satisfy Kirchhoff. 'I consider myself entitled,' he says, 'to conclude that nickel is present in the solar atmosphere; but I do not yet express an opinion as to the presence of cobalt. Barium, copper, and zinc,' he proceeds, 'appear to be present in the solar atmosphere, but only in small quantities; the brightest of the lines of these metals correspond to distinct lines in the solar spectrum, but the weaker lines are not noticeable. The remaining metals which I have examined, viz. gold, silver,

[1] Many more have since been discovered—in fact, there are now more than 450 recognised iron lines.

[2] It is hardly necessary to note, perhaps, that Kirchhoff could not establish this result in the same way as the former, because he could not cause sunlight to shine through the glowing vapour of iron. He employed a method quite as satisfactory, however, causing the light from iron vaporised by the electric spark to form a spectrum side by side with the solar spectrum. The solar light was admitted directly to the battery of prisms; that of the iron was admitted after reflection through a small prism near the slit.

[3] To this I may add that the chance of the observed relation, now that 450 lines of iron are recognised, is less than a fraction whose numerator is unity, and whose denominator consists of no less than 136 figures (the first four being 2,907).

mercury, aluminium, cadmium, tin, lead, antimony, arsenic, strontium, and lithium, are, according to my observations, not visible in the solar atmosphere.'[1]

(879.) The following table, drawn up by M. Angström, exhibits the number of lines belonging to the several elements enumerated, found (by himself and Thálen) to correspond with dark lines of the solar spectrum:—

Hydrogen	4	Manganese	57
Sodium	9	Chromium	18
Barium	11	Cobalt	19
Calcium	75	Nickel	33
Magnesium	4 + (3 ?)	Zinc	2
Aluminium	2 ?	Copper	7
Iron	450	Titanium	200

(880.) To these elements have been added lead, cadmium, strontium, potassium, and a few others, whose presence is indicated by the coincidence of solar dark lines with certain of the lines of elements; but the evidence is doubtful, because the lines in the solar spectrum are now so numerous that the coincidence of one or two solar lines with one or two lines of an element (having, perhaps, many lines in its spectrum) may be apparent only. In fact, cases of supposed coincidence have been shown on adequate increase of dispersive power to indicate no real coincidence, but only a close approach.[2]

(881.) Such evidence as has been obtained in regard to lithium, tin, silver, and indium must at present be held as altogether insufficient to warrant any opinion as to the presence of these elements in the solar atmosphere. As regards gold, platinum, lead, and mercury, there is no evidence at all.[3]

(882.) The general principles of spectroscopic analysis are as follows: –

1. An incandescent solid or liquid gives a continuous spectrum.
2. A glowing vapour gives a spectrum of bright lines, each vapour having its own

[1] I have thus far quoted Kirchhoff, not because the account of his investigations exhibits the actual state of our knowledge at the present time, but because they are so associated with the discovery of the great principle on which spectroscopic analysis depends as to have an interest wholly distinct from that—great as it is—which they derive from their intrinsic importance.

[2] Mitscherlich has shown, however, that we may in some cases accept with a certain degree of confidence the evidence given by the coincidence of one or two lines of a many-lined spectrum with solar dark lines, because under special conditions only one or two lines (of such many-lined spectra) show themselves.

We must remember, however, in forming an opinion as to these elements (as also respecting such elements as carbon, boron, silicon, and sulphur), that while the presence of certain lines in the solar spectrum may prove abundantly that the terrestrial element which has corresponding bright lines exists in the sun's substance, it by no means follows with equal certainty that because all the lines of an element are wanting in the solar spectrum, therefore the sun does not contain those elements. This will appear when we consider the various circumstances which may cause an element really existing in the sun's substance to afford no trace of its presence. In the first place, the vapour of that element may be of a density causing it to lie always at a very low level, and therefore perhaps altogether beneath that level whence proceeds the white light of the sun—that is, the light which gives the continuous spectrum across which the dark lines lie. Or the element may exist in the sun's substance at such a temperature, or at such a pressure, as to produce—not well defined absorption-lines, but only broad faint bands, which no optical means we possess can render sensible as such. Or again, the element may be in a condition enabling it to radiate as much light as it absorbs, or else very little more or very little less—so that it either wholly obliterates all signs of its existence, whether in the form of dark lines or bright lines, or else gives lines so little brighter or darker than the surrounding parts of the spectrum that we can detect no trace of their existence. In these, and in yet other ways, elements may really exist (or rather undoubtedly do exist in the sun) of whose presence we can obtain no trace whatever.

set of bright lines (between certain limits of pressure and density), so that from the appearance of a bright-line spectrum we can infer the nature of the vapour or vapours whose light forms the spectrum. But the lines, or some of the lines, of a vapour change with changes of pressure or temperature or both; increase of pressure usually widening the lines (especially the more refrangible), change of temperature often modifying the spectrum altogether.[1]

3. An incandescent solid or liquid shining through absorbent vapours at a lower temperature gives a rainbow-tinted spectrum crossed by dark lines, these dark lines having the same position as the bright lines belonging to the spectra of the vapours.

4. Light reflected from any opaque body gives the same spectrum as it would have given before reflection.

5. But if the opaque body be surrounded by vapours, the dark lines corresponding to these vapours appear in the spectrum with a distinctness proportioned to the extent to which the light has penetrated these vapours before being reflected.

6. If the reflecting body is itself luminous, the spectrum belonging to it is superadded to the spectrum belonging to the reflected light.

7. Glowing vapours surrounding an incandescent body will cause bright lines or dark lines to appear in the spectrum according as they are at a higher or lower temperature than the body; if they are at the same temperature, they will emit just so much light as to compensate for that which they absorb, in which case there will remain no trace of their presence.

8. The electric spark presents a bright-line spectrum, compounded of the spectra belonging to the vapours of the conductors between which and of those of the gases through which the discharge takes place. According to the nature of these vapours, and of the discharge itself, the relative intensity of the component parts of the spectrum will vary.

It will be seen, as we proceed, that all these principles bear more or less directly on the application of spectroscopic analysis to the interpretation of solar phenomena.

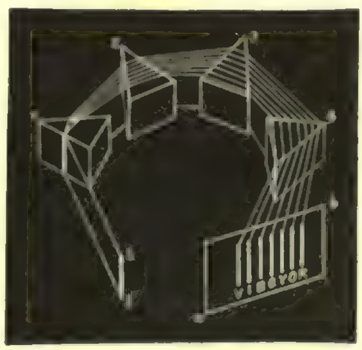


Fig. 242.—Illustrating the Dispersive Action of a battery of Prisms.

(883.) In the experiment illustrated in fig. 239 we see the action of a single prism on the solar light. In most spectroscopic researches the observer has to work with a battery of prisms. The action of such a battery is illustrated in fig. 242. Suppose S S′ to be a slit through which a beam of parallel rays falls upon prism 1, which disperses this beam in the manner already described. The beam falls thus dispersed on prism 2, and is further dispersed; then on prisms 3 and 4, undergoing increased dispersion in passing through these prisms. Therefore, if allowed now to fall on a screen *a b*, or if the emergent rays are received by the eye, either with or without telescopic aid, the spectrum seen is much longer than where one prism only is used.[2]

[1] For instance, the spectrum of sulphur vapour, which is continuous up to 1,000°, changes at higher temperature to a spectrum of bright lines.

[2] On *a b*, in fig. 242, I have shown a violet image of the slit V, an indigo one I, and so on, to a red image at R. In the solar spectrum an infinite number of images are formed, ranging from the extreme violet end to the extreme red end. In places, however, no images are formed, the spaces thus left without light being the dark lines.

It is not difficult then to see on what conditions the visibility of the dark lines will neces-

(884.) In practice that part only of the spectrum is usually examined which has passed through the prism or battery of prisms—as the part corresponding to i' in fig. 239, and to G in fig. 242, has passed—undergoing equal refraction at each face, an arrangement which is essential, or at least extremely desirable,[1] so far as the clear definition of the lines is concerned.

(885.) Let us consider how these difficulties have been or may be encountered.

For this purpose a battery of prisms may be mounted as shown in fig. 245, so that when the last prism is moved the others move with it, retaining their symmetry of adjustment, the first prism being automatically adjusted along with the rest, by means of a fixed slot not shown in fig. 245, but separately shown in fig. 246.

(886.) So far as clearness of definition and the satisfactory study of the whole length of the spectrum are concerned, this arrangement leaves nothing to be desired. But sometimes a greater increase of the spectrum's length is needed than this arrangement permits. For, as we see from fig. 247, the limits of dispersion which one circular battery can give are reached so soon as the total deviation amounts to four right angles.

sarily depend. If we could have a slit which was a true mathematical line, every dark space would be present in the screen, even though the dispersive power were small. But, as a matter of fact, the slit has a definite breadth, however

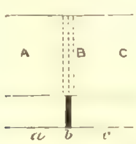


FIG. 243.

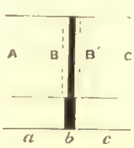


FIG. 244.

Illustrating the effect of increased Spectroscopic Dispersion in purifying the Spectrum.

narrow we may make it. Now, suppose that A B C (fig. 243) represents a small part of the solar spectrum as shown on the screen in fig. 239, but that the *true* nature of this part of the solar spectrum is shown in the narrow band *a b c*, so that in reality sunlight has no rays whose refrangibility corresponds to the band at *b*. Suppose further that the aperture of the slit is equal in width to this band *b*. Then the light corresponding to the extreme limits of the light in *a b c* will form the two images of the slit shown at B; these images will meet, and no absolutely black line will be seen. There will, however, be a dusky band, darkest down the middle, and twice as broad as the true band *b*. But now consider the effects of an increase of dispersive power. Say we double the length of the spectrum, or rather of the particular part shown in figs. 243 and 244. Then the parts *a b c* of the real solar spectrum will all be doubled in length, and in the observed solar spectrum the light corresponding to the extreme limits of the band *b* will produce the two images of the slit shown at B and B′, but these will be no wider than before, and will be separated by a really black band, half as wide as *b*. This band will be bordered by a penumbral fringe whose boundaries are indicated by the dotted lines, the whole breadth from dotted line to dotted line being half as great again as that of the band *b*.

We perceive then the importance of increasing the dispersive power of our battery of prisms; since in this way very fine lines which might otherwise escape detection can be rendered visible. Also, it is obvious that two lines very close together would be shown as one with a certain amount of dispersive power, while with more dispersive power they would be clearly separated. Moreover, a line in the solar spectrum which *seemed* to coincide, without being really coincident, with the bright line of some metallic spectrum (brought into comparison with the sun's in the manner referred to above) might, by increase of dispersive power, be removed appreciably from its supposed counterpart.

[1] It may be shown that in this case those special rays pass with least possible deviation through each prism, so that the condition is generally called that of *minimum deviation.* Minimum deviation *per se* has no advantages; but by this arrangement the primary and secondary foci of emergent pencils are brought as nearly to coincidence as possible; so that, though the image of the slit formed by those special rays is not formed by absolute points of light, it is formed by circles (technically called 'circles of least confusion') having the smallest possible diameter. In a paper in the *Monthly Notices of the Astronomical Society*, vol. xxx., I have shown that the circle of least confusion has in this case a definite, though exceedingly minute, diameter, even in the case of a single prism. The mathematical expression for the radius of this circle is given in that paper, but is somewhat too complex for these pages.

(887.) One way in which this can be done is by the use of what are called direct-vision prisms. In these, a flint-glass prism is combined with two crown-glass prisms,

FIG. 245.

FIG. 246.

Illustrating the Automatic Spectroscope.

FIG. 247.—Complete circuit of Light through a battery of Prisms.

or two flint-glass prisms (F, F), as shown in fig. 248, with three crown-glass prisms (C, C, C). The prisms C cause deviation and dispersion in one direction; the prisms F cause equal deviation and more dispersion in the contrary direction. Hence results a balance of dispersion without deviation; and if we add such a prism to any battery of prisms, we get all the advantage of the dispersion without increasing the deviation which had been our difficulty.

(888.) Yet there is one important disadvantage in direct-vision prisms, more especially when they are employed in researches requiring very neat and exact definition: it is, of course, wholly impossible to employ any method for securing minimum deviation.

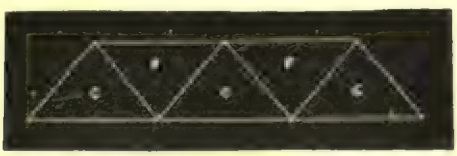


FIG. 248.—A Direct-vision Spectroscope.

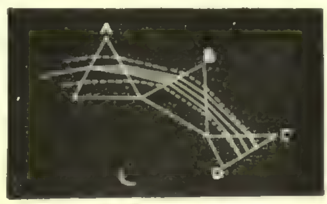


FIG. 249. Illustrating the return of Rays through a battery of Prisms.

(889.) The dispersion in a battery of prisms may be doubled by carrying the light both ways round the battery. Thus, suppose A and B (fig. 249) to be two of the ordinary triangular prisms and C a right-angled prism half the size of the others, then the rays which fall on C D are reflected back again through the battery. The emergent pencil can be viewed in such a case by a telescope in which there is a totally reflecting prism sending the light out at right angles to the axis of the tube. According to another arrangement, in place of such a prism as C, one is employed which by two total reflections carries the rays to a different level; so that (the prisms of the battery being twice as high as usual) rays which have entered and passed round the battery along the lower halves (say) of the prisms, pass back and emerge from the battery along the upper halves (or *vice versâ*).

(890.) By the plan illustrated in fig. 250, dispersion equal to that given by nineteen triangular prisms is combined with a perfectly true automatic adjustment for all parts of the spectrum. The light A B incident on the first battery passes, as shown by the triple set of lines, through the double set of prisms, the dotted return-lines showing the course of the return-rays to emergence at C C′. The intermediate quadrangular prism D E belongs to both parts of the double battery. There is no loss of light in passing from one battery to the other, since the reflection at D D′ is total. The

close double lines show the direction of the slotted bars, E F being a long slotted bar kept square to the rays leaving the face D′E by a curved slot in the face of the metal plate on which the double battery works.[1] (It pivots round E and slots at F.) The figure shows the arrangement of a battery having an effective dispersing power equal to that of nineteen equilateral prisms of heavy flint-glass.

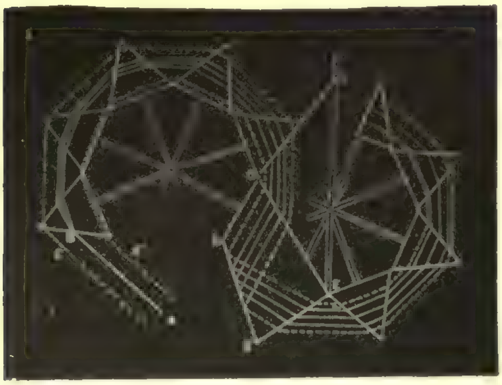


Fig. 250.—The author's double and twice acting battery of Prisms.

(891.) One other detail remains to be considered before we deal with the application of spectroscopic analysis to solar phenomena—the use, namely, of this analysis in measuring rapid motions of approach or recession.

The colour of light corresponds with the tone of sound, a pure colour of the spectrum with a simple tone, a colour formed by the combination of pure prismatic colours with a chord in music. Now if a hearer is approaching or receding from a source of sound, either by his own motion or by the motion of the sounding body, the tone or tones received seem different from those emitted. More sound-waves are obviously received in a given time if the ear and the source of sound are approaching than if their distance remains unchanged. So far as the hearer is concerned this corresponds to an apparent shortening of the sound-waves; for as sound travels a given distance in a given time, the more sound-waves are received in that time the more are distributed over that distance, and therefore the narrower they must individually be. And *vice versâ*, if the distance between the ear and the source of sound is increasing, the sound-waves appear—to the hearer—lengthened. In the case then of approach each tone emitted by the sounding body seems raised, in the case of recession each tone seems lowered. Where the approach or recession is slow the difference is not readily noticed; but where it is rapid the effect is marked; and where rapid approach is quickly followed by rapid recession the change of tone is often very striking.[2] That the change may be clearly recognised the velocity of approach or recession must bear an appreciable relation to the velocity with which sound travels in air—about a quarter of a mile per second.

(892.) Manifestly a similar change must take place with the colour of light, if the observer and the light-emitting source are approaching each other or receding from

[1] G F and G E must be equal, a relation which determines the figure of the curved slot. I have given the equation to the curve (transcendant, naturally) in the *Monthly Notices of the Astronomical Society*, vol. xxv. It may interest the reader to learn that the first time I ever saw a solar prominence it was through an instrument on the above plan constructed by Mr. Browning for the late Mr. Spottiswoode, and lent by the latter to Mr. Huggins, in whose observatory I found the instrument at work. This shows, at any rate, that theoretical considerations, aided by mathematics, may lead to correct practical results even at a first trial; for my plan was described and published several months before it was thus successfully tested. Albeit, as a rule, it is well for the theorist to be prepared to find that a plan which seems sound theoretically may require modifications in detail when put into practice.

[2] Such change of tone is singularly marked when an engine whose whistle (or in America the bell) is sounding rushes swiftly past a hearer in a swiftly advancing train. There may be a change of a whole tone occurring within the fraction of a second during which the rapid approach of the sounding whistle or bell changes into an equally rapid recession.

each other—whether through the motion of one or of the other or of both—with a velocity measurably comparable with that of light. If we suppose, for instance, a mass of glowing sodium approaching an observer with such velocity that the length of the light-waves would be apparently reduced from that corresponding to the orange light (practicably monochromatic) of sodium to that corresponding to the middle yellow of the solar spectrum, the glowing sodium would appear yellow instead of orange; and on the contrary, if the glowing sodium were receding with sufficient velocity, its colour would seem to be red. A change of colour such as this would only occur if, *first*, the velocity of approach or recession were measurably comparable with the enormous velocity—186,300 miles per second—of light; and *secondly*, the light of the glowing body were monochromatic. Doppler, who suggested that the changes of colour observed in certain stars, and especially in certain double stars, might be explained by changes in the direction of their motion, overlooked the fact that bodies like the stars, which shine with all the colours of the spectrum, and also doubtless emit heat-rays and chemical rays outside the range of the visible spectrum, would not change in colour through the effects of changes in the direction of their motion; for if the change (through approach) were such as to alter red rays to orange, orange to yellow, and so on, indigo changing to violet and violet rays to chemical rays not affecting sight, heat-rays, which before had not affected sight, would be changed into red rays, completing the spectral gamut, and leaving the colour of the star unaltered; while, on the other hand, if the change (through recession) caused the violet rays to become indigo, the indigo blue, and so on, the orange becoming red, and the red changing into heat-rays not affecting sight, chemical rays, which before the change had not affected sight, would alter into violet rays, and again the whole range of the spectral gamut would remain. But though such changes would not affect the colour of an incandescent body glowing with all the spectral colours, they would manifestly affect the position of the spectrum itself; and the lines in the spectrum, tested by the position of the known lines of one of the elements present in the sun—as sodium, hydrogen, or magnesium—would exhibit a manifest displacement. Although velocities so great as to alter orange light to yellow light through approach or to red light through recession, have, so far as we know, no existence in nature, yet velocities measurably comparable with the velocity of light exist, and thus in certain cases the spectral lines are measurably displaced. Displacement towards the violet end of the spectrum indicates the approach of the source of light, displacement towards the red end indicates recession. The amount of either displacement, if it can be measured as well as merely recognised, indicates the rate of approach or of recession in miles per second. A velocity of twenty or thirty miles per second—that is, a velocity ranging from $\frac{1}{9000}$th to $\frac{1}{6000}$th of the velocity of light—can be measured in this way, that is, by noting the amount to which any known line is displaced.[1]

[1] Many who recognise quickly how the approach or recession of the eye with respect to a source of monochromatic light will affect the colour, seem to find a difficulty in understanding how the approach or recession of the source of light to the eye can produce a similar effect. Perhaps the following discussion of the corresponding and more easily apprehended relations in the case of sound may help to remove the difficulty. It is from my *Universe of Stars*, in which work will be found several essays on matters which here I can only treat lightly. I wish I could in like manner refer readers to my *Essays on Astronomy*, but that volume is now out of print, and not likely to be reprinted.

[I believe the first public suggestion of the principle on which the spectroscopic measurement of the velocity of rapid motions of approach and recession depends—that is, the recognition of displacement of the lines of known elements in spectra—was made by myself in an article which appeared in *Fraser's Magazine* for January 1868

(893.) It must be remembered that what the spectroscope really does is to give a range of pictures of whatever luminous object or part of an object would be visible through the slit if the spectroscope were removed. Thus, supposing such a portion of the solar photosphere is observed as is shown in the space *s s′*, fig. 251 (S being the solar disc), the images of this portion will give us the spectrum R V, showing the dark lines due to the absence of the images corresponding to specific tints. We are analysing under such circumstances the part *s s′* of the disc, and no other part.

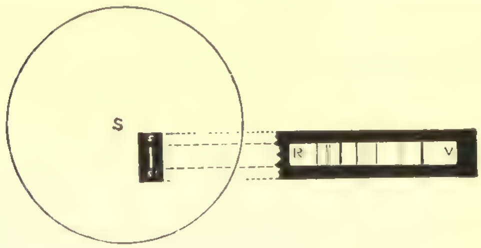


Fig. 251.—The selective Spectroscopic Analysis of a part of the Sun's surface.

(894.) If the slit *s s′* includes part of a spot as shown (on an enlarged scale) in fig. 253, then the space included by the slit, seen separately at S S′, will have for spec-

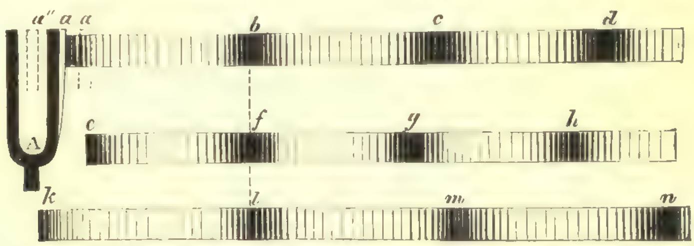


Fig. 252.—Illustrating the effect of approach or recession in modifying the tone of Sound and the colour of Light.

(written in the autumn of 1867). Of unpublished suggestions I know nothing. But I believe Messrs. Huggins and Secchi, possibly others, had been independently at work on researches involving this principle for some time before the spring of 1868, when the first results of such researches were published.

Let fig. 252 represent a series of sound-waves generated by the vibrations of the tuning-fork A. When the right-hand prong is at *a* (the limit of a vibration), *a* is a place of aërial condensation; the next such place is at *b* (*a b* being the *wave-length* corresponding to the vibrations of the tuning-fork), the next at *c*, the next at *d*, and so on. The *wave-amplitude* is measured by the degree of the aërial condensation at *a*, *b*, *c*, *d*, &c., but this does not here concern us. The tone of the sound depends on the wave-length *a b*, and a given turning-fork will cause aërial waves of a particular length (that is, will give out its proper tone), *if it be at rest.*

But now suppose that during the interval which sound would occupy in travelling from *a* to *b*, the tuning-fork has been moved so that the prong *a* is at *a′*. During the interval the prong has made one complete vibration, and *a′* is now therefore a region of condensation instead of *a*; *b* is, of course, a region of condensation, just as it would have been if the fork had been at rest. Hence the wave-length has been reduced to *a′b*; and as all the waves proceeding from the neighbourhood of the vibrating fork are similarly affected, there results a series of waves, *e f*, *f g*, *g h*, &c., as in fig. 252.

On the other hand, if the fork had been moved in the opposite direction, there would have resulted the series of waves *k l*, *l m*, *m n*, &c., represented in fig. 252.

In the former case the tone of the resulting sound appears more acute, in the latter more grave, than it really is. We see also that the velocity of approach or recession should bear an appreciable proportion to the velocity of sound: in other words *a a′* or *a a″* must bear an appreciable ratio to *a b*. This is obvious, since what is wanted is, that *e f* or *k l* should differ appreciably from *a b*.

If we only suppose the vibrating end *a* of the fork to be a particle whose vibrations are generating light of a particular wave-length—that is, of a particular *colour*—we see that the reasoning we have applied to sound-waves must be equally true of these light-waves. If the source of light be approaching us, through its own motion, or ours, or both, the waves will seemingly be shortened; and if the source of light be receding, the waves will be lengthened. In other words, there will be in either case a change of *colour*—the change being towards the violet end of the chromatic scale in the former case, and towards the red end in the latter.

trum a number of images of S S′ ranged side by side, so as to form a strip, as R V in fig. 251. Hence at the top and bottom of this compound spectrum there will be two narrow solar spectra corresponding to the parts S P and S′P′; next to these will be two narrow spectra of the penumbral parts P U and P′U′; and about the middle a narrow spectrum corresponding to the umbral part U U′; all these spectra forming one compound spectrum, whose red end is towards the left (assuming the dispersion to be as in the case illustrated in fig. 251), and its violet end towards the right. By comparing these spectra with the adjacent solar spectra the spectroscopist is enabled to form an opinion as to the nature of the spots, and to make inferences as to the general physical constitution of the sun.

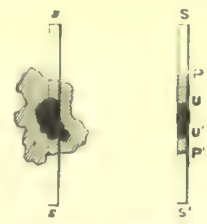


Fig. 253. -Illustrating the Spectroscopic study of a Sun-spot.

(895.) Similar remarks apply to the case where a portion of a facula, or pores, or mottlings, or any other features of the solar disc, fall within the space $s\,s'$. All such peculiarities tend to produce peculiarities in the resulting compound spectrum; since the image of the portion $s\,s'$ is repeated along the whole length of the spectrum R V after the fashion already described.

(896.) The spectroscopic analysis of a spot and its surrounding faculæ has led to results of great interest and significance, but too complex to be simply or readily interpreted.

In the first place we find that the darkness alike of penumbra and umbra is due in the main to general absorption; for the spectrum of each of these portions of a spot shows the rainbow-tinted streak seen in the spectrum of the photosphere reduced in lustre, but complete from end to end.

(897.) But a portion of the reduction of lustre in the penumbra and umbra is shown to be due to increased selective absorption, since many of the dark lines in the spectrum of the photosphere are widened in the spectrum of the umbra, and a few in that of the penumbra; while dark bands are seen, which if they exist in the spectrum of the photosphere are not discernible. With regard to some of these bands we may safely conclude that they are not new bands, but simply become visible in the spot-spectrum because the conditions are more favourable for visibility than in the spectrum of the photosphere.[1] Others, however, are unquestionably due to the absorptive

[1] This applies especially to those bands which Father Secchi regarded as indicating the presence of aqueous vapour in the solar spots. He had found that these bands when seen in the solar spectrum invariably indicated that the part under examination was in the neighbourhood of a spot; but on one occasion he found them clearly recognisable in the spectrum of the photosphere seen through a cirrus cloud. Hence he inferred that they are due to aqueous vapour (which is present in cirrus clouds, though most of the water in such clouds is in the form of ice-crystals). It is impossible, however, that steam can exist in the solar atmosphere, the temperature of which is everywhere far above that at which water is dissociated into hydrogen and oxygen. We must seek, then, another explanation. We may readily find one in the reduced luminosity of the spectrum of a sun-spot, which would render the detection of delicate absorption-bands much easier than on the brighter background of the photospheric spectrum. The amount of light absorbed by aqueous vapour (or by water-particles in whatever form) present in the upper air, is a certain

action of vapours within the spot-region, existing there at higher pressure and lower temperature than outside the photosphere.

(898.) Thirdly, bright lines are occasionally seen in the spectrum of the umbra and penumbra, while in the spectrum of a facula or of one of the bright bridges seen across a spot, the lines of hydrogen are nearly always bright—or, technically, are *reversed*. This, of course, shows that the hydrogen around and above these parts is hotter than the photospheric matter. In the penumbra the lines of hydrogen are often wanting altogether, a peculiarity which must certainly not be interpreted as indicating the absence of hydrogen, but only that the hydrogen in the penumbral region at these times is at such a temperature as to absorb neither more nor less light than it emits.

(899.) It is noteworthy that the relative strength and breadth of the different lines of the same elements are differently affected in the spot-spectrum. Thus a few lines only of the many-lined iron spectrum are strengthened. Different spots differ as greatly in regard to the chemical and physical constitution indicated by their spectra as different volcanic eruptions on the Earth differ in regard to the nature and condition of the substances ejected. This is shown not only by the different relative strengths of the lines of different elements in various sun-spots, but by the different relative strengths of the lines of one and the same element. It seems impossible to classify these diversities, even as it would be impossible to classify the observed proportions of the various substances ejected in different volcanic eruptions. The substances whose lines are most commonly affected in spot-spectra are iron, calcium, cobalt, chromium, manganese, titanium, copper, nickel, zinc, barium, and tungsten. At times of very great solar activity, as indicated by sun-spots, there are seen in spot-spectra widened lines which have not yet been recognised as belonging to known terrestrial elements. On the whole the evidence given by the spectroscopic analysis of sun-spots appears to confirm the theory suggested in 1867 by Dr. Sterry Hunt[1] (see proceedings of the

definite reduction of the solar light, and this reduction bears of course a higher proportion to the fainter light of a spot than to the brighter light of the photosphere ; hence the absorption is sensible in one case, insensible in the other. This would apply also to faint bands or striæ not telluric in origin, but really belonging to the solar spectrum.

[1] This view was subsequently developed by Prof. F. W. Clarke, then of Cincinnati (now of Washington), in a paper on 'Evolution and the Spectroscope,' *Popular Science Monthly*, New York, for January 1873, in which he endeavoured to explain the increasing complexity of stellar spectra, as we pass from white stars like Sirius and Vega to yellow stars like Capella and our own sun, and so to orange, orange-red, and red stars, by a process of development of matter from absolutely elemental forms such as are not known (at present at any rate) in terrestrial chemistry. A few months later a paper, which was little more than a translation of Professor Clarke's, was submitted by an ingenious spectroscopist to the French Academy, and was for awhile regarded as an original contribution to science. But later Dr. Sterry Hunt called the attention of the Academy to the facts of the case. Professor Clarke's application of Dr. Hunt's general theory is of course as original as that theory itself, and though neither can be regarded as established, yet they are both of great interest, and probably sound.

Royal Institution for the report of a lecture on the Chemistry of the Primeval Earth, May 31, 1867) that dissociation by intense heat might resolve the so-called elements into more elemental forms.

(900.) Fig. 254 presents a view of the spectrum of a portion of the sun's surface, including (1) a bright bridge across a spot, (2) a portion of the umbra, (3) the penumbra, and (4) (not numbered in the figure) a part of the photosphere. It affords a good idea of the general nature of the spot-spectrum; but the student is not to suppose that the same spectrum is obtained from different spots. In this particular case the lines of iron and calcium are widened in marked degree; the lines of sodium also are strengthened, whence we may infer that the vapours of these elements are present in large quantities, at high pressures, and at relatively low temperatures. The lines of hydrogen are somewhat stronger across the greater part of the umbra's breadth, but reversed over the rest. This reversal shows that, where it is recognised, the hydrogen above the umbral region is hotter than the vaporous matter (including hydrogen) within that region. As this is the part bordering on the bridge, over whose whole breadth the hydrogen is reversed, we may assume that the intensely hot hydrogen in the region of the bright bridge extended above a part of the umbral region in this place.[1]

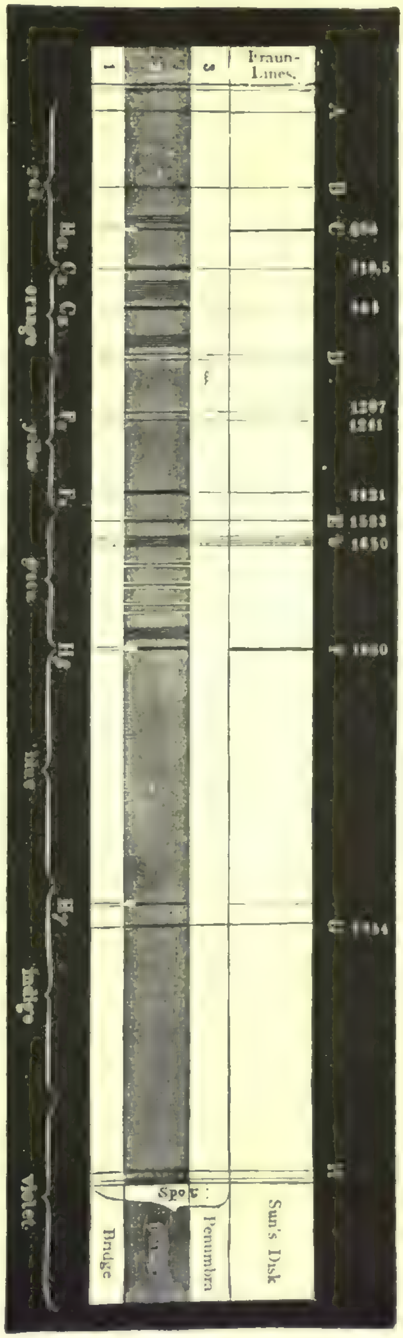


Fig. 254.—Spectrum of part of the Solar Spot of April 11-13, 1869.

(901.) Fig. 255 illustrates other peculiarities which make their appearance when a spot is brought under spectroscopic examination. Here the length of the spectrum (only a small portion of which is shown in the figure) is horizontal, so that the vertical lines are the dark lines of the spectrum. Some of the horizontal lines are due to particles of dust between the jaws of the slit; they show how fine the opening of the slit is; for, of course, only the minutest dust particles can be present in this way. But the horizontal bands correspond to those parts of a spot where a general absorption takes place. All the spectral lines were, on this occasion, visible across these parts of the spectrum. The F line (belonging to

[1] The double fine lines seen on the bridge-spectrum opposite H_α, H_β, and H_γ are not to be understood as indicating double dark lines, but the presence of bright lines instead of the usual dark lines of hydrogen. The other five dark lines along the bridge spectrum are to be interpreted in the usual way.

hydrogen) was, however, peculiarly affected across the whole region of the spot. At the upper and lower extremity we see it of its normal width, while over all the remaining breadth of the spectrum, except two small portions, it is much broader and has shaded edges. In one place it is bent; along another part of its length a narrow line of light is seen to be almost centrally placed upon it; and lastly, in two places it appears bright and irregularly shaped.

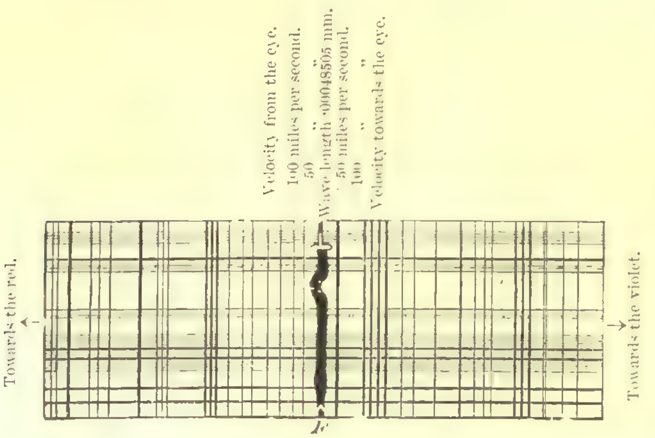


Fig. 255.—Illustrating the changes in certain lines in the spectra of Sun-spots.

(902.) These peculiarities may be interpreted as follows:—

Beginning with the top of the line F in fig. 255, we have first the normal black line showing that in the part of the sun included within the uppermost part of the slit the hydrogen was in its ordinary condition as respects temperature; that is, it was relatively cool. Next we come to a bright hydrogen-line of the normal width on a shaded background. The hydrogen then over this part of the sun was hotter than the solar surface (part of a spot) below it, so that in this place the hydrogen radiated more light than it absorbed, and the line F was rendered relatively bright. Next is a region where the hydrogen-line is still bright but very much wider. Over this part of the sun, therefore, the hydrogen was not only at a higher temperature but at greater pressure. Then we come to the widened dark line, indicating that over the corresponding portion of the sun there was hydrogen at a relatively low temperature and at an abnormally high pressure. The bend towards the left—that is, in the direction of the red end of the spectrum—indicates that the corresponding portion of the hydrogen-envelope was moving from the eye,[1] or, in other words, that there was in this part of the sun a downrush of hydrogen (the line by its narrowing gives evidence of the intenser heat resulting from the increased pressure due to this downrush). Where we see a relatively bright line superposed on the dark one, we learn that above the compressed hydrogen at a relatively low temperature there was hydrogen at less pressure and more heated. Lastly, where we see the pointed dark line close to the bottom of the figure, we learn that above hydrogen as heated as the general radiating substance of the sun, or even hotter, there was the usual layer of hydrogen at lower temperature, very shallow where the line was pointed, but deepening within a short distance to its normal condition.

(903.) Fig. 256 illustrates a yet more remarkable case of disturbance, observed by Professor Young, then of Dartmouth, N.H. The C-line of hydrogen was, in this case, under spectroscopic study. The irregular dark streaks on the right of the main line (itself irregular) indicate velocities of uprush ranging to 320 miles per second, a velocity

[1] The vertical dotted lines on either side of F indicate how far the line F should be shifted to indicate a velocity of 50 and 100 miles per second from or towards the eye. The decimal figures between the vertical lines numbered indicate the length of the light-waves (in parts of a millimetre) corresponding to the part of the spectrum where the line F is situated.

rarely exceeded. Where the line of hydrogen is double or treble, we see that these velocities belong to layers at different levels, the upper moving most rapidly.

(904.) Hydrogen is not the only element whose lines exhibit such peculiarities. The lines of sodium, magnesium, barium, and other elements have been observed to exhibit similar indications of violent action, rapid motion, and remarkable changes of pressure.

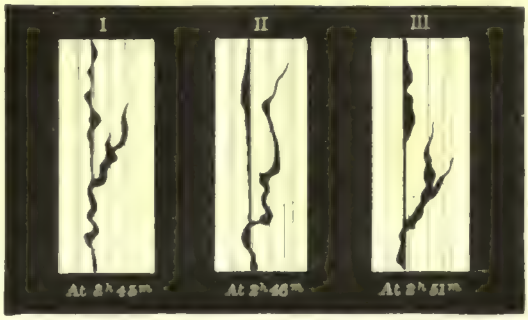


FIG. 256.—Changes in the C line of Hydrogen, September 22, 1870 (*Young*).

(905.) The evidence thus far considered, or rather sketched, though insufficient to determine in detail the nature of the processes at work in the sun, enable us to pass somewhat confidently to certain general conclusions, and in particular to decide the question whether spots and faculæ are the results of disturbances originating outside the sun or from within. The evidence is, I think, decisive in favour of the latter view, though most solar observers favour the former.

(906.) The solar photosphere seems to consist of enormous luminous masses, resembling clouds, the capitals of ascending columns of glowing vapour. The vapours rise in streams from vast depths, and as they approach the radiating region called the photosphere undergo processes of condensation—or perhaps of association from more elemental forms into what we call the elements. Rising rapidly in temperature in the process, these clouds become the source of the chief portion of the solar light and heat.

(907.) The outer solar regions appear to be subject to constant fluctuations, by which the 'clouds,' which otherwise would be uniformly distributed, become so diversely variegated in arrangement as to produce the appearance of mottling described in Art. 798. Where the irregularity of distribution is still more marked we get aggregated masses of these clouds producing the appearance of bright streaks or faculæ.

(908.) These disturbances are probably produced in the main from below. The evidence which shows that the sun's mass is greatly condensed towards the centre (Arts. 787–792) would of itself suffice to render this probable. But when we consider the phenomena of sun-spots we are forced to this conclusion.

(909.) I know that solar students whose opinion in matters physical must always be regarded as of weight[1] have spoken of the darkness of the

[1] Professor Young has advanced a theory of the sun as a kind of bubble, vaporous everywhere except in the photospheric outer region, which he regards as liquid. A careful analysis of this speculation, from a favourable point of view, in an article called 'The Sun a Bubble,' led me eventually to reject it. As advanced later by Professor Young, and associated with the argument

spots, and the evidence thus unquestionably given of relatively low temperature, as suggesting that the matter within the spot-cavities must have been brought from without. Matter arriving from without would doubtless be cooler than the photosphere, until exposed to the compression which (according to this theory) it would experience so soon as it reached the photospheric region. But this compression must result in the immediate generation of intense heat. The heat thus generated would be far greater than the heat required to raise the approaching cooler masses to the temperature of the photosphere. Hence the spots would be bright, not dark, if the matter occupying their immense areas had been brought from without. This is no mere theoretical conclusion; it follows demonstrably from known physical laws. Moreover, the lower and more contracted parts of spots would be brighter than the upper, the narrowing at the lower levels indicating increased pressure and consequently the development of greater heat. The darkness of the spots, undoubted evidence as it is of relative coolness, affords as decisive evidence of expansion, and consequently of the movement of vapour from below upwards, as do the snow-covered summits of terrestrial mountain ranges. This again is no merely theoretical conclusion, but practically certain. There is no available explanation of a cooling so marked (as shown by the spectroscopic evidence) and affecting so widely extended a region and such vast masses of vapour, save as resulting from expansion.[1]

(910.) Although many students of science who must be familiar with the laws connecting mechanical action with heat, as Secchi, Langley, Faye, Young, Newcomb, and others, have overlooked the bearing of these laws on the evidence of greatly diminished heat afforded by the darkness of sun-spots, we may accept with confidence the conclusion that, *since sun-spots are regions of cooled vapours, where, therefore, work has been expended by those vapours (not upon them), such sun-spots must be regions of expansion, not of compression. And since vapours arriving from without would undergo compression,*[2]

that if the spots were produced by disturbance from within they would be regions of greater heat, this theory would lead to the belief that he holds the spots to be undoubtedly produced from without. 'As to sun-spots,' he says, 'there can no longer be any doubt, I think, that they are cavities in the upper surface of the photosphere, and that their darkness is due simply to the absorbing action of the gases and vapours which fill them.' This view appears to me demonstrably incorrect.

[1] Those who imagine that the arrival of cooler matter from without would explain the cooling, altogether underestimate the loss of heat which would have to be explained, even if we could imagine cool matter arriving quietly from without. But instead of this, it would acquire immense velocity in approaching the photosphere; and both through the rapid reduction of this velocity to rest, and by the immense pressures to which it would be exposed, it would be raised to a degree of heat exceeding that of even the brightest parts of the solar photosphere.

[2] Of course matter arriving from without would undergo expansion in consequence of the sun's own heat and of the heat resulting from the reduction of its own velocity to rest. But such processes, which, considered *per se* would indicate loss of heat, being in fact the results of heat and work expended, are altogether slight compared with the heat-generating causes of which they are the partial products.

whereas vapours ascending from below would expand, we can confidently conclude that the cooled vapours in the interior of newly formed spots were flung upwards from the inconceivably compressed regions below, and not drawn inwards from regions outside the photosphere.

(911.) Beyond this, however, we can no longer draw confident or definite conclusions. What takes place below the solar photosphere remains, and must probably for ever remain, a mystery. We cannot tell how the eruptive, or explosive, or repulsive forces manifestly at work within the sun are generated, how they act, why they alternate periodically in energy—as shown by the varying activity of the sun's surface—or from what depths below the photosphere they are exerted.

(912.) We can, however, see that, granted an explosive action, carrying a mass of highly compressed vapour from great depths upwards, until at length reaching the region of outer solar clouds the vapour breaks its way through that region, the phenomena of sun-spots should be presented in the sequence actually observed.

(913.) The outrushing vapour would necessarily expand rapidly as it approached the photospheric regions. Both its outrush and its expansion would be most efficiently cooling processes. Without pretending to estimate either the ordinary rate of outrush in such cases, or the relative rapidity of expansion—in other words, without undertaking to decide about details in the absence of sufficient evidence—we may assume the state of things during the beginning of a solar eruption to be such as is shown in section in fig. 257. Here an outrushing stream of vapours, which must be regarded as exceedingly complicated in structure, is shown expanding, first gradually, but as it nears the photosphere more rapidly, driving aslant the streams of luminous matter which had been vertical in the solar atmosphere before the disturbance began. It is evident that the result of this process will be the formation of a circular opening, the upper part of which, regarded as limited by the streams of luminous matter all round it, will have the saucer-shaped form recognised in the solar spots. This region, looked into vertically by the observer on Earth, would present a circular region of absorption, the outer part of which would be less dark than the central portion, because, though

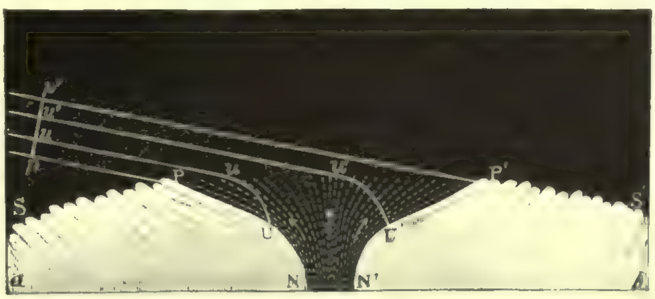


FIG. 257. Ideal vertical section of a Sun-spot in the earliest stage of its development.

the vapours are cooler above than below, and therefore most strongly absorptive, the luminous matter forming the surface of the saucer-shaped cavity is seen through a relatively small depth of this actively absorbing region, while the central portion of the spot, as thus observed vertically, is affected by the absorption of the whole column of ascending matter, perhaps by fifty or sixty, or even by a hundred, thousand miles' depth of outrushing and expanding, and therefore (for both reasons) relatively cool and strongly absorptive vapour.[1]

(914.) So long as matter continued to be flung upwards with sufficient energy and in sufficient amount, the opening in the photospheric regions would retain a well-rounded and regular form, because of the expansion of vaporous masses freshly arriving from below. But so soon as the supply began to diminish the spot would begin to lose its regularity of form. There would no longer be such effective expansion as would suffice to prevent the inrush of the photospheric masses which had been driven away from the region of disturbance. Yet the inrush would not take place equally on all sides. In particular, there would be one side towards which the main volume of such vaporous matter as still continued to ascend into the spot-region would be flung, viz., the eastern side (as we look at the sun), since the relatively slow rotational motion of the vapour flung from below would cause it to lag against the more rapidly rotating photospheric matter above. Yet we must not expect to find the eastern side of every spot in this latter stage to be the more complete and better rounded, since movements affecting the position of the whole spot-area—cyclonic or tornado movements for example—might readily carry the side which had been towards the east towards the west, and *vice versâ*.

(915.) The spot-region would thus assume that characteristic pear-shaped form which we notice nearly always in spots which have passed the first stage of their development. It is well shown in fig. 258, where we notice also that near the small end of the spot the photosphere shows by the appearance of small spots that it is being, as it were, drained by the rush of matter towards the spot-region on this side. The spots thus formed often acquire considerable dimensions while the original spot is being covered over. We can recognise also at this spot how the luminous matter outside the spot finds inlets at places where the resistance of the vapours already present

[1] The distinction between the umbra and the nucleus shows that below the region of the luminous streams whose upper expansion forms the granules there must be luminous matter, forming an inner region of greater brightness, and therefore hotter than the matter within the spot-cavity. In fig. 257 I indicate a way in which the width of the penumbra on the side farthest from the sun's edge (occasionally equal to the width of the side nearest to the edge) may be explained by the refractive action of the vapours within the spot-cavity. The lines Pp, Uuu, U$'u'u'$, and P$'p'$ are supposed to be lines of sight from the Earth when the spot is viewed aslant.

within the spot is least effective, while elsewhere resistance is maintained. There is only one place, and that narrow, where the photospheric matter finds entrance on the more rounded side of the spot. But already this region is being invaded from behind by the luminous matter which had earlier found its way in at a region of less resistance.[1]

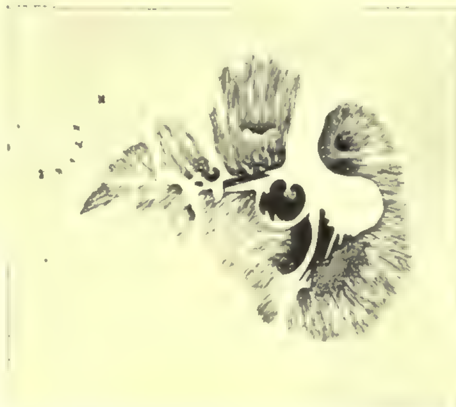


Fig. 258.—Pear-shaped aspect of a spot after the circular stage has passed (*Trouvelot*, April, 1870).

(916.) Later, the process of in-rush covers over the whole spot-region with photospheric matter. There is now no region of darkness, but yet there remain great facular streaks, indicating the irregular distribution of the luminous clouds. Gradually the faculæ become less and less conspicuous, and at last the region formerly occupied by the spot resumes the normal aspect, uniform in general tint save for the maculations, which are never wholly wanting from any part of the sun's surface.

(917.) We have then, *first*, every reason to expect that the principal solar disturbances would be produced from below; *secondly*, we have in the darkness of the spots, implying the relative coolness of the vaporous material occupying the spot-regions, evidence practically decisive that there has been such expansion, accompanied by cooling, as would take place with matter rising from great depths, and not such contraction and compression accompanied by intense heating as would follow the arrival of matter from without; and *thirdly*, we find all the phenomena of sun-spots, from their first formation to their disappearance, corresponding with what we should expect if they were formed by swiftly uprushing and actively expanding vapour-masses, and subsequently modified and finally obliterated by the inrush of surrounding photospheric matter necessarily following the passing away of those vapours from the disturbed region.

(918.) So much may be considered established; but in this we have no more than the beginning of the interpretation of the complicated series of solar phenomena already recognised, while every year of research, though making such general inferences more obvious, renders the actual problems we have to deal with, when details are considered, more perplexing and more difficult.

[1] Plates XIII. and XIV. are well worth studying for examples of the later stages of sunspot development. It is especially instructive to note how the luminous streaks beneath the bright clouds are shown (in Plate XIV.) where the photospheric matter is being broken up to cover over the spot opening.

CHAPTER VII.

THE SUN'S SURROUNDINGS.

(919.) The recognition of the fact that sun-spots are phenomena of eruption lends special interest to the study of the region of the sun outside the photosphere.

Where we look *through* this outer region at the photosphere, we can of course learn little about the nature and condition of the sun's surroundings. We can, however, study these surroundings outside the visible limits of the solar disc ; and although this view, in which all we look at is greatly foreshortened, is not the most favourable for satisfactory observation, yet it may serve to give us the evidence we require as to the solar outbursts, whose range we cannot suppose to be limited to the level of the photosphere.

(920.) During the total solar eclipse of May 2, 1733, Vassenius, at Gottenburg, observed several red clouds floating, as he supposed, in the moon's atmosphere. 'These spots seemed,' he writes, 'composed in each instance of three smaller parts or cloudy patches of unequal length, having a certain degree of obliquity to the moon's periphery. Having directed the attention of my companion, who had the eyes of a lynx, to the phenomenon, I drew a sketch of its aspect. But while he, not being accustomed to the use of the telescope, was unable to find the moon, I again, with great delight, perceived the same spot, or, if you choose, rather the invariable cloud occupying its former situation in the atmosphere near the moon's periphery.' There can be no doubt that the appearances seen by Vassenius were really the phenomena now known as the coloured prominences, a name which must be understood as relating to their appearance, not to their real nature. It remains in use, for convenience only, now that their real nature is known.

(921.) Ferrer, in 1806, and Van Swinden, in 1820, noticed faint traces of some peculiar coloured appendages round the dark body of the moon in total eclipses ; but their observations were not satisfactory, nor was any attention drawn to the subject.

(922.) During the great eclipse of 1842, a number of observers were distributed along the line of total obscuration. Airy, Arago, and the younger Struve,

Littrow, Baily, Santini, Valz, and Biela—a host, in fine, of the best trained observers in Europe—watched the eclipse. All of them recognised the presence of rose-coloured prominences round the disc of the eclipsed sun. Baily and Mauvais compared the prominences to Alpine peaks coloured by a setting sun. The latter noticed that two protuberances on the western side of the sun grew higher as the eclipse proceeded, and that meantime a third made its appearance. It was after these observations that these appendages of the sun came to be called *the coloured prominences*.

(923.) Biela, Schumacher, and others recognised a border of rose-coloured light surrounding a part of the moon's edge at a lower level than that attained by the prominences. This phenomenon had been noticed earlier than the prominences themselves; for, during the total eclipse of 1706, Captain Stannyan remarked that a blood-coloured streak of light appeared before the sun's limb emerged from behind the moon. In 1715, also, Halley noticed that two or three seconds before the emersion the moon's limb appeared to be tinged with a dusky but strong red light, forming a long and narrow streak; and during the same eclipse, Louville saw what he describes as an arc of deep red colour along the edge of the moon's disc. The latter astronomer was careful to assure himself that the appearance was no illusion, and to this end he brought the red arc into the middle of the telescopic field of view, when he found that the red colour remained unchanged. Don Ulloa, in 1778, and Ferrer, in 1806, had noticed a similar phenomenon.

(924.) This border of rose-tinted light received at this time the name of the *Sierra*, a title which corresponds much better with the serrated appearance of this solar appendage than the affected and incorrect name *chromosphere*,[1] which has the further and more serious disadvantage of involving a theory, and a theory unquestionably incorrect. The surface of the Sierra is certainly not spherical.

(925.) When the various accounts of the eclipse of 1842 came before the astronomical world, several theories were propounded in explanation of the red prominences. The theory that they are mountains in the sun was for a while in favour: but Arago pointed out that some of them were too considerably inclined to the perpendicular to be so regarded. Others supposed them to be clouds in the solar atmosphere; while others again suspected them to be enormous flames. As ordinarily happens in such cases, there were not wanting those who denied that the coloured prominences had any real existence whatever. M. Faye asserted his belief, for example, that they are purely optical illusions—'mirages, perhaps, produced near the moon's surface.'

(926.) The eclipse of 1851 removed these doubts for the most part, though it is to be noted in passing that, despite the evidence obtained then, and yet again in 1860, there were some who continued, even until the great Indian eclipse of 1868, to deny that the coloured prominences and the rose-tinted arcs seen at a lower level could really be regarded as solar appendages.

(927.) Many skilful observers made drawings of the prominences seen during the total eclipse of 1851. These pictures exhibited a sufficiently satisfactory agreement to convince the observers that they had all witnessed the same phenomena; though the discrepancies between them afford instructive evidence of the difficulty of delineating

[1] As bad as *phosphere* and *chromic* would be for *photosphere* and *chromatic*. The word should of course have been *chromatosphere*, if a title of the pedantic and affected sort were to be used at all.

with exactness the details presented during eclipses. The following six pictures represent in order the work of the Astronomer Royal, Mr. Dawes, Mr. Hind, Mr. Lassell, Mr. Gray, and Mr. Stephenson.

Fig. 259. Fig. 260. Fig. 261.

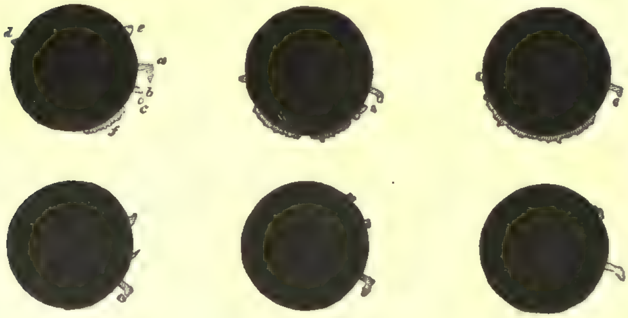


Fig. 262. Fig. 263. Fig. 264.

The Coloured Prominences seen during the Eclipse of 1851.

(928.) It was now proved beyond all possibility of reasonable question that the great globe of the sun is surrounded by a deep layer of coloured matter, while from portions of this vast envelope enormous protuberances start out, their height so vast that ten globes such as our Earth might be piled one upon the other on the sun's surface without attaining to the summit of the highest prominence. But this great fact was not to take its place in our treatises of astronomy until, although twice proved already, it had been proved once again at least.[1]

(929.) The principal interest of the eclipse of 1860 resides in the photographic work of Fr. Secchi and Mr. De la Rue. There was considerable reason to fear that success would be difficult on account of the colour of the prominences. A red or orange light has commonly no actinic power whatever, insomuch that the 'dark room' of the photographer is, in a photographic sense, nearly as dark when its walls are of orange-coloured glass as though they were absolutely opaque. So that if the light of the prominences really were pure red, it was hopeless to endeavour to obtain photographs of these objects.[2] The two photographers adopted different methods. Mr. De la Rue employed the Kew heliograph, and the small image formed at the

[1] Scientific progress will never be so rapid as it might readily become so long as each new result must be established over and over again before it is admitted by the main body of the scientific world. The progress of science has been at least as seriously checked by undue caution as by undue boldness. It would seem almost as though some students of science were continually in dread lest the work of our observers should become too productive. The value of scientific observation seems to be enhanced in their eyes precisely in proportion as its fruits are insignificant. The cry is always with those thus unready to work up the evidence already obtained, 'We must wait for fresh researches.' And as this has happened with facts now accepted, so it is happening still, and so it will happen hereafter with facts which have been in truth demonstrated, but the demonstration of which does not lie on the surface.

[2] It is worthy of notice, and affords a fresh proof that observations may involve important results altogether apart from their direct significance, that the successful photographing of the prominences afforded all but complete proof of that which was afterwards demonstrated by the spectroscope—the fact, namely, that the prominences consist of glowing vapour. Secchi or De la Rue might quite confidently have asserted that when the prominences came to be examined with the spectroscope the spectra would show a band or bands near the blue end of the spectrum, separated by wide dark gaps from certain bands in the red and orange part. The greater part of the light of the prominences corresponded to these latter bands, as is shown by their colour, but that part by which the photographs were taken corresponded to the former.

focus of the object-glass was enlarged before being received upon the plate. Secchi received on the plate the image formed by the object-glass of his telescope. This image was about an inch in diameter. 'The result,' says Secchi, 'proved that both systems are excellent, each having its special advantages. In the enlarged image one can distinguish more details, but the direct image gives a greater extension to the corona.'

Fig. 265.

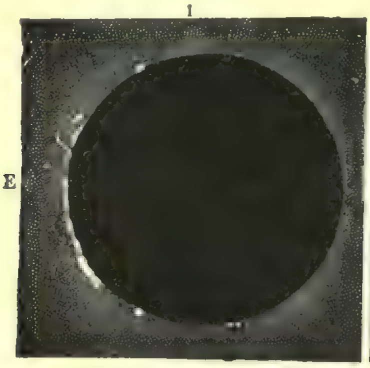
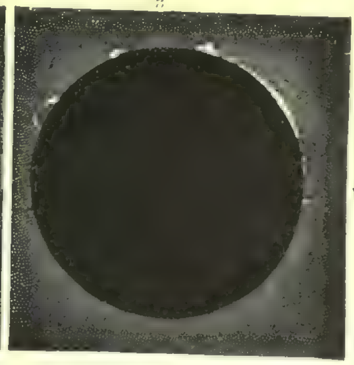


Fig. 266.



From Photographs of the Sun during the total Solar Eclipse of June 1860 (*De la Rue.*)

(930.) The two observers were situated at different stations—Fr. Secchi at the Desierto de las Palmas, near the Mediterranean; Mr. De la Rue at Riva Bellosa, near the Atlantic. Thus an interval of about six minutes elapsed before the moon's shadow passed from one station to the other, and an opportunity was afforded of determining whether the prominences change rapidly in figure. Besides this, there was a slight difference in the apparent course traversed by the moon's disc in crossing the sun's: for Secchi and De la Rue were at different distances—and as a matter of fact on opposite sides of the path of the centre of the moon's shadow, so that De la Rue's series of photographs shows more of the prominences on the superior part of the sun's limb, while Secchi's series shows more of the prominences on the inferior portion.

(931.) It will be unnecessary—so closely do the two series resemble each other in all essential respects—to exhibit both; but farther on there will be found a copy of one of Secchi's pictures (fig. 286, Art. 984), which may be compared with figs. 265 and 266, copied from Mr. De la Rue's photographs.[1]

(932.) Fr. Secchi thus summed up the result of his observations :—

1. The prominences are not mere optical illusions: they are real phenomena appertaining to the sun. Our observations having been made at two places separated

[1] It will be noticed that fig. 265 represents the earliest phase. The moon is advancing from right to left, and has just hidden the last fine thread of direct sunlight on the left. Thus we see the full height of the prominences on the left, while no prominences are seen at all on the right of the sun. At the upper and lower part of the sun's limbs the prominences are partly concealed, and necessarily remain so throughout the eclipse. In fig. 266 the moon has obliterated a large proportion of the prominences on the left, while it has in turn revealed a number of small prominences, a long range, or sierra, and a lofty and massive projection on the upper right-hand quadrant. Fr. Secchi observed the prominences directly, and with great care, while his assistants managed the photograph work. Amongst the phenomena he noticed may be mentioned the circumstance that the strange prominence seen in both figures on the upper left-hand quadrant possessed a helicoidal structure. In the magnified picture from Mr. De la Rue's photographs this structure can be clearly recognised.

a hundred leagues from each other, it is impossible to suppose that shapes so well defined and so exactly identical could be produced by a phenomenon resembling mirage.

2. The prominences are collections of luminous matter of great brilliancy and possessing a remarkable photographic activity. This activity is so great that many of the prominences which are visible in the negatives could not be seen directly, even with powerful instruments, perhaps because they emitted only chemical rays and few or no luminous rays.

3. There are masses of prominence matter suspended and isolated like clouds in the air. If their form is variable, the variations take place so slowly that it is impossible to recognise their effect during an interval of ten minutes. (This opinion had subsequently to be modified.)

4. Besides the prominences, a zone exists of the same material, enveloping the whole of the sun's globe.[1] The prominences spring from this envelope; they are masses which raise themselves above the general level, and even at times detach themselves from it. Some among them resemble smoke from chimneys or from the craters of volcanoes, which, when arrived at a certain elevation, yields to a current of air, and extends horizontally.

(933.) The prominences were finally placed in their true place in the solar scheme by the observations made in 1860. Doubts still continued to be expressed by a few; but all who could understand the evidence now regarded the coloured prominences as solar appendages.[2]

[1] Grant, Swan, and Von Littrow had already recognised this; and Leverrier, from observations made during the same eclipse, had come to the same conclusion.

[2] It must be remembered that, though the

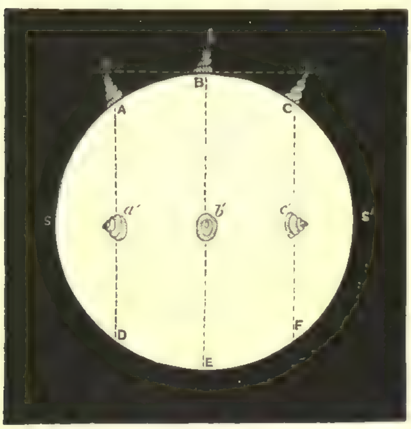


Fig. 267.—Illustrating the distribution of large Prominences over the Sun's surface.

prominences are seen all round the circumference of the solar disc, they do not really form a circle. They are the foreshortened projections of objects which may lie—and many of which *must* lie—thousands of miles from that circle of the sun which at the moment forms the apparent boundary of his disc. We know, in fact, that many prominences are as high as 3′—that is, extend to some 80,000 miles from the sun's surface, while a few have attained a much greater height even than this. Supposing A B C to represent a part of the sun's circumference, and *a b c* three prominences each 3′ in height, an observer view-

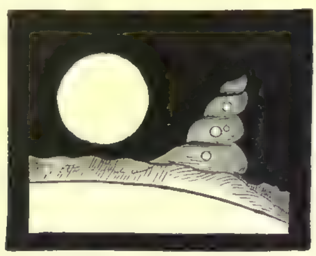


Fig. 268.—Illustrating the vast scale of the larger Prominences.

ing the sun from a point at a great distance away towards the right or left would only see the extreme tips of the prominences *a* and *c*, while he would see the full height of the prominence *b*. But in order that these two prominences should be thus in appearance sunk below the solar limb the line *a c* would need to be about 510,000 miles in length. So that if there were any prominence of so great a height as *a*, *b*, or *c* along any part of

(934.) Fig. 269 presents a portion of the disc of the sun as photographed at Aden during the eclipse of August 1868. On that occasion the nature of the prominences was for the first time determined by means of the spectroscope. A method was soon after perfected and successfully applied for observing the prominences

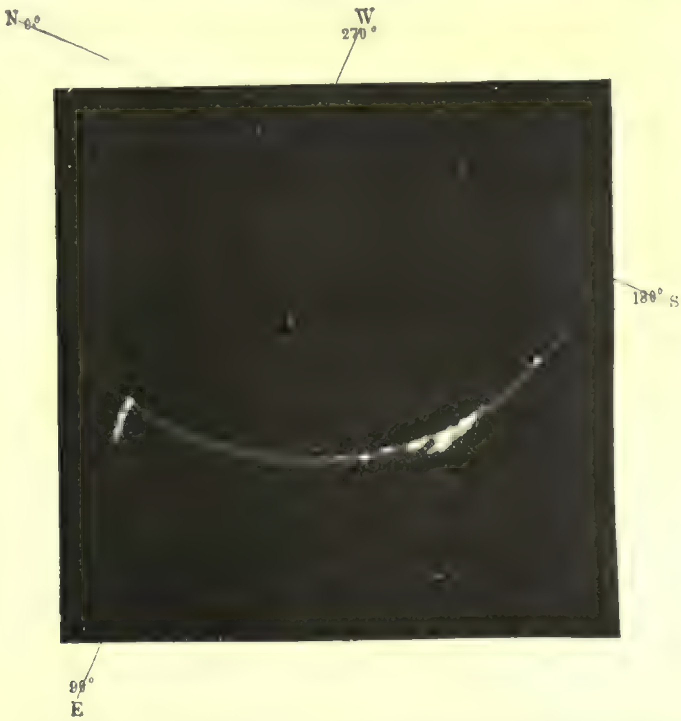


FIG. 269.—The Eclipsed Sun, August 1868. Photographed at Aden.

without waiting till the sun is totally eclipsed. Nearly all that has been discovered since August 1868 respecting the prominences has been learned in this way. But two pictures of the prominences as seen during the eclipse of August 1869 are worth studying.

(935.) First, we have in fig. 270 a view of the prominences as photographed by the American astronomers, the moon's disc being reduced so as to permit all the prominences to be visible at once.[1] Two photographs by Mr. Curtis were specially valuable on account of their delicacy and the number of details shown in them.

the arc A B C, it would appear to rise above the sun's level. So also three such prominences as *a*, *b*, and *c*, situated anywhere on the zone A B C F E D—as at *a′*, *b′*, and *c′*—would be visible from a station far to the right or left of the globe S S′; a prominence anywhere on the great circle of which B′*b*′E is one half would show its full height; but a prominence 3′ high, on the small circles of which A D and C F are foreshortened halves, would be just visible at its apex. Now the zone, of which A C F D is one half, has a surface bearing to the whole surface of the sun the same proportion that A C bears to S S′, or about 10 to 17.

If fig. 268 be supposed to represent one of the coloured flames (exceptionally spiral in structure) four minutes in height, then the four interior (or so-called terrestrial) planets would be represented on the same scale by the four small discs shown on the convolutions—Mercury uppermost, then Venus, the Earth and Moon, and Mars; while even the giant bulk of Jupiter would bear no greater proportion to the enormous solar prominence than is shown by the dimensions of the largest disc in fig. 268.

[1] The line F E indicates the course of the moon's centre across the sun, A B being a declination-circle and C D a declination-parallel.

Mr. Curtis showed that the sharply-defined outlines of prominences, seen in most photographs of eclipses, result from excessive development. The encroachment of the prominence-bases on the black disc of the moon is due to a similar cause. Mr. Curtis's report of the eclipse included an ingenious inquiry into the phenomena presented by the strangely-shaped prominence shown between E and C in fig. 270. He referred all its peculiarities of appearance to the action of a cyclonic storm in the upper regions of the solar atmosphere at this place.

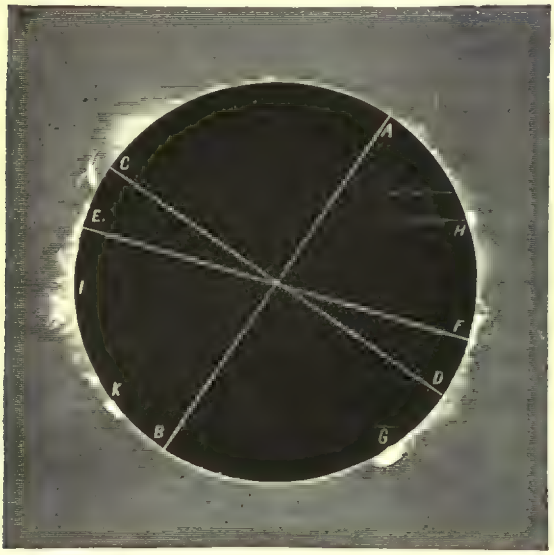


Fig. 270.—The Eclipse of August 7, 1869. From photographs by the American astronomers.

(936.) Secondly, fig. 271, an enlarged view of the prominences between H and D of fig. 270, (by Mr. G. H. Knight, who studied the prominences with a telescope magnifying 120 times) is worthy of careful study. It is copied from part of a fine drawing sent to me by Mr. Knight,[1] probably the most detailed drawing of the prominences yet made during total eclipse.

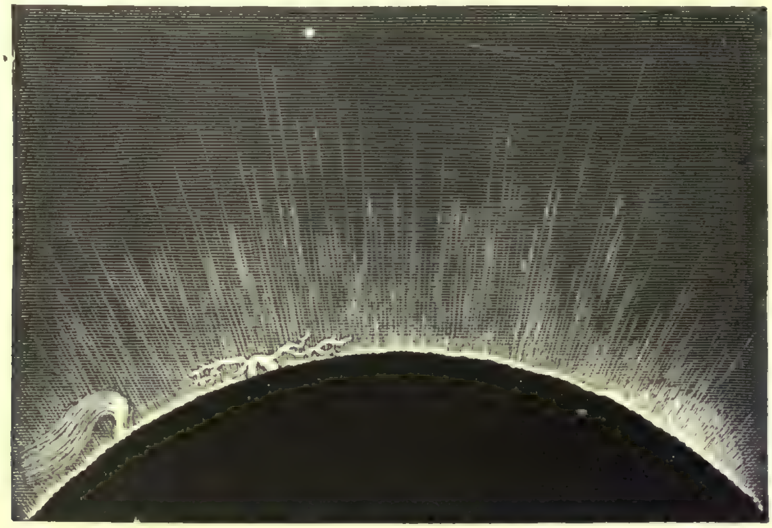


Fig. 271.—Enlarged view of the Prominences seen during the Eclipse of August 1869, showing also Mercury close by the Sun (*Knight*).

[1] Respecting this drawing Mr. Knight makes the following remarks :—'I believe my rude attempt more true to life than any photographs I have seen. One marked peculiarity of such appearances—the delicate blended haze of light—which to some observers extends much farther into space than to others, is ignored by the photograph, which is an inveterate Rembrandt in its rejection of middle tints. Then, again, the protuberances and the quivering fringe of fire, of which they are the more salient portions merely, are of various tints of gold, copper, and pink, confessedly difficult colours for actinic action, but which the wonderful camera of the human eye takes in perfectly. Besides which, I had the advantage of a more powerful instrument than any other observer I have heard of.'

(937.) As early as 1866 the observations made by Mr. Huggins on the star T Coronæ (during the great outburst of 1866), in whose spectrum he saw the lines of hydrogen bright on the rainbow-tinted background, led that able observer to believe that spectroscopic evidence might be obtained respecting the coloured flames when the sun was shining in full splendour. He was not able to effect this, however, nor could Secchi and others who examined with the spectroscope both the sun's disc and the region around its edge where prominences are seen during total solar eclipses. Mr. Huggins's failure may be attributed chiefly to the unfitness of the spectroscope he employed for this particular work, since it had been specially constructed for stellar researches. But partly also we may attribute the failure of those who sought with the spectroscope for the coloured prominences to their uncertainty as to the nature of these solar appendages, since it is far easier (in all classes of observation) to recognise what we know certainly to exist than to determine the existence of that which is as yet unknown. It was not until the real nature of the prominences had been demonstrated by spectroscopic observations made under the easier conditions of total eclipse that the method devised by Mr. Huggins [1] was successfully applied.

(938.) In the eclipse of August 18, 1868, remarkable for the duration of totality, the first attempt was made to apply spectroscopic analysis to eclipse phenomena, the corona being the chief object of interest at that time. As it turned out, however, the prominences gave the most satisfactory information.

Herschel (then Lieutenant), Rayet, Janssen, Pogson, and Tennant, all gave the same general account of the spectrum of the prominences, viz., that it consisted of bright lines only, a result indicating (Art. 882) that the prominences are masses of glowing gas. As to details the observers differed. Herschel noted three lines—a red one, which he regarded as possibly C, the red line of hydrogen; an orange one, which he regarded as almost certainly D, the orange (double line) of sodium; and a blue one,

[1] As the question of priority in regard to the spectroscopic method of observing the prominences has been raised, it may be well to quote the following lines from the Report of the Council of the Astronomical Society for the year 1867, *Monthly Notices* for February 1868:—'*During the last two years* Mr. Huggins has made numerous observations for the purpose of obtaining a view, if possible, of the red prominences seen during a solar eclipse. The invisibility of these objects at ordinary times is supposed to arise from the illumination of our atmosphere. If these bodies are gaseous, their spectra would consist of bright lines. With a powerful spectroscope the light reflected from our atmosphere near the sun's edge would be greatly reduced in intensity by the dispersion of the prisms, while the bright lines of the prominences, if such be present, would remain but little diminished in brilliancy.' This appeared half a year before the nature of the prominences had been ascertained by spectroscopic research. It is no mere vague suggestion that the prominences might be detected somehow by means of the spectroscope, but indicates the precise principle on which their visibility with the spectroscope depends, besides anticipating the discovery made six months later by Herschel (then Lieutenant), Rayet, and Janssen that the coloured flames are gaseous. From the mention of two years we may infer that Mr. Huggins had thought out the method immediately after his observations of T Coronæ, when first the spectroscope indicated the existence of gaseous matter shining with specific spectral tints in the midst of or around a mass of incandescent matter—that is, of matter glowing with all the colours of the rainbow, and so shining as a whole with white light.

which he thought might possibly be F, the blue-green line of hydrogen. Major Tennant saw five lines which he associated with the lines C, D, *b*, F (probably), and G. M. Janssen saw six—red, yellow, green (two), blue, and violet, and established the coincidence of the red and blue lines with the lines C and F. M. Rayet saw no fewer than nine lines, five of which he recognised as brighter than the rest; these he associated with the lines B, D, E, *b*, and F. M. Janssen, who presumably was acquainted with the principle enunciated by Mr. Huggins six or seven months before, was so struck by the brightness of the lines in the spectrum of the prominences that even while totality was in progress he exclaimed, 'Je reverrai ces lignes-là!' and on the morrow (for clouds concealed the sun soon after totality) he so successfully carried out the idea that he could describe the day as for him 'one of continuous eclipse.'

(939.) But the news that the prominence spectrum consists of bright lines, including the lines of hydrogen, was telegraphed to Europe, and spectroscopists there were prepared to apply Mr. Huggins's method successfully so soon as they knew what to look for. Among them Mr. Joseph N. Lockyer, one of the readiest and most promising of the younger spectroscopical observers of that time, was the first to achieve success. He had recently obtained, through the Royal Society, a spectroscope of considerable power, admirably suited for the method of observation required, the work of Mr. Browning, whose skill had been for several years applied to the construction of spectroscopic apparatus. With this instrument Mr. Lockyer succeeded in seeing the prominence-spectrum about two months after the Indian eclipse. But the letter announcing his success reached the French Academy simultaneously with M. Janssen's letter from Guntoor. Others, as Secchi, Stone, and Huggins himself, armed with less suitable spectroscopes, did not achieve the same practical success till later.

(940.) We must here briefly consider the principle enunciated clearly, but somewhat succinctly, by Mr. Huggins more than half a year earlier (see last note).

Suppose that P P′ (fig. 272) is a prominence, S S′ the edge of the sun, and *s s′* the space included by the slit. Then *p′s′* (Arts. 893 and 894) produces a solar spectrum (which, however, commonly presents certain peculiarities when belonging to the edge of the sun's disc); the part *p p′* includes a portion of the prominence, and gives a prominence-spectrum which we may suppose to be represented by the bright lines at C and F and near D. But it will also give a solar spectrum, for the light of our own illuminated air comes from the space included within the slit *s s′*; and as our air is illuminated by solar light, it produces a solar spectrum. Also the part *s p* will give a solar spectrum due to the illuminated air. Now, so far as ordinary telescopic observation is concerned, the prominence P P′ is obliterated from view by the illuminated air, which extends around and over the place of the sun. But when the strip *s p′* is observed through the spectroscope the light of the illuminated air within the small space *s p′* forms a rainbow-tinted spectrum occupying such a space as is shaded with cross lines in fig. 272, and is reduced in intrinsic brightness in corresponding proportion; while the light of the prominence-matter within *p p′* is spread only over the three lines shown in the figure (and a few fainter ones), and is

Fig. 272.

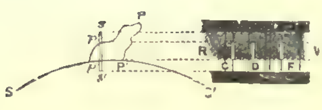
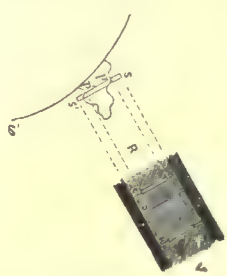


Fig. 273.



The Spectroscopic Analysis of a Solar Prominence.

therefore proportionately but little reduced. Hence, if we only have enough dispersive power, we can make sure of rendering the prominence-lines visible, for we get appreciably the same luminosity for them whatever the length of the spectrum, the only effect of an increase of length being to throw the bright lines farther apart; whereas the atmospheric spectrum which forms the background will obviously be so much fainter as we spread its light over a longer range. By this plan we get a certain number of images of a portion of a prominence—a mere strip so to speak; and we can get any number of such portions, and in any direction as compared with the sun's limb. For example, if S S′ (fig. 273) be the sun's limb, $p\,p'$ a prominence, we can get from such a strip as $s\,s'$ the spectrum R V. And obviously, since the length of the bright lines tells us the length of the part $p\,p'$ in figs. 272 and 273, we can, by combining a number of such parallel strips as S S′, learn what is the true shape of the prominence $p\,p'$.

(941.) The new method of observing the prominence-spectrum enabled the observer to determine at his leisure the true position of the prominence-lines, their characteristics as respects shape (the significance of which feature has been already referred to, Art. 961), and also the existence of lines which had escaped observation while the eclipse was in progress.

(942.) The spectrum of the sun's limb seen at the same time (or that of the illuminated terrestrial atmosphere) affords the most satisfactory means of determining the position of the bright prominence-lines. It is only necessary to see which (if any) among the dark lines in the solar spectrum coincide with the bright ones, the prominence-spectrum, to learn what elements, in the form of glowing vapour, are present in the coloured prominences.

(943.) Janssen found in this way that the orange line of the prominences does not correspond, as had been thought, with the D or sodium (double) line of the solar spectrum. He found, however, that the red and blue prominence-lines coincide with the C and F lines of hydrogen. So that these enormous objects, extending in some instances to a height of more than 80,000 miles above the sun's surface, consist in part, at least, of the glowing vapour of hydrogen.

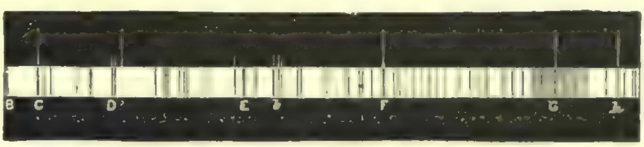


FIG. 274.—The Spectrum of the Prominences (upper) compared with the Solar Spectrum (lower).[1]

(944.) But it was possible to recognise other lines besides these three (see fig. 274) by the new method, when an instrument of adequate dispersive power was employed.

[1] Father Secchi gives the following description of the principal lines seen in the prominence-spectrum under ordinary conditions:—

'The line C of hydrogen is the most easily seen of all. It sometimes reaches the enormous height of three minutes, indicating the presence of such colossal prominences as are seen during eclipses. This line also extends in a well-marked manner within the limb, overlapping the disc by ten seconds and more. Further on the disc there is a region where the line cannot be seen, being neither bright nor dark, but of the same tint

The additional lines are usually, however, shorter than the lines of hydrogen, and that line near D (called D_3, the double D-line of sodium being called D_1D_2), which indicates the presence of some gaseous element as yet undetermined. Thus the lines of sodium D, and magnesium (*b*), are seen in the upper spectrum of fig. 274, but they are shorter than the other lines.

(945.) We have here evidence of the existence of that lower region of coloured matter called by Airy in 1842 the *Sierra*, and photographed by Secchi in 1860. This solar envelope was described by Leverrier in 1860 as a bed of rose-coloured partially-transparent matter, covering the whole surface of the sun, whose existence was 'established by the observations made during the total eclipse of that year.'

(946.) The Sierra as it appears in the telescope during total eclipses (and also as spectroscopic observation discloses its configuration) is well shown in fig. 271 (Art. 936). It is obviously not a stratum or layer of gaseous matter, but consists of multitudinous small tongues and streaks of glowing gas, somewhat like small flames. We have no evidence that it extends downwards as far as the visible limits of the photosphere. If a distance of half, or even a larger proportion, of its own average depth separated the lower limit of the Sierra from the photosphere, we could detect no trace of this space, since a line of sight directed to the edge of the sun's disc would pass through the gaseous substance of the Sierra both on the nearer and farther side of the sun's globe.

(947.) The Sierra cannot be regarded as the true solar atmosphere—

exactly as the neighbouring part of the spectrum, which is thus in this part of its length continuous and uniformly bright. Outside the disc the line is much brighter near its base than at the summit, and the line is dilated at the base, and seems to terminate in a point where its light fades off, until it becomes of the same brightness as the neighbouring part of the spectrum [of the sun's disc near the edge] before becoming a dark line.

'The line in the yellow, near the sodium lines, is about twice and a half as far from the nearest of these as these are from each other. This line is sensibly the prolongation of a bright line in the solar spectrum. In height and brightness it corresponds closely to the C-line, but I have noticed that it will not bear high magnifying power so well. While the lines F and C remain brilliant under such powers, this line becomes weaker, so that only the practised eye can detect it. The line ends in a point, and it often extends itself brilliantly upon the disc of the sun.

'The F-line is in general not so high as the C-line, and grows faint at the extremity, where it takes the form of a lance. Sometimes I have seen it prolonged beyond the edge of the solar disc, as in the case of the C-line; at other times a very fine black thread shows itself on the more refrangible side.

'The third line of hydrogen near G I have seen as a bright line, but it is necessary to reduce the dispersive power of the prismatic battery in order to obtain sufficient light.'

Secchi was surprised to find on one occasion, when several lines besides the above-mentioned were visible, that of the three lines forming the group *b* of magnesium one only was visible along with a second line holding a position midway between the other two lines of the same group. He remarks that Rayet had observed during the eclipse of August 1868 only two lines of this group, and that these two doubtless corresponded with those seen by himself, of which, as we have seen, one does not accord with either of the two remaining magnesium lines. He was so surprised at this peculiarity that he searched diligently for two hours to detect traces of the other lines of magnesium, but could find none whatever. We seem to have evidence here in favour of Dr. Sterry Hunt's view (Art. 899, Note) that the so-called elements are not really elemental.

though it may be described (being gaseous) as forming a part of the solar atmospheric envelope. Even this is doubtful, however, if the word 'atmosphere' is to be understood in its exact sense; since it is by no means certain (nay, on the contrary, rather unlikely) that the gaseous matter forming the Sierra is continuous with the more complex gaseous regions below it, or that even these have the continuity of texture (so to speak) which characterises a true atmospheric envelope.

(948.) The spectrum of a prominence is nearly always simpler than that of the Sierra below it, though occasionally, where there is a great disturbance below a prominence, the spectrum of the prominence indicates the presence of metallic vapours usually limited to the Sierra. It is probable that the spectrum of the Sierra grows more complex with approach to the sun's surface. It is certain that the brighter and more easily observed lines in this spectrum increase in breadth as the sun's surface is neared. Fig. 275 illustrates the evidence on this point. Here 1 is a part of the spectrum of the sun's limb, while 2 represents the line F in the spectrum of the Sierra.[1]

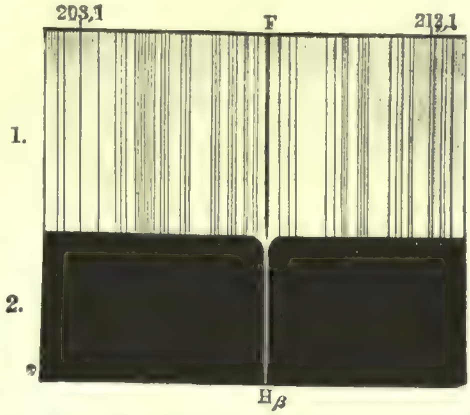


Fig. 275.—Illustrating the widening of the F-line of Hydrogen near the base of the Sierra.

(949.) From the observed width of the F-line near the sun's limb (see fig. 275) it has been estimated by Wüllner that the pressure at the base of the Sierra, or at the surface of the photosphere, is less than the pressure of the Earth's atmosphere.[2]

[1] The widening of this line close by the sun's limb may be regarded as unquestionably indicating an increase of pressure, because the researches of Plücker, Hittorf, Huggins, and Frankland have demonstrated that the F-line of hydrogen does actually increase in this way in width when the pressure at which the hydrogen subsists is increased. Temperature, also, has an effect on the hydrogen lines; and it is not easy to separate the effects due to pressure from those due to temperature. On the whole, it seems probable that pressure is chiefly in question, while it may be regarded as absolutely certain that temperature alone is insufficient to account for the observed change.

[2] In the first edition of my *Sun* (1870) I made the following remarks on the widening of the F-line:—

'It must not be forgotten that the width of this line where it actually reaches the spectrum of the limb is not known. The observed width on which Wüllner founded his researches may be that corresponding to a height of 50, 100, or even 200 miles above the photosphere; and within these 50, or 100, or 200 miles an increase of pressure may take place by which the actual density of the atmosphere close by the photosphere may be enormously increased. There may be an atmosphere including the vapours of iron, sodium, magnesium, &c. (of all the elements, in fine, whose dark lines appear in the solar spectrum), extending, say, 100 miles above the photosphere; and yet no instruments we possess could suffice to reveal any trace of its existence, *unless the dark lines in the solar spectrum be thought to demonstrate the fact that such an atmosphere actually does exist.*'

It had been suggested that the absorption indicated by these dark lines may be due to vapours below the photospheric level.

What I thus here suggested as probable was established by an observation made by Prof. Young, of America, during the eclipse of December, 1870. The slit of the spectroscope, being so placed as to include the part of the sun's edge

He has even assigned the limits of pressure at the level of the solar photosphere as lying certainly between 50 and 500 millimetres (or between 2 inches and 20 of a mercurial barometer at the Earth's surface).

FIG. 276.—Illustrating the Spectroscopic indications of Solar Cyclonic Action.

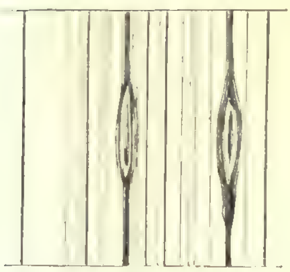


FIG. 277. Double-reversal of the Sodium lines D_1 and D_2. (Observed by Professor Young.)

(950.) Often the lines in the Sierra spectrum are distorted close by the sun's edge, as shown, for instance, in the case of the F-line of hydrogen in fig. 276.[1] Such changes indicate (Art. 892) that very rapid motions are taking place, either due to the swift rush of the glowing hydrogen through the solar atmosphere or to the effects of cyclonic motions in the atmosphere itself, by which the glowing hydrogen is borne along.

(951.) The lines in the Sierra spectrum, especially those of magnesium and sodium, are often found reversed in the spectrum of the adjacent part of the sun's edge, the

which was last covered by the moon, included, necessarily, the lowest layers of the solar atmosphere. So long as the sun's direct light illuminated the spectroscopic field, only a few bright lines belonging to the Sierra could be seen. But so soon as the last fine sickle of the sun's disc was concealed, hundreds of bright lines made their appearance and continued visible for a few seconds—in fact, until the moon had covered the region thus shown to form a true atmosphere, exceedingly complex, and formed in large part of the glowing vapours of our familiar gases.

In later eclipses this observation (called in question at the time by several spectroscopists) was successfully repeated.

If we consider the true significance of the widening of a line of hydrogen shown in fig. 275, we perceive that the light of that portion of the luminous spectrum of the photosphere which corresponds to the widened F-line is in part due to glowing hydrogen at high pressure—the absorption-line F being due to hydrogen at higher levels and consequently at lower pressure (and also, of course, cooler). Now if we extend this to the other elements which produce the dark lines, we shall see that a large part (we cannot tell how large a part) of the background of the solar spectrum must be due to the light from those very vapours in a more compressed state, which at higher levels, where they are cooler and more tenuous, produce the dark lines by their absorption. The luminous background of the solar spectrum is, in all probability, *wholly* made up of the broad-band spectra of the elements composing the sun's mass, including the elements which range above the photosphere, and by their absorptive action cause dark lines to appear athwart that luminous, rainbow-tinted background.

[1] At 1 we see the line deflected towards the violet, showing that the portion of the Sierra under examination was moving rapidly towards the observer; at 2 we see a deflection both towards the red and towards the violet, indicating that in the same field of view (that is, in the portion of the Sierra included within the slit) there were masses moving towards as well as from the eye; while, lastly, at 3 we see a deflection towards the red, indicating a rapid motion from the eye. During observations such as these, evidence has been obtained of motions ranging to the almost inconceivable velocity of 320 miles per second.

dark line widening, and along its middle a fine bright line appearing. Sometimes the phenomenon of double-reversal is seen, the bright line in turn widening, and along *its* middle a fine dark line being seen. Fig. 277 presents an example of the double-reversal of the double D-line of sodium as observed by Professor Young in October 1880.[1]

(952.) The ordinary spectrum of the Sierra contains, according to Ångström, the following lines:—

	Wave-length	Letter	Element
1	7055 *circ.*		Unknown
2	6561·8	C (H_α)	Hydrogen
3	5874·9	D_3	Unknown [Helium]
4	5315·9	[1474 Kirchhoff]	Unknown [Cosmium]
5	4860·6	F (H_β)	Hydrogen
6	4471·2	*f*	Cerium ?
7	4340·1	(H_γ, near G)	Hydrogen
8	4101·2	*h* (H_δ)	Hydrogen
9	3969 ?	—	Unknown
10	3967·9	H	Hydrogen, probably
11	3932·8	K or H_2	Hydrogen, probably

(953.) But many more lines make their appearance in the Sierra-spectrum under conditions of disturbance. No exact account of these can be given, inasmuch as, like the lines strengthened or reversed in the spot-spectrum (Art. 900), they vary in number and relative intensity—and even in both respects, when the lines of one and the same element are considered. Professor Young gives the following selection (professedly arbitrary, since 'nearly as many more are seen pretty frequently') of lines which make their appearance in the Sierra-spectrum 'on very slight provocation.'[2]

1′.	6676·9.	Iron.
2′.	6429·9.	?
3′.	6140·6.	Barium.
4′.	5895·0, D_1.	Sodium.
5′.	5889·0, D_2.	,,
6′.	5361·9.	Iron.
7′.	5283·4.	?
8′.	5275·0.	?
9′.	5233·6.	Manganese.
10′.	5197·0.	?
11′.	5183·0, b_1.	Magnesium.
12′.	5172·0, b_2.	,,
13′.	5168·3, b_3.	Iron and nickel.
14′.	5166·7, b_4.	Magnesium.
15′.	5017·6.	Iron and nickel.
16′.	5015·0.	?
17′.	4933·4.	Barium.
18′.	4923·1.	Iron.
19′.	4921·3.	?
20′.	4918·2.	Iron.
21′.	4899·3.	Barium.
22′.	4500·3.	Titanium.
23′.	4490·9.	Manganese.
24′.	4489·4.	Manganese and iron.
25′.	4468·5.	Titanium.
26′.	4394·6.	?
27′.	4245·2.	Iron.
28′.	4235·5.	
29′.	4233·0.	Iron and calcium.
30′.	4215·0.	Calcium and strontium.
31′.	4077·0.	Calcium.

[1] We must infer that sodium, always present at greater density than most of the elements outside the photosphere, was on this occasion present in greater quantities than usual, producing the widened absorption-bands which indicate increased density. At a considerable height above the photosphere, where the density was less, some local cause increased the temperature of the sodium vapour, so that it gave lines of emission seen bright on the broader absorption-bands. But above this region of higher temperature the sodium vapour, here still rarer, was cooler again, and so produced absorption-lines, dark on the broader emission-bands.

[2] The author of the article on the Sun in the *Encyclopædia Britannica* appears not to have heard of Professor Young's researches on the Sierra-spectrum, as he mentions but a few lines, and only eight as detected after the year 1869. It is only under very favourable conditions, such as Professor Young secured (chiefly by observing from a great height above the sea-level), that most of the more delicate Sierra-lines can be seen; but Professor Young's well-de-

(954.) Janssen, Lockyer, Secchi, and Zöllner all succeeded in inferring the shapes of certain coloured prominences by taking successive sections such as are shown in fig. 272 and combining them together. But Huggins, who had been the first to publish a detailed account of the method those observers thus employed, showed how the method could be extended so that a prominence could be seen bodily instead of piecemeal.[1]

(955.) Suppose that in place of a narrow strip at *s s′*, in figs. 272 and 273, we view spectroscopically a space such as is shown in fig. 278—in other words, let the slit be considerably widened, so that the field of view includes the prominence P P′—then the

FIG. 278.—Illustrating the Spectroscopic Method of seeing a Prominence.

part S S′ of the sun will produce a solar spectrum—altogether impure, of course, on account of the great width of S S′, and brighter than the solar spectrum as seen through a fine slit in the same degree that the slit is widened, because so much more light is admitted through the open slit. All the remainder of the space, including the prominence P P′, will give an impure solar spectrum due to the illuminated air, and also brightened in proportion to the widening of the slit. Three coloured images will be formed of the prominences (other fainter ones need not be considered), one red at C, one orange near D, the other greenish-blue at F. These images will be as bright (neglecting variations in the intrinsic brilliancy of the prominence) as the corresponding lines in the cases illustrated by figs. 272 and 273 ; and though the background of the spectrum is brightened by opening the slit, this brightening may be corrected by increasing the dispersive power of the spectroscopic battery, and then these images of the prominence will be as well seen as the prominence bright lines had been. Of course this method does not allow of all the images being seen at once, as shown in fig. 278. The red image must be examined alone, or the orange-yellow alone, or the greenish blue.

(956.) Many observers have employed the open-slit method successfully from the year 1869 until now. Among these may be specially named Messrs. Zöllner, Lockyer, Respighi, Secchi, and Tacchini.

In 1869 Zöllner noted that there is a material difference between the red and the blue image on the one hand and the yellow on the other. The latter is very intense only in close proximity to the sun's limb,[2] and corresponds there to the other images, but the more delicate details disappear at a greater distance.

served reputation alike for accuracy and for skill in delicate spectroscopic researches suffices to remove all doubt respecting the trustworthiness of his observations.

[1] It seems to me that he thus proved unmistakably that the vague suggestion made as early as 1867, 'May not the spectroscope show something of the prominences?' indicated no clear apprehension of the nature of the spectroscopic method. For otherwise Mr. Huggins would hardly have been the first to see his way to the extension of the method to show the prominences bodily. As it was, none of the first observers of the spectra of prominences on the uneclipsed sun went a step beyond that part of the track which Mr. Huggins had definitely cleared in February 1868.

[2] Zöllner's results as regards the coloured images correspond with those obtained by Secchi in observing the coloured *lines*.

(957.) Zöllner was the first to arrange the prominences into two distinct classes —the cloud-like and the eruptive. Respecting objects of the former class, he remarks that they remind him of the different forms of our clouds and fogs. 'The cumulus type is completely developed in some of the figures. Other formations remind one of masses of clouds and fogs floating closely over lowlands and seas, whose upper parts are driven and torn by currents of air, and which present the ever-varying forms so well known when viewed from the tops of high mountains.'[1]

(958.) The eruptive forms recognised by Zöllner appeared to him to indicate the existence of some enclosing shell or layer (*Trennungschicht*) through openings in which glowing hydrogen is shot forth in great jets. In one case he saw a column of glowing hydrogen 40.000 miles in height, which appeared to have shot out as if through a rifled opening, since it presented a strikingly spiral structure. Fifty-eight minutes later the upper part of this column of hydrogen had swollen out into a bulbous form above the columnar and spirally striated portion still remaining beneath, proportioned to the upper part somewhat as the stem of a palm tree to the leafy portion above.

(959.) Respighi, who in 1870 made a series of systematic studies of the coloured matter (Sierra, cloud-forms, and eruptive prominences) around the sun, maintained that the prominences are all phenomena of eruption. He found that the formation of a prominence is usually preceded by the appearance of a rectilinear jet, either vertical or oblique, and very bright and well defined. This jet, rising to a great height, is seen to bend back again, falling on the sun like the jets of our fountains, and presently the sinking matter is seen to assume the shape of gigantic trees more or less rich in branches and foliage. Gradually the whole sinks down upon the sun, sometimes forming isolated clouds before reaching the solar surface. In the upper portions of such prominences most remarkable and rapid transformations are often witnessed. But whereas some prominences which have assumed the cloud-like aspect disappear in a few minutes, others remain visible for many successive days.

(960.) Respighi inferred (as Zöllner had done) that the solar eruptions must take place through some compact substance forming a solar crust or shell. It would follow necessarily, though neither he nor Zöllner knew of the evidence (Arts. 787-792) which

[1] An important observation was made by Zöllner on June 27, 1869. 'On this day,' he says—'the first clear day after a long spell of cloudy weather—I observed the bright protuberance-lines, without, however, being able at that time to make a complete observation of these formations. As soon as I approached the slit of the spectroscope to a certain position in the sun's limb, where the protuberance-lines appeared particularly long and bright, brilliant linear flashes passed through the whole length of the dull spectrum *over* the limb of the sun, about three or four minutes distant from the latter. These flashes passed over the whole of the spectrum in the field of view, and became so intense at a certain point of the sun's limb as to produce the impression of a series of electrical discharges rapidly succeeding one another and passing through the whole of the spectrum in straight lines. Mr. Vogel, who afterwards for a short time took part in these observations, found the same phenomenon at a different portion of the sun's limb, where protuberance-lines also appeared. 'The phenomenon can be explained,' adds Zöllner, 'by the hypothesis that small intensely incandescent bodies, moving near the surface of the sun, emitted rays of all degrees of refrangibility, and produced flashes of a thread-like spectrum as their image passed before the slit of the spectroscope.'

It may be noticed, as confirming Zöllner's observation and the conclusion to which he was led, that Mr. Gilman observed during the eclipse of 1869 several bright red points in the heart of the large red prominence (G of fig. 270, Art. 935).

M. Trouvelot observed bright linear flashes across the spectrum of prominences before totality during the eclipse of July 1878; and in describing the phenomenon mentions that he had noticed it on six previous occasions.

forces the conclusion upon us, that the source of the eruption lies many thousands, perhaps many tens of thousands, of miles below the surface we see. It is difficult to form an opinion as to the origin of the explosive force ;[1] but it is not difficult to recognise many ways in which explosive action may be generated within a mass so full of might and activity as that of the orb which lies concealed within the glowing surface we call the solar photosphere.

(961.) Secchi's researches supplemented—partially confirming but also partially correcting—the results obtained by Zöllner and Respighi. The following are his conclusions, which, though obtained many years since, remain valid to this day :—

The prominences may be divided into three classes—heaps, plumes, and jets :—

(1) The *heaped prominences* include many varieties of form, but need no description.

(2) The *plume-prominences*, most of which are, in fact, Zöllner's *cloud-prominences*, show no signs of eruptive origin. They often extend to enormous heights, and usually last longer than prominences of the eruptive sort ; but they are subject to rapid changes of shape. They are distributed indifferently over the sun's surface.

(3) The *jet-prominences* alone present the characteristics attributed by Respighi to prominences of all orders. They are obviously products of eruption. Their luminosity is intense, insomuch that they can be seen through the Sierra, which would often imply that they are seen through several thousand miles of the luminous gas forming this envelope. When they have reached a certain height they cease to grow, and become transformed into intensely bright masses, which eventually break up into fiery clouds. The jet-prominences last but a short time, rarely an hour, frequently but a few minutes. *They are only to be seen in the neighbourhood of spots. Wherever there are jet-prominences there are also faculæ.* But the converse does not hold, for faculæ are often seen without accompanying prominences of the eruptive sort. 'It would seem,' says Secchi, 'that *in the jet-prominences a part of the photosphere is lifted up*' (*or broken through*), whereas in the case of the plumes only the Sierra is disturbed. *The spectrum of the jet-prominences indicates the presence of many elements besides hydrogen.*

[1] I suggested in 1870 something akin to geyser action, with due reservation as to the difference of condition between the sun's interior and the regions whence geyser-ejections take place. 'The temperature and pressure at which dissociation occurs,' I wrote, 'may bear the same relation to the outbursts of these gaseous masses that the temperature and pressure at which water is converted into steam bear, in Bunsen's theory of geysers, to the occurrence of geyser-eruptions. But it is probable that changes involving much higher temperatures than this view would imply are in question. We have to deal with conditions under which probably not only the so-called elements into which terrestrial compounds are dissociated by high temperatures are themselves dissociated, but under which the still more elementary forms thus produced (or rather maintained) pass into various allotropic conditions, with corresponding changes in the law connecting pressure and density. It is evident that if a large quantity of any vapour in the sun's interior is in such a condition that a slight change of temperature or pressure, or both, will cause it to pass to another of its forms, where the law connecting density and pressure will undergo a marked change, a sudden great expansion or contraction may be brought about by slight movements or other changes affecting temperature or pressure. (Those who have followed the history of chemical research since the recognition by Cagniard de la Tour, Andrews, and Hannay of the critical temperatures for gases, will recognise the possibility, at any rate, of great disturbances being thus brought about by comparatively slight local changes.) Sudden great expansions would necessarily lead to eruptive action ; and sudden great contractions, though not leading to such action directly, would as certainly lead to disturbances which would be followed or accompanied by expansion and eruption.

(962.) It is clear that in the jet-prominences we have precisely those signs of eruption which (Art. 910) we were led to look for outside the visible surface of the sun. They appear over the right parts of the sun's edge, viz. over the spot-zones, and nowhere else; they are definitely connected with the spots and faculæ; they occur at the right times, viz. when the sun's face shows spots, and only at such times; they present manifest signs of being erupted from great depths; and the spectrum of every jet-prominence indicates the presence of elements belonging to parts of the sun lying deep below the photosphere.

(963.) If we had no other evidence than that afforded by the jet-prominences, instead of having been already directed by decisive evidence supplied by the phenomena of the spots themselves, we might confidently conclude that the sun-spots are phenomena of eruption.

(964.) Zöllner, Respighi, Secchi, and others agree in speaking of the hydrogen recognised in the jets as itself ejected, and similarly of the vapours of other elements. It appears to me altogether incredible that the real *ejecta* in these solar eruptions can be the glowing hydrogen mixed with other vapours, seen in the jets. These, in all probability, bear the same relation to the chief products of ejection that the smoke and other products of chemical change ejected from the mouth of a cannon bear to the ball projected by the exploding powder. Or, to take a more exact illustration, the luminous jets and streaks of hydrogen are no more to be regarded as themselves the products of ejection than the luminous streaks behind advancing meteorites are to be regarded as themselves projected through the air.[1]

(965.) All the phenomena of jet-prominences, but especially the changes

[1] During the eclipse of August 29, 1886, Tacchini discovered that the spectroscopic method of observing the sun's coloured prominences shows only the brighter parts of the prominences as they actually exist and as they are seen during totality. He observed the eclipsed sun through a telescope six inches in diameter, and noted (as during the eclipse of 1882) the existence of whitish extensions around and above the ruddy flames. Making spectroscopic study round the edge of the sun's disc after the eclipse, he found that the ruddy part—the *core*, so to speak—of these prominences, was all that could be seen of them by the spectroscopic method. This discovery seems only explicable by my theory that the ruddy, jet-like portion of the prominence owes its light, and therefore its heat, to the velocity of outrush with which ejected matter passes through the hydrogen and helium already outside the sun, and not to the outrush of those gases themselves in an intensely heated condition. For, outrushing gases brought from a region of great pressure to a region of very small pressure would expand rapidly and be quickly cooled, so that the outlines of the heated and luminous portion would be sharply defined, and they would be surrounded by a region not only cooler than the ejected matter, but even cooler than the surrounding atmosphere. On the other hand, ejected matter would travel outwards with diminishing velocity owing to the retarding action of solar gravity, while such portions as returned after reaching a certain height would not only be scattered around somewhat widely, but would reach the sun's surface with less velocity than they had had at leaving it, because of the effects of frictional resistance. Hence, above and around the region of rapid outrush, intense heat, and brilliant light, there would be a region where the hydrogen and helium in the sun's atmosphere would be heated by the rush of matter through it, and would therefore be luminous, but would be less heated than the region of outrush. This exactly corresponds with what Tacchini has discovered.

following the jet-like motion, indicate that matter much denser than hydrogen is shot through the region above the photosphere, and that the glowing hydrogen of the jets has not been itself ejected, but has simply been made to glow by the swift rush of the ejected matter through it.

(966.) The following case, interesting as the first instance of a great solar eruption, the phenomena of which were watched in detail, suffices to demonstrate the true relation of the hydrogen streaks seen in jet-prominences to the actually ejected matter :—

On September 7, 1871, Prof. Young, of Dartmouth, N.H., observed a remarkable outburst from the sun. At noon he had been examining with the tele-spectroscope an enormous protuberance or hydrogen cloud on the eastern limb of the sun (see fig. 279). 'It had remained,' Prof. Young remarks, 'with very little change since the preceding noon—a long, low, quiet-looking cloud, not very dense or brilliant, nor in any way remarkable except for its size. It was made up mostly of filaments nearly horizontal, and floated above the Sierra with its lower surface at a height of some 15,000 miles, but was connected with it, as is usually the case, by three or four vertical columns brighter and more active than the rest. In length it measured 3′ 45″, and in elevation about 2′ to its upper surface—that is, since at the sun's distance 1″ equals 450 miles nearly, it was about 100,000 miles long by 54,000 high. At 12h. 30m., when I was called away for a few minutes, there was no indication of what was about to happen, except that one of the connecting stems of the southern extremity of the cloud had grown considerably brighter, and was curiously bent to one side; and near the base of another at the northern end a little brilliant lump had developed itself, shaped much like a summer thunder-head. Fig. 279 represents the prominence at this time, *a* being the little "thunder-head."[1]

FIG. 279.—Hydrogen Cloud, 100,000 miles long, Sept. 7, 1871, 12h. 30m. (*Young*).

'What was my surprise, then, on returning in less than half an hour (at 12h. 55m.), to find that in the meantime the whole thing had been literally blown to shreds by some inconceivable uprush from beneath. In place of the quiet cloud I had left, the air, if I may use the expression, was filled with flying *débris*—a mass of detached vertical fusiform filaments, each from 10″ to 30″ long by 2″ or 3″ wide, brighter and closer together where the pillars had formerly stood, and rapidly ascending. When I first looked some of them had already reached a height of nearly 4′ (100,000 miles), and while I watched them they rose with a motion almost perceptible to the eye, until in ten minutes (1h. 5m.) the uppermost were more than 200,000 miles above the solar surface. This was ascertained by careful measurement; the mean of three closely-accordant determinations gave 7′ 49″ as the extreme altitude attained, and I am particular in the statement because, so far as I know, the matter of the Sierra

[1] The sketches do not pretend to accuracy of detail, except the fourth; the three *rolls* in that are nearly exact.

(*red hydrogen* in this case) had never before been observed at an altitude exceeding 5′. The velocity of ascent also, 166 miles per second, is considerably greater than anything hitherto recorded. A general idea of its appearance when the filaments attained their greatest elevation may be obtained from fig. 280. As the filaments rose they gradually faded away like a dissolving cloud, and at 1h. 15m. only a few filmy wisps, with some brighter streamers low down near the Sierra, remained to mark the place. But in the meanwhile the little "thunder-head" before alluded to had grown and developed wonderfully into a mass of rolling and ever-changing flame, to speak according to appearances. First it was crowded down, as it were, along the solar surface; later it rose almost pyramidally 50,000 miles in height; then its summit was drawn out into long filaments and threads which were most curiously rolled backwards and downwards, like the volutes of an Ionic capital; and finally it faded away, and by 2h. 30m. had vanished like the other. Figs. 281 and 282 show it in its full development; the former having been sketched at 1h. 40m., and at the latter at 1h. 55m. The whole phenomenon suggested most forcibly the idea of an explosion under the great prominence, acting

FIG. 280. The same region at 1h. 5m.

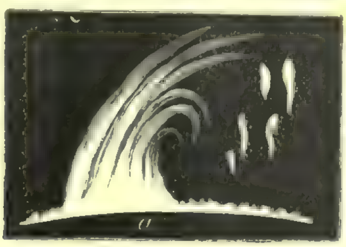


FIG. 281.—A portion (*a*) of the same region at 1h. 40m.

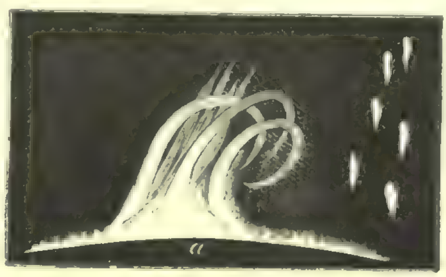


FIG. 282.—The same portion *a* at 1h. 55m.

mainly upwards, but also in all directions outwards, and then after an interval followed by a corresponding inrush; and it seems far from impossible that the mysterious coronal streamers, if they turn out to be truly solar, as now seems likely, may find their origin and explanation in such events.'[1]

(967.) It is impossible to regard the irregular filaments of hydrogen in

[1] The same afternoon a portion of the Sierra on the opposite (western) limb of the sun was for several hours in a state of unusual brilliance and excitement, and showed in the spectrum more than 120 bright lines whose position was determined and catalogued—all that Professor Young had ever seen before, and some fifteen or twenty besides. Other remarkable cases of disturbances affecting similarly two directly opposite parts of the sun's edge have been noticed (Art. 968). The idea that this may not be merely accidental seems to be confirmed by the resemblance often noticed between opposite streamers of the corona (see in particular figs. 285, 287, and 288). On the whole, however, it seems more likely that chance-coincidence is in question than that any real connection can exist between disturbances affecting opposite part of the sun's surface.

fig. 280 as masses of hydrogen themselves rapidly ascending. Obviously they are streaks of glowing hydrogen behind uprushing bodies, probably meteoric in character (see note to Art. 964). Such streaks would appear to rise as swiftly as the bodies really rising, since they would always be immediately behind—that is below—these bodies.

From calculations based upon the observed average rate of upward motion in the case of this remarkable outburst, I have shown [1] that the velocity of outrush past the level of the photosphere cannot have been less than 257 miles per second, and was probably not less than five or six hundred miles per second. Such velocities as these last would carry matter away from the sun, never to return—a velocity of 382½ miles per second sufficing (Art. 722) for this. Even the velocity just named would carry matter well into the region occupied by the solar corona, so that we are naturally led to study this solar appendage next, as likely to afford us evidence respecting the eruptive theory which I have selected as the most suitable line on which to thread the principal solar phenomena.

(968.) Before passing, however, to the corona, I must mention that even more remarkable disturbances than that witnessed by Professor Young have been observed during the last fifteen years. In some cases glowing hydrogen has been seen to attain a height of 350,000, even of 400,000, miles from the sun's surface. In one or two instances the singular coincidence noted by Professor Young, viz. that the region of the sun opposite the region where a great disturbance has taken place should also be strangely disturbed, has been noticed. In one case M. Trouvelot noted two immensely tall jet-prominences, among the loftiest yet observed, at precisely opposite points of the sun's limb!

(969.) As to the velocities observed in these eruptions, the actual measurement and timing of thwart motions can very seldom be effected satisfactorily; and whatever velocities may be estimated in this way, we may be sure that the real velocities are very much greater. Measurements by the spectroscopic method would be much more satisfactory, but the nature of the case prevents the astronomer from noting actual velocities of eruption in this way, since these are not velocities of recession or of approach. (No eruption occurring at a place on the middle of the solar disc could be satisfactorily detected by the spectroscopic method.[2]) All the more satisfactory,

[1] See my *Geometry of Cycloids*, Section VII., on the Graphical Use of Cycloidal Curves.

[2] The application of this method to movements taking place in the Sierra has already been considered. In observing prominences the spectroscopic method gives similar results; but the lines distorted through the effects of motion are longer, and present a more remarkable appearance. Fig. 283 shows the nature of the observed displacements. (The dots below the portion of each spectrum—in the neighbourhood of the F-line—indicate velocities of 8, 16, 24, and 32 German miles, each about 4⅗ English miles, per second from or towards the eye.)

Some doubts have been suggested in regard to the spectroscopic method of recognising veloci-

then, is the evidence of very rapid motions towards and from the eye in the outskirts of a spirally ascending jet. In an eruption witnessed on July 1, 1877, by M. Jules Flenyi, at Kalocsa, in Hungary, the velocities of approach and recession indicated by the spectroscopic method exceeded 300 miles per second. As the vertical portion of the velocity must, from the observed forms of spiral jets, be much greater than the velocity towards the eye on one side and from the eye on the other side of the spirally uprushing matter, the enormous velocities which I inferred in the case of the eruption witnessed by Professor Young in 1871 are more than confirmed. It is a demonstrated fact that masses of matter are from time to time ejected from the sun (probably, as fig. 280 suggests, in the form of flights of small highly condensed bodies) with such velocities that they never return to him, but, unless perchance captured by one or other of his family of planets, pass away into interstellar space, visiting the domain of some other sun, and thence, unless captured there, passing away to another and another, flitting thus from sun to sun, until in the fulness of time they fall through some planet's atmosphere, and shining therein for a few seconds as falling stars, their careers as independent bodies are brought to a close.

(970.) It may fairly be believed that during the earliest total solar eclipses observed by mankind the corona, or crown of glory, which surrounds the black disc of the moon must have attracted attention. Yet records of this phenomenon are neither so numerous nor so distinct as might have been expected.

Plutarch describes the appearance actually presented by the corona when he remarks, 'Even though the moon should hide at any time the whole of the

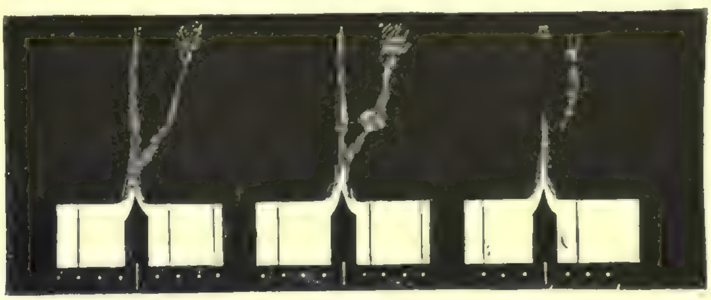


FIG. 283.—Illustrating the Spectroscopic indications of rapid Motions in tall coloured Prominences.

ties of recession and approach, partly because of the immense velocities thus indicated, and partly because of peculiarities affecting the behaviour of the different lines of the same element, observed in the neighbourhood of the sun. As respects these last, they may be explained by the probable dissociation of the so-called elements into more truly elementary matter, as suggested by Dr. Sterry Hunt, and confirmed by Professor Clarke. As to the trustworthiness of the method, this has been practically demonstrated by the measurement of the sun's rotation-velocity by this method, the approach on the eastern and the recession on the western side corresponding closely, as determined by the spectroscope, with the rates calculated from the observed motion of the spots.

sun, still the eclipse is deficient in duration as well as amplitude, for a peculiar effulgence is seen round the circumference which does not allow a deep and very intense shadow.'

Some of the earlier observers of total solar eclipses would seem to have been misled by the great brightness of the corona close to the sun, and to have supposed that a ring of direct sunlight had remained uncovered.

(971.) Clavius having expressed his belief that the eclipse of 1567 was annular, Kepler was led to investigate the subject, and he proved that that eclipse must needs have been total. In 1605 he witnessed a total eclipse at Naples, and found in the features it presented the explanation of the remarks of Clavius. 'The whole body of the sun,' he says, 'was completely covered for a short time, but around it there shone a brilliant light.' We might suppose that he referred to the Sierra, because he says that the light was 'of a reddish hue;' but as he adds that it was 'of uniform breadth, and occupied a considerable part of the heavens,' there can be no doubt that he spoke of the corona.

(972.) During the seventeenth and eighteenth centuries the corona was frequently observed. The descriptions given by Wyberd, Plantade, Maraldi, Don Antonio d'Ulloa, and others indicate just such variety in the configuration, brightness, and extent of the corona as the observations of recent times have shown to exist, so that we need not occupy space here with the details recorded by those earlier observers.

(973.) An exception may be made, however, in the case of the total eclipse of 1733, which was well observed, and presented the following interesting features as studied at Catherinesholm in Sweden.

'A ring of light which appeared round the disc of the moon was of a reddish colour, but at a considerable distance from the sun the ring appeared of a greenish hue.' 'During the total obscuration the edge of the moon's disc resembled gilded brass, and the faint ring around it emitted rays in an upward as well as in a downward direction, similar to those seen beneath the sun when a shower of rain is impending.' These rays were evidently very faint, for they were not seen at all stations. They 'plainly maintained the same position, until they vanished along with the ring upon the reappearance of the sun.'

(974.) In the eclipse of 1778 the corona (according to d'Ulloa) appeared of a reddish hue towards the margin of the lunar disc; then it changed to a pale yellow; and from the middle to the outer border the yellow gradually became fainter, until at length it seemed almost white.

(975.) The corona was carefully studied during the eclipse of 1842:—[1]

Fig. 284 represents the general phenomena seen during the eclipse of 1842; but it must be remembered that on that occasion the pictures of the corona were

[1] Unfortunately the same observers who tell us about the appearance of the prominences are those from whom we derive our information respecting the corona. The accurate observation of both phenomena was more than could be expected even from the most skilful astronomers.

drawn after the eclipse was over, and represent merely what the observers remembered. It was observed that the light of the corona was not uniform, nor merely marked with radiations, but that, in places, interlacing lines of light could be seen. Arago, at Perpignan, recognised this peculiarity with the naked eye. He saw, 'a little to the left of a diameter passing through the highest point of the moon's limb, a luminous spot composed of jets entwined in each other, and in appearance resembling a hank of thread in disorder.'

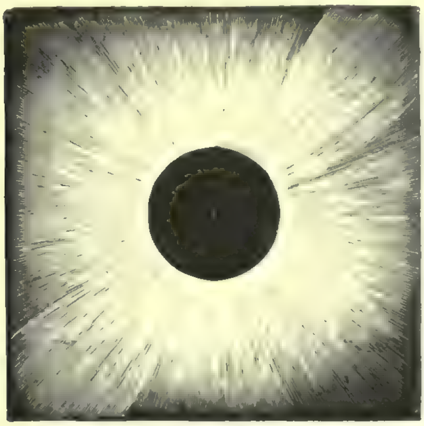


FIG. 284.—The Corona during the Eclipse of 1842.

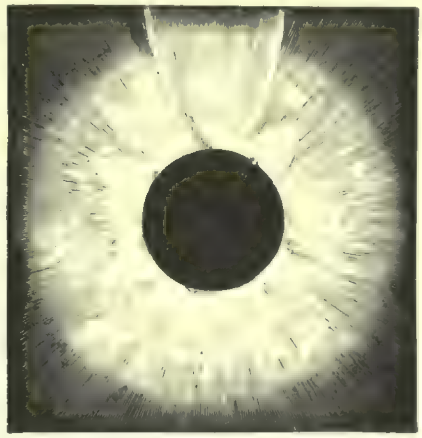


FIG. 285.—The Corona during the Eclipse of 1858 (*Liais*).

(976.) During the eclipse of 1851 Airy found the corona broader than in 1842. 'Roughly speaking,' he says, 'the breadth was little less than the moon's diameter, but its outline was very irregular. I did not notice any beams projecting from it which deserved notice as much more conspicuous than the others; but the whole was beamy, radiated in structure, and terminated—though very indefinitely—in a way which reminded me of the ornament frequently placed round a mariner's compass. Its colour was white, or resembling that of Venus. I saw no flickering or unsteadiness of light, it was not separated from the moon by any interval, nor had it any annular structure. It looked like a radiated luminous cloud behind the moon.'

FIG. 286.—The Corona as photographed during the Eclipse of 1860 (*Secchi*).

(977.) The eclipse of 1858, visible in Brazil, is chiefly remarkable on account of the strange drawing made by the French astronomer Liais (fig. 285). At the time, the accuracy of this picture was questioned; but later observations and photographs show that, though somewhat inartistic, and hard, it presents such features as the corona does occasionally exhibit.

(978.) The eclipse of 1860 is remarkable as the first in which photography was employed to secure views of the corona. It will be seen, on a reference to figs. 265 and 266, that Mr. De la Rue succeeded in obtaining traces of the corona. Those seen in Fr. Secchi's photographs are somewhat more distinct, the method he employed giving a smaller and more fully illuminated image. In a third of Fr. Secchi's photographs (fig. 286) the corona is yet more distinctly shown. 'The corona,' says Secchi, 'is very irregular, but it can be seen that it shows a greater extension towards the right and left than in other directions—that is to say, it is more fully developed in the plane of the equator (indicated by the cross-wire) than towards the poles.' But the figure indicates rather an extension opposite four points lying between the equator and poles than an extension at the equator. In the second of Mr. De la Rue's photographs the brightest portions seem similarly disposed.

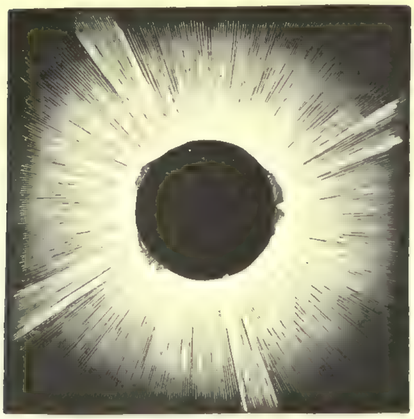


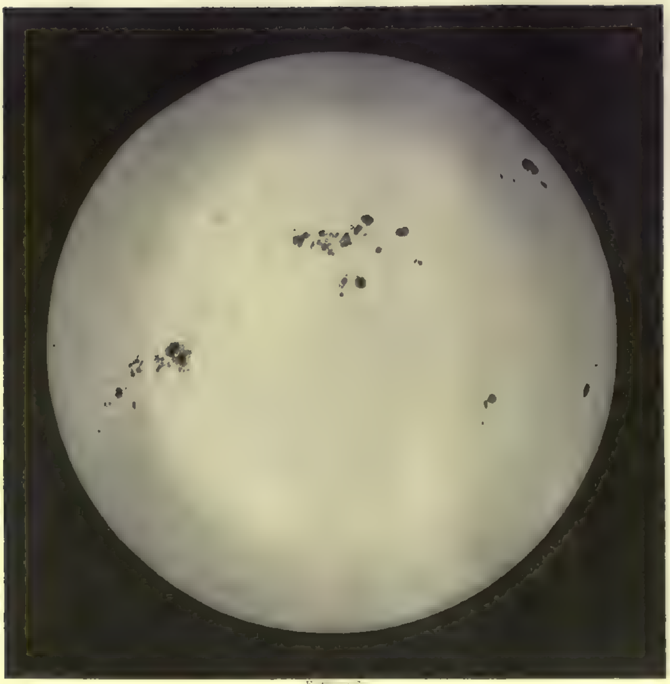
FIG. 287.—The Corona during the Eclipse of 1860 (*Feilitsch*).

(979.) Fig. 287 represents the corona as it appeared to the naked eye during the eclipse of 1860. Most of the observers considered it to be white. Fr. Secchi found that the corona continued visible forty seconds after the appearance of the first rays of direct sunlight. A similar peculiarity was observed during the eclipse of April 1865.

(980.) During the total eclipse of August 1867 the light of the corona was reddish close by the moon, to a distance of about five minutes. This reddish portion was not sharply bounded, but extremely diffused, and less distinct in the neighbourhood of the poles. Outside this inner region the corona was white. It extended much farther opposite the sun's equator than opposite the poles—a third of the moon's apparent diameter in the latter direction, but four-fifths of that diameter in the direction at right angles to this. Its white light *was not in the least radiated itself*, but it had the appearance of rays penetrating through it; or rather as if rays ran over it, especially in the direction of east and west, forming symmetrical pencils diverging outwards and passing far beyond the boundary of the white light. These rays had a more bluish appearance, and might best be compared to those produced by a great electro-magnetic light. In the white light of the corona, close upon the moon's limb, there appeared several dark curves. They were symmetrically arched towards the east and west, sharply drawn, and resembling in tint lines drawn with a lead pencil upon white paper. They gave the impression that they proceeded *from one point*, farther than the sun's centre below the part of the edge where they were seen. Beginning at the distance of one minute, they could be traced up to about nine minutes from the moon's limb. Throughout the duration of the eclipse *they underwent no alteration whatever*, remaining constant both in form and colour until the disappearance of the corona.

(981.) Fig. 288 represents the corona as drawn during the eclipse of August 1868 at Mantawalok-Kekee. It is important to notice that, unlike most pictures of the corona, this one can be trusted.[1] Several sheets of paper prepared beforehand were

[1] I use, in lecturing on the sun, a tinted picture of the corona of August 1868, kindly given me by Captain Bullock, of the British Government Navy, which, while it corresponds in all important



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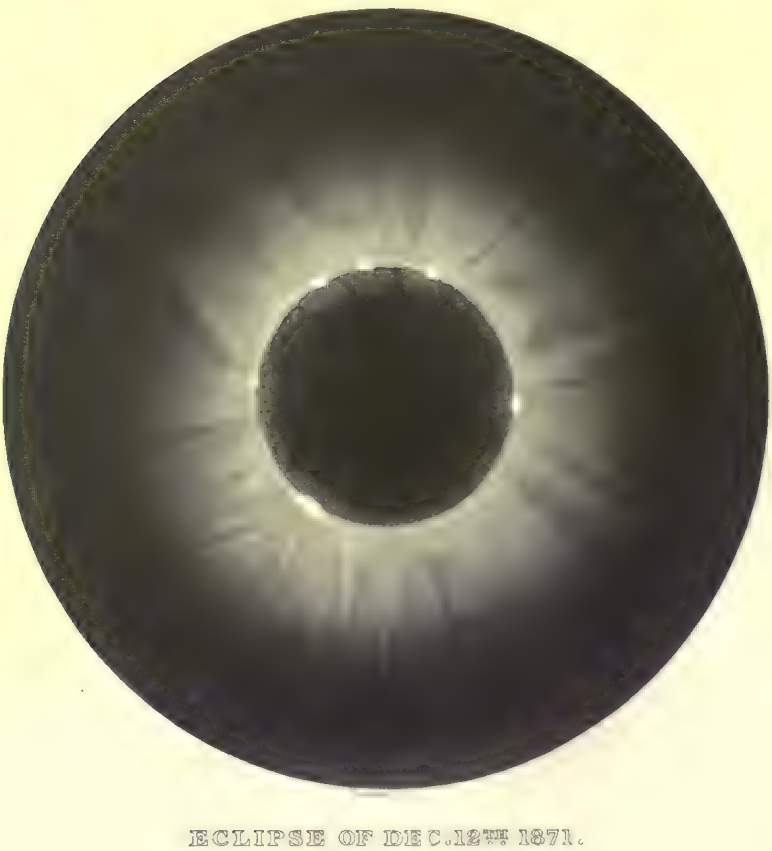
PHOTOGRAPH OF THE SUN, SEP. 22ND 1870.



ECLIPSE OF DEC. 12TH 1871.

FROM PHOTOGRAPHS BY C. L. TENNANT & MR DAVIS

London: Longmans & Co.

introduced one after another into a dark chamber, so that the image of the eclipsed sun fell upon them, and the features of the corona were rapidly sketched out on each. The corona had a somewhat triangular aspect; but there are four rays of the longer sort, so that the corona in this case, as in so many others, exhibits a general approach to the trapezoidal figure. The slightly curved streak of white light crossing the longest of the coronal beams is a very remarkable feature. This streak is described as of an intensely white and uniform light. It appeared (or was at least first noticed) some two minutes after the beginning of the totality, and remained visible until the sun began to reappear.

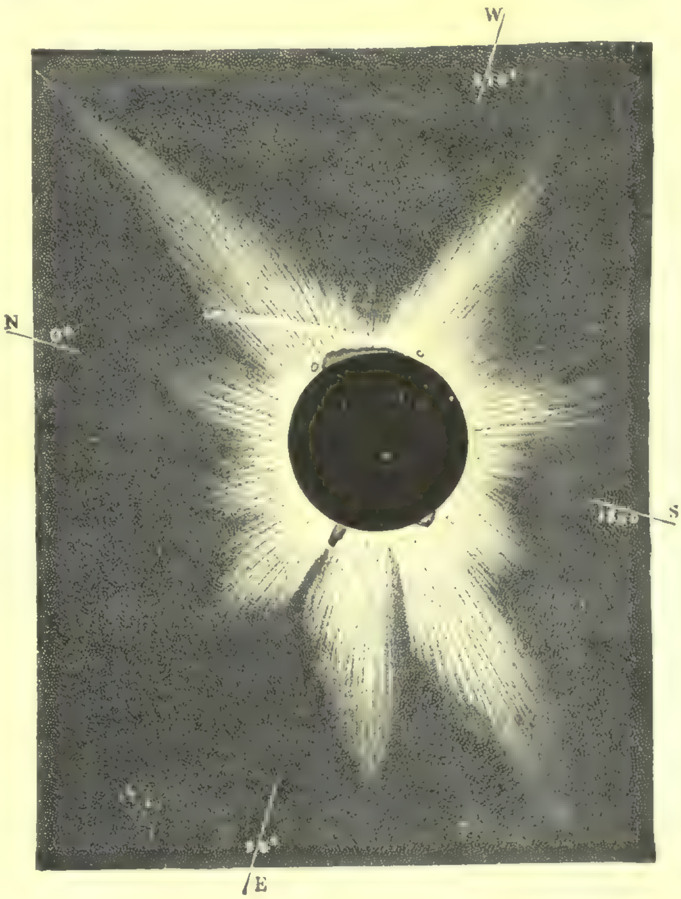


FIG. 288.—The Corona during the total Eclipse of August 1868, as seen at Mantawalok-Kekee.

(982.) In the total eclipse of August 1869 the corona had a trapezoidal form, the greatest extensions corresponding to solar latitudes midway between the equator and the poles. It was composed of an infinitude of fine violet, mauve-coloured, white, and yellowish-white rays, issuing from behind the moon. The exterior edge was very ragged in appearance, but did not possess a harsh outline, having, on the contrary, a soft blurred look. One observer described the corona verbally as appearing to be formed of countless fine jets of steam issuing from behind a dark globe. Near the moon's disc the light seemed almost phosphorescent.'

(983.) Professor Eastman's account of the corona deserves careful study, for it indicates careful observation. 'I was considerably disappointed,' he says, 'with the appearance of the colour and brilliancy, as well as with the extreme contour of the corona. Most observers have described the colour as "pure" or clear white, and the light as very brilliant, while nearly all the published sketches represent the contour as nearly circular and regular, and the coronal rays as radial, and equally distributed about the body of the sun. The colour of the corona, as I observed it, both with the telescope and without, was a silvery white, slightly modified in the outer portions by

respects with fig. 288, is more complicated and less radial. Captain Bullock, who was engaged at the time in surveying coast-lines, &c., assured me that he could answer for details of shape (but not of colour or intensity) as confidently as for the outlines of coasts which he had surveyed.

an extremely faint tinge of greenish violet; and I could not detect the least change in the colour or in the position of the rays during totality. The light of the corona was not brilliant—perhaps from the effect of haze—but appeared more like the pale light from the train of a meteor than anything else that I could recall at the time. The corona seemed to be composed of two portions, both visible to the naked eye; in which I was unable to trace any similarity of structure. The portion nearest the sun was about one minute high, forming nearly a continuous band about the sun, and appeared to be a mass of nebulous light, resembling in structure the most brilliant irresolvable portions of the Milky Way. Its colour was silvery white, and, like its density, appeared the same throughout its whole extent. The outer portion consisted of rays of light arranged in two different ways. In five places they were arranged into groups resembling star-points, composed of slightly convergent rays, but elsewhere were disposed on radial lines. The colour of the bases of the star-points and of the radial lines was the same as that of the inner portion, while the outer portion of the points had a very faint greenish-violet tint. The radial lines were the most prominent.' He adds that 'four of the star-points projected farther from the sun than the ordinary radial lines, and gave the contour of the corona the form of a trapezoid.' Between two of the largest protuberances scarcely any corona was observed.

(984.) The eclipse of 1869 was the first to show the effect of elevation in increasing the apparent extent of the coronal radiations:—

General Myer watched the progress of the eclipse from the summit of White Top Mountain, near Abingdon, Virginia, 5,530 feet above the sea-level. He remarks that, in the telescope, the corona or aureola exhibited a clear yellowish bright light closely surrounding the lunar disc, and fading gradually, with perhaps some tinge of pinkish green, into the line of the darkened sky. 'Upon this corona, extending beyond its brightest portion, the well-defined rose-coloured prominences were projected at various points of the circumference.' But it is when we turn to the description of the corona, as seen by the naked eye, that the characteristic peculiarities resulting from the position of the observer are recognised. 'To the unaided eye,' says Myer, 'the eclipse presented, during the total obscuration, a vision magnificent beyond description. As a centre stood the full and intensely black disc of the moon, surrounded by the aureola of a soft bright light, through which shot out, as if from the circumference of the moon, straight, massive, silvery rays, seeming distinct and separate from each other, to a distance of two or three diameters of the lunar disc, the whole spectacle showing as upon a background of diffused rose-coloured light. The silvery rays were longest and most prominent at four points of the circumference, two upon the upper and two upon the lower portion, apparently equidistant from each other, giving the spectacle a quadrilateral shape. The angles of the quadrangle were about opposite the north-eastern, north-western, south-eastern, and south-western points of the disc.'

(985.) Thus far it has been taken for granted that the corona seen during eclipses is a true solar appendage, and in no way associated either with the moon or with our own atmosphere. In reality this should be obvious so soon as the phenomena are considered with any degree of attention. Supposing *e* (fig. 289) to be the Earth, *m* the moon, and *s* a point on the sun's limb, then the coronal light opposite this point *s* is all included between the lines *e s* and *e s′*, and we know certainly that the luminous

or illuminated matter which produces this part of the coronal glory lies somewhere within the angle *s e s'*.

(986.) First, if the luminous matter surrounded the moon, it ought to show conspicuously round the full moon, where it would be under direct solar illumination; and the presence of matter, vaporous or dustlike, in such a position ought to be recognised in other ways and at other times than during solar eclipses. This is not the case.

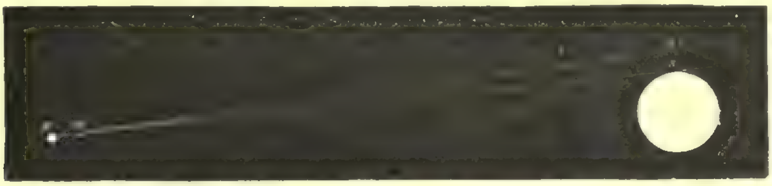


FIG. 289.—Illustrating the main objection to Oudemann's theory of the Coronal Rays.

(987.) Secondly, if the matter were within the space *e m*, the features of the corona would necessarily change rapidly and conspicuously during totality; moreover, matter surrounding the Earth within this distance could not but be recognised at other times than during a total eclipse.

(988.) Thirdly, the supposition that the luminous matter is at *e*, or is no other than the Earth's atmosphere lit up by the sun's rays, may be readily disposed of. For the shadow of the moon in our atmosphere has a nearly cylindrical form, such as *a b b'a'* (fig. 290); *b b'* being the elliptical shadow on the Earth's surface, and the least diameter of this ellipse being not less than 100 miles in any considerable eclipse. Within the region *a b b'a'* there comes no light (on the supposition we are inquiring into). To an eye, then, situated at *o*, at the time of mid-totality there will be no light at all within the conical space included within lines extending from *o* to all parts of the oval *a a'*, if we assume the light-reflecting air to extend about 500 miles in height, or from *o* to the oval A A' if we assume such air to extend about 200 miles in height. In either case an oval space with the moon's disc at its centre, but extending far from the moon's edge even along its shortest diameter, would be absolutely black, and outside that black space there would be light growing gradually brighter as the distance from the moon and the eclipsed sun increased. This is very unlike what is actually observed, viz. a black space corresponding to the cone *o m*, shown partially as enclosed by lines from *o* to the moon's edge, with a luminous glory within such a space as *c o c'* around the moon, and growing gradually fainter, instead of brighter, with distance from the place of the eclipsed sun.

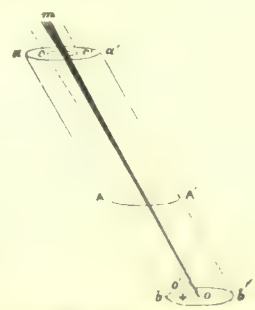


FIG. 290. The cylindrical Shadow of the Moon in the Earth's atmosphere.

(989.) It is certain, then, that the luminous or illuminated matter to which the corona seen towards *s s'* (fig. 289) is due must occupy some portion of space lying beyond *m*; and this being proved, we can have no hesitation, seeing the shape of the corona and its relation to the sun's globe, in recognising its true nature as a luminous or illuminated appendage of the sun surrounding his globe on all sides, extending towards us and on the side of the sun remote from us as well as all round his apparent outline.

(990.) All this was as certain in 1869, at least for those who considered the matter attentively (with competent power of appreciating geometrical relations), as it has been made since by more generally comprehensible evidence. But not everyone was willing thus to consider the matter; and strangely enough, some who strove (it may be supposed) seemed unable to do so. Thus, many looked to photography to remove a difficulty which should have had no existence. Here, then, we can consider the valuable results of celestial photography, as applied to the corona, without wasting time or space by noting, at any stage of the work, how erroneous theories, otherwise disposed of, were disproved by the photographic evidence.[1]

(991.) In the eclipse of 1869 the corona was photographed more successfully by Mr. Whipple at Shelbyville, Kentucky (fig. 291). The four-cornered aspect recognised by observers with the telescope may be recognised in this picture, and probably with a longer exposure on more sensitive plates the rays would have been presented, though not with the extension recognised by General Myer.

Fig. 291.—From a photograph of the Corona taken during the Eclipse of August 1869 (*Whipple*).

(992.) During the eclipse of 1870 the corona was successfully photographed by Mr. Willard (acting under the directions of Professor Winlock, of Harvard College) at Xerez, and by Mr. Brothers, of Manchester, at Syracuse. These photographs are represented in figs. 292 and 293. In Mr. Willard's (fig. 292) the extension of the field was limited by a diaphragm. Due account being taken of this, it will be seen that the two photographs, so far as they can be compared, are practically identical. Mr. Brothers' photograph was one of five, the duration of exposure being only eight seconds. Fig. 293 only indicates the extension of the corona in this valuable photograph; the negative shows details of structure resembling the features presented in Liais's picture of the corona of 1858 (fig. 285), but much more delicate. The second and fourth photographs obtained by Mr. Brothers (exposures eighteen and fifteen seconds respectively) were fairly good: they are valuable as indicating the unchanging aspect of the corona throughout totality. But the first and third (exposures three seconds and thirty seconds) were failures, owing to clouds. In fact, it was only towards the close of the totality phase that the sky around the eclipsed sun was fairly clear.[2]

(993.) The results obtained in 1870 were interesting as showing (when

[1] The long-continued advocacy of this notion illustrated singularly the unwillingness of the average mind to inquire into matters lying in the slightest degree beneath the surface. This was strikingly shown also by the way in which the decisive photographs of 1870 and 1871 were welcomed as settling a matter which needed no settling, and as chiefly valuable on that account,—that being in reality the least of the services they rendered science.

[2] Mr. Brothers' success under conditions so unfavourable, and with only eight seconds of exposure for his best photograph, seems to show that his method—that of photographing the corona with an ordinary camera mounted on an equatorial telescope—is that which should be employed to secure pictures showing the greatest extension of the coronal rays. As yet full advantage has not been taken of this method. Had an ordinary camera with a large field been used at one of the well-elevated stations in the eclipse of 1878 throughout totality, probably the corona would have been photographed to a distance of several millions of miles from the sun.

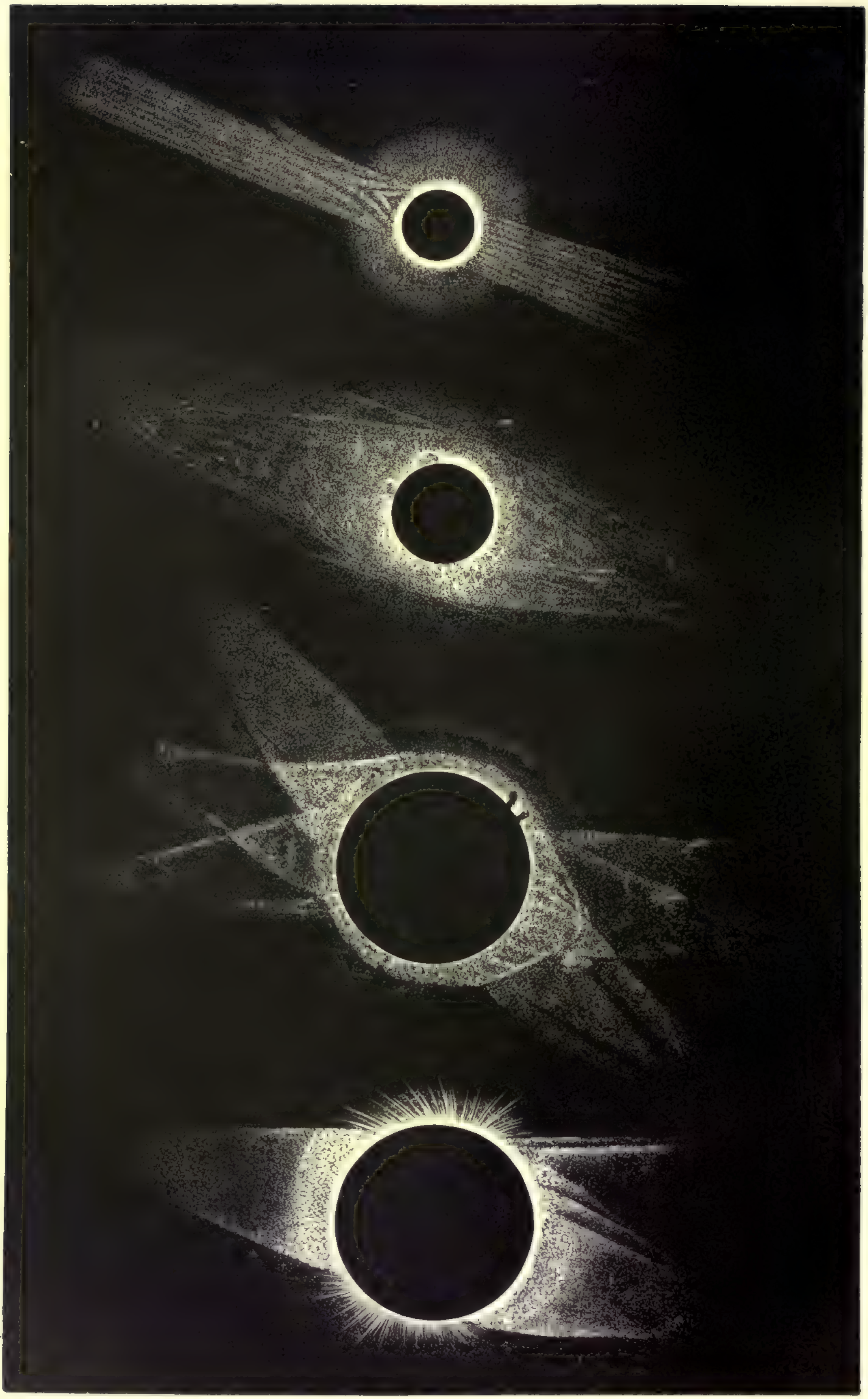
PLATE XV.

I.

II.

III.

IV.



DRAWINGS OF THE CORONA, JULY 29, 1878.

I. Prof. Langley. II. Prof. Newcomb. III. Mr. Denison. IV. From the photographs.

the photographic records, the best drawings, the verbal descriptions, and spectroscopic observations were carefully compared) that—

1. *Where any great gap or rift appears in the outer or radiated part of the corona, there a depression is seen in the inner and brighter portion.*

Fig. 292.—Mr. Willard's photograph, taken near Xerez.

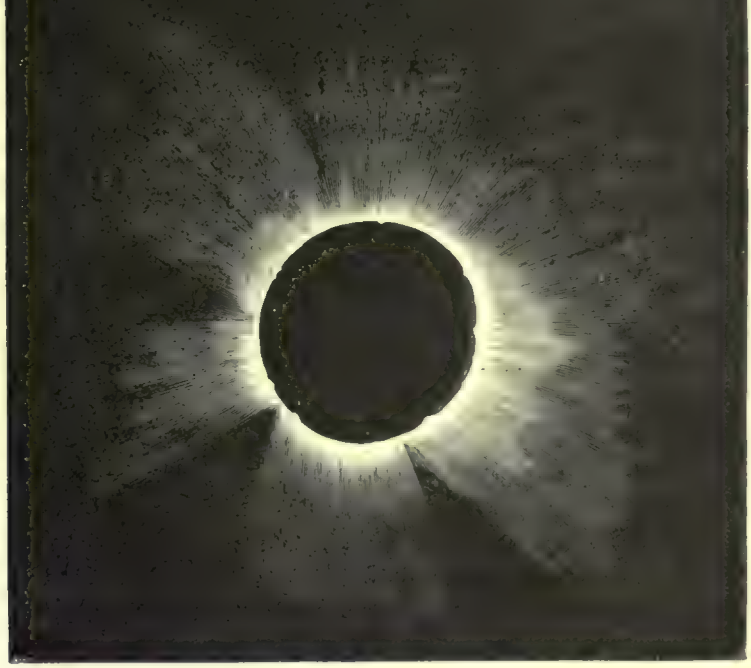


Fig. 293.—Mr. Brothers' photograph, taken at Syracuse.

2. *Where the inner portion of the corona is depressed, there the coloured prominences are wanting and the Sierra is very shallow.*

3. *The greatest extensions thus observed are opposite the solar spot-zones.*

These relations are well illustrated in fig. 294, showing the outer and inner corona and the prominences as drawn by Colonel (then Lieut.) Brown.

In these peculiarities we have decisive evidence that a portion of the coronal light is due to the ejection of matter from the sun's interior (deep down below the photosphere), and either chiefly or wholly from below those regions of the solar photosphere which we have called the spot-zones.

(994.) During the eclipse of December 1871 seventeen excellent photographs were obtained,—six at Baicul by Mr. Davis (photographer of Lord Lindsay's expedition), six by Colonel Tennant on the Neilgherries, the rest at Avenashi and Jaffra. Plate XIV. has been obtained by combining together the information afforded by all these pictures. But in reality a number of minute details, which could not be shown on the scale of Plate XIV. can be recognised in the photographs obtained on this occasion. For larger views and fuller details I would refer the student to the valuable collection of evidence respecting eclipse phenomena in Mr. Ranyard's 'Observations made During Eclipses' (*Mem. R. Ast. Soc.* for 1879).

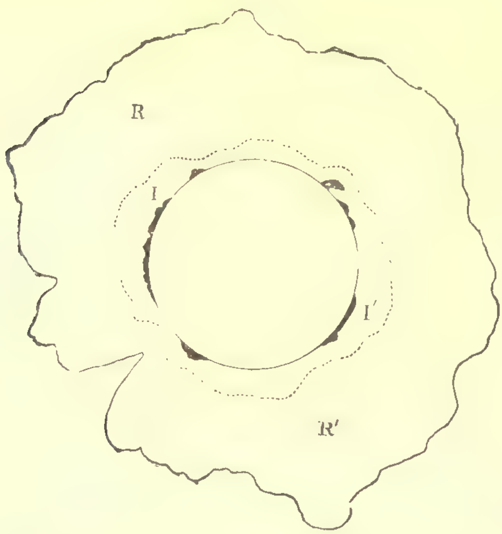


Fig. 294.—The Eclipse of December 1870. R R′, the outer radiated corona ; I I′, the inner and brighter corona (*Lieutenant Brown*).

(995.) The next important contribution to our knowledge respecting the figure and extension of the corona was obtained during the eclipse of July 29, 1878. On this occasion the shadow of the moon crossed the Rocky Mountains, and observations were made under favourable conditions from lofty stations. Amongst other such places Pike's Peak in Colorado was occupied, and at heights ranging from 5,000 to 10,000 feet above the sea-level observations were made which served to prove, what had already been recognised by the more thoughtful, that the visible extension of the corona depends almost wholly on the clearness of the air through which the region around the eclipsed sun is observed.

(996.) The greatest extension of the corona was observed by Professor S. P. Langley, whose drawing of it (made from a station on Pike's Peak, about 10,000 feet above the sea-level) is shown in the first figure of Plate XV. It is to be noted, however, that he observed the coronal extension on the right first ; he next turned to the left side, where the extension was greater, amounting to about six diameters of the moon ; he then looked round for rays, perceiving none ; and finally he looked again carefully at the left side, and found the extension to be now twelve diameters on that side. He felt that his eye was at this time only beginning to be in the right condition for recognising the delicate light of the long radial streaks, and adds that 'the twelve diameters through which he traced' the streak on the left '*were* (I feel great confidence in saying) *but a portion of its extent*.' (The italics are his.) It is clear that the inferior extension of the ray observed on the right corresponded to the less sensitive condition of Professor Langley's eye when he looked at that side of the corona. It will be observed that he assigns to it a length of more than four diameters, the ray on the other

side having an apparent length of six diameters, as seen afterwards, and going to a length of twelve diameters later. Had he been able to look at the left side later he would doubtless have found, like Newcomb, the extension on that side as great as on the other. Of the coronal wing which he did observe, he says that 'the central part was brighter than the edges, which were so diffuse as to make the determination of its boundary difficult. It was not so absolutely structureless as the zodiacal light, perhaps, and it appeared longer in proportion to its breadth than that; otherwise I should compare it to the zodiacal light with more confidence than to anything else.'

(997.) Several interesting views of the corona were obtained on this occasion. Professor Newcomb, hiding by means of a disc set on a pole a circle one degree in diameter (with the moon in the middle), was able to trace the corona in two long extensions, nearly coincident with the ecliptic, to a distance of about six degrees from the disc on either side. They looked, he says, very like the zodiacal light on a reduced scale. Six degrees from the disc would be $6\frac{1}{2}°$ from the disc's centre, or at the sun's distance more than ten millions of miles. His drawing is reproduced in fig. II. of Plate XV. In comparing it with the others it must be remembered that the black disc is not the moon, but the circular screen—having an apparent diameter of one degree and nearly twice the moon's.

(998.) Mr. Charles Denison's drawing of the corona (fig. III., Plate XV.) is interesting as indicating the effect of perspective. We see from it how the overlapping of the luminous tongues of the corona produces such shapes as had seemed perplexing in Liais's drawing. The two well-marked star-pointed radiations shown on the left (with apices about two-thirds of a diameter from the moon's edge) are seen not to indicate any actual radiations having these definite shapes, but to be produced by the overlapping in the visual field of three much longer radiating beams. We may infer that in all probability these longer beams are themselves in turn produced by the overlapping of beams longer still, but too faint to be separately discernible. It will be observed that as thus interpreted the several outlines of these pointed beams are in all cases real; what is unreal is the association of two outlines which really belong to different streamers into the apparent outline of a single-pointed beam.

(999.) We see no indications of the inner set of pointed beams in fig. IV., Plate XV., which represents the results of photography as employed at various stations during the eclipse of July 1878. From this we may infer that the eye was more sensitive to slight differences of luminosity in the coronal beams on this occasion. The absence in the photographic picture of some of the beams shown in Mr. Denison's drawing indicates their relatively small actinic intensity. On the other hand the delicate curved streaks seen over the parts of the moon's limb between the long streamers are much better shown in the photographs than in most of the drawings.

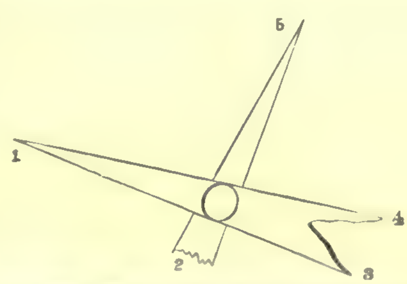


FIG. 295. Shape of the Coronal Beams seen by Professor Cleveland Abbe, July 29, 1878.

(1000.) Professor Cleveland Abbe, observing the eclipse from a station on Pike's Peak only about 5,000 feet above the sea-level (mountain-sickness having compelled him to retreat from the higher station) recognised features which escaped the attention of most other observers. He had no optical or other instrument, but a pair of spectacles. He first noticed the beam marked 1 (fig. 295).

It had then a length of three diameters; but in a minute or two he could trace its tapering end to a distance of six diameters. This tapering beam is evidently but the core of Langley's (compare I. and II., Plate XV.) Beam 2 was seen as soon as 1, as was also 3. No. 4 was not noticed at all until the totality was half over; it was brighter at the end farthest from the sun. No. 5 extended fully five diameters from the sun's edge; it was fainter than 1. Its edges were straight, except where the coronal glare broadened the base.

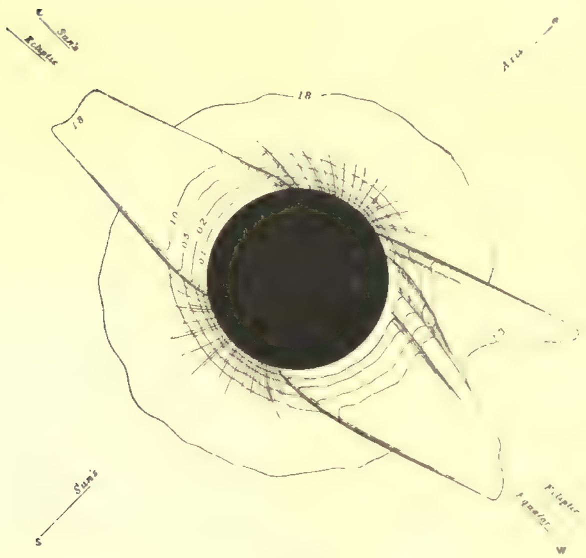


Fig. 296.
The Corona of July 1878. From combination of photographs.

(1001.) Fig. 296 indicates the results of the photographic analysis of the corona, the numbers showing the time (to tenths of a minute) required to photograph the various extensions. From the absence of any trace of Professor Abbe's beam 5 it may be inferred that it was much fainter than the others. In fact, no trace even of its brighter part can be seen in any other picture of the corona, except one by Mr. F. C. Penrose. (Too much prominence is given to this beam in Professor Young's view of the corona, 'from combination of various drawings,' fig. 74 of his treatise on the Sun.)

(1002.) Fig. 297 represents the corona as seen during the eclipse of May 1883. Interpreting the pointed beams as suggested in Art. 998, that is, regarding them as due to the overlapping (in the visual field) of longer but fainter beams—we must attribute to the corona of 1883 a great extension in all directions—but chiefly in four—around the sun.

Fig. 297.—The Corona during the Eclipse of May 1883.

Fig. 298, representing the corona as pictured towards

the end of totality in the eclipse of August 29, 1887, is worth studying in the light of the evidence supplied by Mr. Denison's picture of the corona on July 29, 1878. Obviously the strange lenticular forms on the right are effects of perspective (akin to atmospheric) and foreshortening.[1]

FIG. 298.—The Corona during the Eclipse of August 11, 1887 (*Khandrikoff*).

(1003.) It can hardly be doubted that the long extensions of the corona seen by Professors Langley and Newcomb (especially the more symmetrical view obtained by the latter, owing to the special precautions by which he secured the necessary visual sensitiveness) formed the core of the *zodiacal*, that slant tongue of light seen above the western horizon in spring evenings, and above the eastern horizon in autumn mornings. The consideration of this solar appendage may conveniently be deferred, but a chapter on the solar surroundings would be incomplete without some reference to it.

(1004.) The results of the spectroscopic study of the corona can be readily summarised, though several volumes like the present might be filled with what has been written on the subject.

In the eclipse of August 1868 the spectrum of the corona was not successfully studied. Colonel Tennant could see neither dark lines nor bright lines in the spectrum of the corona, but obtained simply a faint continuous spectrum. If we accept the observation as correct and (for the occasion) decisive, we must infer that in August 1869 the greater part, if not the whole, light of the corona came from incandescent matter, presumably meteoric bodies near enough to the sun to be raised to a white heat by his rays.

(1005.) In the eclipse of August 1869 Professors Harkness and Young recognised a green line (bright in the sense that it was brighter than the continuous spectrum on which it was seen, but not bright in the absolute sense). Professor Harkness failed to see any spectrum at all with a narrow slit, but on slightly widening the slit a continuous spectrum without any dark lines was seen, with the line in the green showing as on a relatively dark background. Professor Young saw this line, and suspected the presence of two others. The green line was subsequently identified as the more refrangible of two into which the line 1474 of Kirchhoff's scale (wave-length 5315·9) is divided under high spectroscopic dispersion. The other component of 1474 is a line of iron: but the coronal line, seen also in the spectrum of the corona, has not yet been identified with a line of any known element. It has been suspected that it either belongs to the

[1] I think it probable that the outlines of the two lenticular projections seen on the right of fig. 298 are incorrect, really overlapping where the smaller and higher lenticular projection is seen between them.

spectrum of hydrogen in an allotropic form (at high temperature and very low pressure) or else to one of the elementary constituents of hydrogen, regarded according to this view as a compound substance (see Art. 899, and Note).

(1006.) For some time after the eclipse of 1869 it was supposed that the green line of the corona corresponded with a green line, or rather band, in the spectrum of the aurora; but it has since been abundantly demonstrated that there is no point of identity between the spectra of the solar corona and of the terrestrial aurora.

(1007.) During the eclipse of 1870 the observations of Harkness and Young were confirmed. The spectrum of the corona showed also, as studied by Signor Denza, another bright line in the greenish yellow. The lines of hydrogen were also seen, but in such a way as to show that outside the Sierra and the prominences the hydrogen shone very faintly,[1] if perceptible at all.

(1008.) In the eclipse of 1871 spectroscopic results of great interest were obtained:—

First, Janssen showed that besides the 1474-line several other fainter bright lines exist in the spectrum of the corona, and proved (what had been regarded as doubtful after the eclipse of 1870) that the hydrogen lines are at least occasionally visible in the spectrum of the corona.

Secondly, the same observer recognised the solar dark lines (notably the sodium lines) on the faint continuous spectrum of the corona.

Thirdly, Respighi, using a prism in front of the object-glass of his telescope (and no slit), saw three coloured images of the gaseous corona, viz. one green image belonging to the 1474-line gas, and two images (one red, the other greenish blue) belonging to hydrogen. These images extended visibly to a distance of more than 200,000 miles from the sun's edge. But this would not indicate the actual range of the gaseous matter, only the distance to which Janssen's eye was able to trace it. For Mr. J. N. Lockyer, using a slitless spectroscope (a simple train of prisms—after a plan suggested by Professor Young), though he saw the same images as Respighi, could not trace them half as far from the moon's edge.[2]

Fourthly, it was definitively proved during this eclipse that, as had been suspected in the eclipses of 1870 and 1869, the rays, streaks, and irregularities (see Plate XIV.) of the corona's structure have no part (or but very slight part) in producing the bright lines of the coronal spectrum. For the lines were obtained in equal strength from the dark rifts as from adjacent bright rays, streaks, or patches.[3]

[1] Professor Young, reasoning on this point as though the hydrogen were of the same lustre wherever it was present, inferred a much greater extension for the gas producing the 1474-line than for the glowing hydrogen; but the observed relative intensities of the hydrogen lines and line 1474 could be equally well explained by supposing that while hydrogen extended as far, or nearly as far, as the gas corresponding to line 1474, it shone only with great brilliancy in the Sierra and prominences.

[2] Mr. Lockyer made his observation 80 secs. after total eclipse began, Respighi later, at the time of mid-totality. Mr. Lockyer was also observing under the mistaken idea that the only part of the corona really belonging to the sun is as much as this erroneous idea permitted him to see. Self-deception often plays a large part in observation. Still it remains clear that since Mr. Lockyer saw only the brighter parts of the coloured images, while Respighi, under more favourable conditions, saw about twice as great an extension, much more might have been seen than even Respighi saw had the conditions been more favourable still.

[3] I have not touched on the evidence of the polariscope either in regard to the photosphere or the prominences, because, in fact, this evidence is exceedingly doubtful. The results obtained from the polariscopic study of the corona are in like manner unsatisfactory, and, though not perhaps wholly valueless, would yet not justify

PLATE XVI.

Spottiswoode & Co. Lith. London

SOLAR PROMINENCES

Seen by the light of the C line

From a drawing by Trouvelot published in the 8th Vol. of the Annals of Harvard Observatory

(1009.) During the eclipse of July 1878 the lines of hydrogen were not seen in the coronal spectrum, or were barely suspected. The slitless spectroscope failed altogether, though several observers tried it. On the other hand the continuous spectrum was well seen, and a great number of the Fraunhofer lines were recognised. As the sun was free from spots, the solar pores small, and all the features of the photosphere such as to indicate comparative quiescence, whereas during the eclipse of 1871 the sun was actively disturbed, it was natural that the great contrast between the coronal spectra observed in 1871 and 1878 should be associated with the difference in the sun's condition as shown by the photosphere. Yet it appears to me unnecessary to imagine as some have done that the gaseous envelopes seen in times of spot frequency, and unseen at times when spots are absent, are poured out from the sun's interior, and withdrawn into it again, as solar disturbances alternately wax and wane. It seems far more reasonable to suppose that the gaseous surroundings of the sun simply alternate as regards visibility, than to adopt so fanciful, and indeed incredible, an idea as their alternate emission and withdrawal would involve.

(1010.) We have seen that times of great solar disturbance are times of great eruptional activity, as indicated by the prevalence of jet-prominences at these times and their absence at others. We have also found that the velocities indicated by the movements taking place in jet-prominences are such as would carry ejected matter far beyond the limits indicated by the height of the loftiest prominences. Indeed, this is so clearly recognised that many, apparently regarding it as the easier theory, suggest that it is by such eruptive action that the gaseous matter seen in the inner corona has been ejected. But we have seen that far more probably flights of small bodies are ejected in these eruptions; and the rush of these through the region of the inner corona would account satisfactorily for the increased luminosity of this region at the time of great solar activity.

(1011.) During the eclipse of May 1883 M. Janssen saw the dark Fraunhofer lines in the spectrum of the corona in great numbers, counting more than a hundred. On this occasion also M. Tacchini, an experienced student of solar spectroscopy, recognised in the great coronal plume (fig. 297) the bands belonging to the well-known carbon spectrum of comets. While one result indicated the presence of matter reflecting sunlight, the other showed that matter to be probably meteoric, and (like other meteoric flights) associated with cometic matter.

(1012.) A result demonstrated during the eclipse of August 1886,[1] but suspected

the introduction here of such an account of this method of observation, or such an explanation of the principles on which it depends, as would be essential for the student's correct interpretation of the evidence. It may suffice to mention, then, that during the eclipse of 1871 it was proved by polariscopic evidence that to a height of at least a million miles from the sun's edge the corona's luminosity includes reflected light. In what proportion reflected light and inherent light were present at different distances from the sun (on that occasion) the polariscopic observations do not enable us to determine.

[1] It had been hoped, from the comparative strength of the indigo and violet in the continuous coronal spectrum, that it might be possible to photograph the corona round the uneclipsed sun, by using absorptive media allowing such tints only to pass through. Mr. Huggins for a long time believed that certain radiated surroundings seen in photographs of the sun obtained under such conditions belonged in reality to the true solar corona. But during the eclipse of August 1886 evidence was obtained which proved decisively that Mr. Huggins's hopes were fallacious. For the same photographic methods (applied with the same instruments by the same observers) by which such radiated photographs of the sun had been obtained at Cape Town, showed no trace of the moon's form outside the solar disc during the partial phases of the eclipse of August 1886 as observed at that station. The same processes

during the eclipse of May 1882, strongly confirms the interpretation just suggested. It was found by Messrs. Turner and Perry that such lines in the coronal spectrum as are only obtained in the laboratory with high temperatures can be traced farther out in the corona than those visible in laboratory experiments when lower temperatures are employed.

(1013.) Such are the chief discoveries which have been made respecting the corona. They agree well with the general conclusions to which we have been led (Arts. 906 to 910) respecting the sun himself, and with the general evidence afforded (see Arts. 961, 966 and 967) by the eruptive prominences. But it is evident on the one hand that no simple theory can be advanced in explanation of the phenomena of solar appendages manifestly complex and varied, and on the other that the details of coronal structure and of coronal phenomena present problems far too difficult to be as yet solvable. All that seems tolerably clear respecting the corona may be summed up as follows. In the neighbourhood of the sun, and probably to a height of some 300,000 miles from his surface, exist large quantities of exceedingly tenuous and diffuse gases (not in atmospheric continuity) at a higher or lower temperature (probably because of solar ejection through them) according as the sun is more or less disturbed, and therefore spots more or less numerous. The gaseous matter in the sun's neighbourhood appears to be acted upon by repulsive forces akin to those which act in gaseous matter raised from comets' heads ; and apparently the action of such repulsive forces on the gaseous matter moving with considerable rapidity in the sun's neighbourhood produces the strangely curved streamers seen in the inner corona. The varying luminosity of the gaseous corona cannot, however, be satisfactorily explained without recognising the ejective action, carrying matter to greater or less distances from the sun, usually to return at once to him, but occasionally giving to such matter velocities so great as to carry it away from the sun and from the solar system for ever. The rays, streaks, and other irregularities seen in the outer corona must be attributed to meteoric and cometic matter, the meteors in thousands of intermixed and interlacing flights, and the essentially cometic matter subject here, as in the inner corona, to repulsive action producing forms akin to those seen in comets' tails. But wherever such interpretation is possible, we should prefer, I think, to attribute coronal features to complexly interlacing meteor streams diversely illuminated (according to their position) and occasionally even rendered incandescent by the solar rays. Passing farther and farther

failed to show the moon's form during the phases preceding and following totality at the West Indian stations. It had been proposed to test Mr. Huggins's method by comparing the photographs during the partial phases at these widely separated stations. But the absence of any trace of the moon's outline in the photographs obtained in either one or other region was of itself decisive, and unfortunately in an unfavourable sense.

outwards from the sun, the coronal beams first, and then the zodiacal, show less and less of structure, not because there are fewer peculiarities and irregularities in the distribution of cometic and meteoric matter (probably such peculiarities become more marked with increasing distance from the sun), but because the range of the visual line through such matter is much greater, and the multitudinous peculiarities really existing along these immense ranges of luminous or illuminated matter are not individually discernible, their combined effect being a general uniformity of illumination growing gradually fainter with increasing distance from the sun.

CHAPTER VIII.

THE INFERIOR PLANETS.

Vulcan (?), Mercury, Venus.

(1014.) The planets which travel on paths inside the Earth's path around the sun, are called *inferior planets*, according to the ancient system which regarded the distance of a heavenly body as indicating the diameter of its 'sphere,' and therefore the height of that sphere in the heavens overhead. There was, doubtless, reference to the relative power or influence of the heavenly bodies, those having the higher spheres being regarded as more powerful and therefore *superior*. But modern astronomy, though still using the distinctive terms *inferior* and *superior*, does not regard Mercury and Venus as in some way inferior to the Earth, and the remaining planets as superior.

(1015.) For about a score of years after 1860 many astronomers believed in at least one planet travelling inside the path of Mercury.

Leverrier had announced in the autumn of 1859 that his researches into the movements of the planets indicated the existence of bodies travelling nearer than Mercury to the sun and not far from the plane of Mercury's motion.[1] On December 22, 1859, he received a letter from M. Lescarbault, a doctor of Orgères, announcing that on March 26, 1859, Lescarbault had seen a round black spot crossing the upper part of the sun's disc on a slant (ascending) path, and traversing in 1h. 17m. a chord equal in length to rather less than one-fourth of the sun's diameter. Leverrier went to Orgères early in January 1860 to inquire into the matter. Adopting a tone which would perhaps have been suitable (but more probably not) had Lescarbault been one of his subordinates, he denounced Lescarbault for hiding his observation so long—supposing he had really made it. He asked for particulars, inquired how the doctor could count seconds, and was satisfied when a silk pendulum swinging seconds was shown him; examined the doctor's telescope; asked for his notes, and was content to find these 'on a piece of laudanum-stained paper,' used at the moment as a marker in the French Nautical Almanac (the 'Connaissance des Temps'); and finally seemed no whit surprised to learn that Lescarbault, using the boards of his workshop for his calculations, had preserved for nine months the calculations, (failures though they

[1] He had found that the motion of the perihelion of Mercury required to be increased from 9′ 41″ per century, the amount indicated by theory, to 9′ 45″, the amount actually deduced from twenty-one transits of the planet between 1697 and 1848. The motion of the planet's nodes agreed with theory. (See further, Art. 736.)

were) which he had made in chalk upon those boards. Learning in the village that Lescarbault was a good physician, Leverrier forthwith obtained for him the decoration of the Legion of Honour!

(1016.) Leverrier's calculations made the time of revolution of Lescarbault's planet 19 days 7 hours, the distance from the sun 140·7 (Earth's 1,000), the inclination $12\frac{1}{5}°$, the rising node in longitude 12° 59′, corresponding to the longitude of the sun on or about April 3 (so that central transits would occur if the planet were in inferior conjunction either on April 3 or on October 6). From Lescarbault's statement respecting the size of the dark spot, Leverrier assigned to Vulcan—as Abbé Moigno called the supposed planet—a volume equal to $\frac{1}{17}$ of Mercury's: whence, assuming the mass to correspond, it would follow that Vulcan would be quite unequal to the task of disturbing Mercury, as Leverrier's theory required.

(1017.) It may readily be shown[1] that—

(1) The greatest apparent distance of Vulcan from the sun would be about 8°.

(2) The greatest opening of his apparent orbit (attained about July 3 and January 5 each year) would be about $3\frac{1}{2}°$.

(3) Transits would occur if the planet were in inferior conjunction within about 18 days before and after April 3 and October 6: so that, since the planet's sidereal period is 19 days 7 hours and his synodical period less than $20\frac{1}{2}$ days,—

(4) There would usually be two transits, and always one transit between March 16 and April 21 (Vulcan being then near enough to his ascending node when in inferior conjunction), and between September 18 and October 24 (Vulcan being then near enough when in inferior conjunction to his descending node). Thus every year there would be generally four and never fewer than two transits of Vulcan. Every transit would be visible wholly or in part from more than half the Earth's surface.

(5) At or near his elongations Vulcan would be brighter than Mercury at or near his, Vulcan's greater proximity to the sun, and consequently greater illumination, somewhat more than making up for his smaller disc. Since Mercury can be seen with the telescope when much less than 8° from the sun, Vulcan at that distance would be quite readily seen with a telescope if not with the naked eye. As he attains that distance from the sun at average

[1] The method of calculating the orbit of an intra-Mercurial planet from such observations as Lescarbault's is indicated in my *Geometry of Cycloids*, p. 181. The problem corresponds with that of the determination of the orbit of a superior planet from observations made during a few hours in a single night. Each problem is simple. Mr. Chambers, in his *Descriptive Astronomy*, after giving Leverrier's elements of Vulcan, remarks that the application of Kepler's Third Law gives a remarkable semblance of truth to these elements. Since the elements had to be determined so as to fulfil Kepler's Third Law, it is not very surprising that the result obtained by Leverrier fulfils one of the conditions on which the solution of the problem depended: without this condition the problem would have been indeterminate. This affords no more evidence in favour of Vulcan than the obedience to Kepler's law shown by the two satellites of Mars, imagined in Swift's account of Laputan philosophy, proved the reality of those two satellites. Swift (or rather Arbuthnot) gave the semblance of truth to the Laputan satellites by assigning to them orbits and periods fulfilling Kepler's Third Law: Leverrier, accepting Lescarbault's observations, determined an orbit which would satisfy them while obeying that law. It was reserved for an American, Mr. E. Holden, to discover with the splendid refractor of the Washington Observatory an imaginary third satellite of Mars moving in direct disobedience to the law which harmonises all the celestial motions. But astronomers do not recognise this body.

intervals of less than $10\frac{1}{4}$ days, and remains each time for fully two days quite favourably placed for observation, he ought to be readily seen at one or other of the numerous observatories in different parts of the world more than thirty times each year.

(6) During one half, at least, of the total eclipses observed by astronomers, Vulcan would be a most striking object, shining much more brightly than a star of the first magnitude would shine when distant fifteen or twenty degrees from the eclipsed sun.[1]

(1018.) News arrived soon after Dr. Lescarbault's results had received official approval, that at the very time when Lescarbault saw the round black spot, M. Liais in Brazil, observing the same part of the sun with a better telescope, had seen nothing of the kind. Dr. Lescarbault and his staunch advocate the Abbé Moigno declined to answer M. Liais's remarks, because among them appeared one which seemed to impugn the doctor's veracity; for M. Liais did not hesitate to call attention to the fact that, whereas Dr. Lescarbault claimed to have seen the black spot enter on the sun's disc, he admitted afterwards to M. Leverrier that the spot was already on the face of the sun when first observed. Considering the character of M. Liais's objections regarded as a whole, and that this particular discrepancy might readily have been explained as due to forgetfulness, the reason given by Lescarbault and Moigno for remaining silent seems slightly fanciful.

(1019.) None now believe in Dr. Lescarbault's Vulcan. The 'planet of romance,' as the Abbé Moigno called it, has long been considered the 'planet of fiction;' though it may be believed Lescarbault saw a round spot[2] whose apparent westwardly motion (due to the change which the diurnal motion produces in the apparent position of the sun's vertex) he mistook for the westwardly motion of a planet. So late as April 4, 1875, there was still so much vitality in Vulcan that Herr Weber, a German astronomer, having seen at Pecheli, in China, a small round spot upon the sun (on April 4, 1875) which a few hours later had vanished, telegraphed the news to Europe, to the joy of the Abbé Moigno and M. Leverrier, but to the sorrow (possibly doubtful) of Lescarbault, who cherished strong feelings of dislike to Germany and all Germans. Luckily this spot had been scrutinised with a powerful telescope at Madrid, and photographed at Greenwich, so that its character as a solar spot (with penumbral fringe, and not even quite round) was decisively determined.

(1020.) Doubtless other round spots on the sun, a score or so of which have been mistaken for planets in transit, may be similarly interpreted. Only two or three have been seen by astronomers of repute, and these have been doubtful. None of the skilful

[1] Leverrier somehow fell into the mistake of supposing that, comparing Vulcan and Mercury at their elongations, Vulcan would be the less brilliant. But with the assigned volume of Vulcan, giving a diameter rather less than $\frac{2}{5}$, and a disc about $\frac{2}{13}$ of Mercury's, Vulcan, lit up more than $7\frac{1}{2}$ times as brightly owing to his greater proximity to the sun (this distance being to Mercury's as 141 to 387), would be brighter than Mercury nearly as 15 to 13.

[2] This is quite consistent with the fact that M. Liais saw no such spot at a time when, as he showed, Lescarbault's planet, if real, would have been already advanced by twelve minutes' motion on the sun's disc. For Lescarbault's spot might have formed after M. Liais's observations closed. This interpretation would not account for the delay in Lescarbault's announcement, or its coincidence as to time with Leverrier's appeal for an intra-Mercurial planet, and other singular circumstances, to say nothing of the contradiction noticed by Liais. But it is as far as we can reasonably go in accepting Dr. Lescarbault's statements.

observers who for sixty years have kept the sun's face under systematic survey have seen these planet-simulating spots.[1]

(1021.) Intra-Mercurial planets of a different kind have been suspected during total eclipse. The best authenticated instance of the kind may suffice to show how fallacious all such observations have probably been :—

During the eclipse of July 29, 1878, Professor Watson, whose discoveries of planetoids, and skill alike in observation and mathematics, compel respectful attention to all that he recorded, devoted his attention to the search for intra-Mercurial planets. The sun was situate as shown in fig. 299 (see also fig. 93). After sweeping over the region east of the eclipsed sun (that is, on the right), and finding nothing,

FIG. 299. - Professor Watson's observation of supposed intra-Mercurial Planets.

Professor Watson turned to the western side. He saw a star which he took for Zeta Cancri,[2] but afterwards (by a singularly complex series of corrections and comparisons between the indications of his own instrument and Professor Newcomb's) set

[1] There can be little doubt that most of the accounts of black spots have been made in as perfect good faith as Herr Weber's. It is indeed difficult to imagine a condition of mind permitting actual perversion of facts. We can even not be too earnest in denouncing such wrong-doing as would be involved in the embellishing of observations actually made to give them fictitious importance—a fault of which I cannot, for my own part, acquit Dr. Lescarbault, since he described what he certainly could not have seen. But we must not overlook the possibility of deliberate untruth, since science would suffer if we blinded ourselves to it. The particular class of observation we are now considering would seem specially to encourage such wrongdoing, because the ill-informed imagine that detection is impossible. As an example, among three or four known to me, I may mention a Mr. Tice, of Louisville, Kentucky, who, to support a system of weather prediction by which he had deceived many foolish persons, asserted that Lescarbault's Vulcan, which was necessary, as he affirmed, to his system, had to his knowledge passed across the sun's disc on a certain day in September, which he showed to be separated by an exact number of Vulcan's periods from Lescarbault's observation in March 1859. To give an air of innocence to his account, he said he mistook the black spot he saw for Mercury in transit, not knowing that he was thus proclaiming his ignorance as obviously as if he had spoken of a lunar eclipse when the moon was half full. But it was further pointed out to him that for a transit of Vulcan in September or October (that is, at the imaginary planet's descending node), a certain number of revolutions, plus *one half*, were required, unless Vulcan were to be seen *through* the sun. From the day when he made this mistake 'Professor' Tice, as he was called, began to lose ground even with the exceedingly ignorant persons who, until then, had put faith in him. But weather-prediction remains a fruitful form of charlatanry in America.

[2] I give Professor Watson's work in the order in which he first publicly announced it; in his official report he says nothing of the first view of Zeta Cancri.

at the spot marked *b*, a degree or so east of Zeta. He had been surprised to find Zeta so bright as this unknown star appeared. Returning towards the sun he got Theta Cancri, as he believed, into view, and saw at the same time [1] in the position shown at *a* a star about two magnitudes ('fully a magnitude,' he said later) brighter, where no star, at least no fixed star, should be. He then returned to the star which he had supposed to be Zeta and recorded its position. By this time totality was over. Professor Watson was convinced, and remained convinced to the time of his death, that *a* and *b* were both intra-Mercurial planets.

(1022.) It is practically certain, however, that what Professor Watson regarded at first as Zeta Cancri, though he was surprised by its brightness, was really that star, while the star he set at *a* was really Theta, and the star he mistook for Theta was a star of the sixth magnitude shown in fig. 299 at 20 (Flamsteed's number). It would not have been visible in the same field, but on this point Professor Watson's memory may have played him false. We must suppose that his eye had become accustomed to the eclipse-light while he was searching east of the sun, so that on the west the stars Zeta and Theta looked a full magnitude brighter than he expected to find them, while the small star 20 looked as bright as he expected to find Theta. Deceived by this last change, subjective though it was, he set Theta nearly two degrees east of its true place, and working out afterwards the position of the star he had rightly taken for Zeta, he set that star at *b*, a degree or more east of its right place. In this way, though he would not admit it, all that Professor Watson saw on that occasion can be satisfactorily explained. He does not appear to have ever duly considered the difficulty involved by the circumstance that both the objects he saw presented circular discs—precisely as stars seen with the small telescope he employed would have done,[2] whereas an inferior planet would only present that appearance if near superior conjunction. Now star *b* was as far away as Vulcan would be near elongation, when an inferior planet presents only a semi-circular disc; and even *a*, if a planet moving on an orbit inside Mercury's, would have shown some sign of gibbosity when situated where Watson sets this object.[3]

[1] In his official report Professor Watson used the words 'at the same time;' in a report published in the newspapers he was made to say 'in the same field'—not necessarily the same thing. He may have meant 'in the same sweep,' or even 'on the same occasion.'

[2] Powers being equal, the smaller the object-glass of a telescope with which a star is observed, the larger is the diffraction disc-image of the star.

[3] I attach no weight to the supposed observation of a strange orb near Theta Cancri by Mr. Swift, of Rochester, N.Y. Mr. Swift is now, I believe, in charge of an observatory at Rochester, generously erected by Mr. Warner from a portion of the proceeds of a patent medicine; and as Mr. Swift has done useful work in discovering a number of comets (for which Mr. Warner offered prizes), it may be assumed that he has now some skill in astronomical observation. But on the occasion of the eclipse of 1878 he manifestly had none. His whole attention during the totality was devoted, according to his own account, in trying to manage a telescope to which he had fastened a ten-feet pole, which allowed the telescope to be swayed in one direction, but stuck into the ground when he tried to sway the telescope back again. He saw two stars, one of which he knew (that is, thought he knew) to be Theta Cancri; but he did not know which of the two was Theta. They were about 8′ apart, and on a line pointing directly towards the sun, if observations made under the conditions described can be trusted. (Mr. Swift first set his intra-Mercurial planet at the place marked *c* in fig. 299; but afterwards, supposing that Professor Watson's *a* and his own unknown were the same, interchanged Theta and *c*.) But this is as entirely inconsistent with Professor Watson's observation of two stars—one of which was undoubtedly Theta, whichever we suppose to have actually been that star. Thrice Mr. Swift tried to sweep to the south, and thrice his pole-encumbered telescope brought back the two mysterious stars. He was still struggling with his strange polar attachment when the totality

(1023.) Whatsoever matter must be assumed to travel within the orbit of Mercury, to account for the motion of the planet's perihelion, is evidently neither gathered into a single planet nor distributed among several bodies which, though small, could be regarded nevertheless as planets. In the former case, we could not fail to recognise an orb so important and so brilliantly illuminated during eclipse, or by telescopic aid without eclipse, or when crossing the sun's face (which it must do frequently).[1] In the latter case, powerful telescopes could not fail to show each year many of the small planets in transit. The only supposition which remains available is, then, that the matter within the orbit of Mercury consists of multitudinous small bodies individually invisible. Many among these may be several tons, or hundreds of tons, in mass; but (when considered with reference to the enormous region they occupy, and compared with the masses of even the smallest planets) they must be regarded collectively as mere planetary dust.

(1024.) Mercury, the planet which travels nearest the sun, moves in an orbit having a mean distance of 35,915,000 miles from the sun, or about $\frac{387}{1000}$ths of the Earth's mean distance, in a period of 87·97 days, or short of 88 days by less than three-quarters of an hour. His path is more eccentric than that of any other among the primary planets, the eccentricity being no less than ·2056, so that the greatest mean and least distances are as 12,056, 10,000, and 7,944 respectively, or roughly as 6, 5, and 4. Hence the heat and light received from the sun when the planet is at its nearest, at its mean distance, and at its greatest distance are approximately as 36, 25, and 16. At its mean distance Mercury receives more heat and light from the sun than the Earth (comparing square mile for square mile squarely illuminated by the sun) in the same degree that the square of 1,000 exceeds the square of 387, or that 667 exceeds 100. It follows that the heat and light received by Mercury in perihelion at mean distance, and in aphelion, exceed the heat and light received by the Earth at her mean distance about as 8, $6\frac{2}{3}$, and $5\frac{1}{3}$ times respectively. The diameter of Mercury is about 3,010 miles, or rather more than $\frac{3}{8}$ths of the Earth's. There is no measurable polar compression; some

came to an end. No value whatever can be attached to observations thus made. Probably what Mr. Swift saw was 25 Cancri and the small unnamed 7th magnitude star shown near it in fig. 299.

[1] Venus, though her path is so slightly inclined to the ecliptic, transits the sun's face at intervals averaging about 60 years. [This is true, however, only during the long periods in which two transits, 8 years apart, occur after the long intervals alternately between $105\frac{1}{2}$ and $121\frac{1}{2}$ years. In reality (see my *Transit of Venus*, p. 117) there are not four, but only $2 + \frac{77}{135} + \frac{6}{31}$ transits in 243 years, whence it follows that the true average is about 85 years.

Mercury, with a path much more inclined, has his transits at average intervals of about $7\frac{3}{4}$ years (see Art. 1049). A planet as far within the path o Mercury, and travelling nearly in the plane of Mercury's orbit, as theory requires, would transit the sun's face at intervals averaging less than half a year. It would be a much more conspicuous object in transit than Lescarbault's 'black spot.' The average duration of transit would not be less than an hour and a half, and every transit would be visible over more than half the Earth's surface. It is impossible that ten years could pass without at least one transit of such a planet being observed; and utterly unlikely that even for a year such a planet could escape recognition in this way. Moreover, during nine eclipses out of ten a planet of the necessary size and illuminated so brightly (being so near the sun) would be staringly conspicuous.

observers have indeed supposed that they have recognised compression—even great compression ; Mr. Dawes, for instance, supposed he had measured a polar compression of $\frac{1}{29}$. But there can be no doubt from a comparison of all the results that, if there is any compression, it is too small to be measured by any means we have. Mercury's surface is rather less than $28\frac{1}{2}$ millions of square miles, or about $\frac{1}{7}$th of the surface of the Earth, and little more than half the Earth's land-surface. The volume of the Earth is about $18\frac{1}{5}$ times the volume of Mercury ; but the mean density of the planet is apparently somewhat greater than the Earth's, so that the mass of the Earth is little more than 15 times that of Mercury, whose mean density is estimated as exceeding the Earth's in the ratio of about 6 to 5.[1] Gravity at the planet's surface is about $\frac{5}{11}$ths of terrestrial gravity—more exactly $\frac{4577}{10000}$ths. We have no satisfactory determinations either of the inclination of Mercury's axis to the plane of his orbit or of the planet's rotation-period : but such observations as have been made suggest an inclination of about 70° ; that is, the equator of Mercury seems to have an inclination of about 20° to his orbit, and the planet to have a rotation period of 24h. 5m. If these results can be trusted, the seasons of Mercury, so far as they depend on the inclination of his equator, would resemble the Earth's ; but the Mercurial year would contain only about $87\frac{2}{3}$ Mercurial days, and each season only 21 days 22 hours of Mercurial time.

(1025.) Among all the various positions into which the two planets may come with regard to the sun, there are none which give the inhabitants of the Earth a satisfactory view of Mercury :—

We may conveniently regard the Earth as at rest in some point of her orbit, while Mercury makes a complete circuit of his, obtaining in this way all the various relative presentations of the two planets as satisfactorily as if we took the less simple way of carrying the two bodies simultaneously at their varying rates round their several orbits.[2]

If, however, we imagine our Earth at E_1, fig. 300, while Mercury travels round his orbit $m_1m_2m_3m_4$, we must suppose Mercury to complete a circuit in his synodical period of 115·877 days, not in his actual period of 187·969 days. For, each return of Mercury to m_1, the Earth being at E_1, implies a conjunction with the Earth, and the interval between successive conjunctions is asynodical period.

(1026.) 1. When Mercury is at m_1, it is clear that the planet will present towards E_1 its unillumined hemisphere. At his nearest, then, Mercury is invisible, unless indeed it so chances that he is so nearly on a line between S and E_1 that he is seen as a black

[1] The measurement of the dimensions of Mercury and the estimate of his mass are both open as yet to some degree of doubt, the former depending on observations, the latter on calculations of considerable difficulty (see Art. 710).

[2] It is true that, as the Earth's orbit has some degree of eccentricity, and Mercury's is exceptionally eccentric (as the figure shows), we should obtain slightly different results, according to the part of the orbit where we supposed the Earth to be set during the imagined revolution of Mercury ; but, for such considerations as are dealt with in the text, this is a matter of no importance.

spot on the sun's face; but usually, as the path of Mercury does not lie in the same plane as the Earth's path, the planet will be a little above or a little below the line S E_1, and then will be simply invisible.[1] 2. As Mercury passes on from m_1 towards m_2 he will show more and more of his enlightened hemisphere, until when approaching m_2 —more precisely at the point of his orbit touched by a tangent line from E_1 towards m_2—he shows as much of his bright as of his dark face. Here, then, he appears as the moon does when half full. But as his distance from the Earth has now greatly increased, his apparent size has considerably diminished. He is now at his greatest apparent distance from the sun. 3. Passing onwards towards m_3 Mercury shows more and more of his enlightened half, but grows smaller and smaller in apparent size, until, when at m_3, he turns his full face Earthwards, and is at his greatest distance. But at m_3 Mercury cannot be seen from E_1, lying now in the same direction (from E_1) as the sun at S, and being lost therefore in the sun's glory. 4. Advancing from m_3, Mercury passes through the same changes in reverse order. He turns less and less of his illumined face Earthwards till, a little beyond E_1 (where a tangent from E_1 touches his orbit), he only shows a half face, and still turning more and more of his dark hemisphere towards the Earth becomes invisible again at m_1. During the movement from m_3 through m_4 to m_1 Mercury has been growing larger and larger apparently, till at m_1 he presents the largest apparent disc.

Fig. 300.—Paths of Mercury (m_1m_4) and the Earth (E_1E_3) round the Sun, S.

[We should, of course, have obtained precisely the same results if we had imagined the Earth at E_2, E_3, E_4, or anywhere else on her orbit (apart from the slight effects of eccentricity), while Mercury travelled from m_2 round to m_2, or from m_3 round to m_3, or from m_4 round to m_4.]

(1027.) Combining the effects of varying apparent phase (as Mercury is viewed from the Earth, for from the sun there is no such change) and of distance, we see that the planet passes through the changes of aspect and apparent size shown in fig. 301.[2]

[1] We must, in fact, imagine Mercury travelling round S with only the excess of his angle-sweeping velocity over the Earth's. Since the Earth completes a circuit in 365¼ days, or at the average rate of rather less than one degree round the sun in a day, while Mercury goes round in 87·97 days, or at the rate of more than 4 degrees a day, we must imagine Mercury's rate of angular advance round S to be only the difference of these, or about $3\frac{1}{10}$ degrees a day, making the circuit relatively to the Earth last about 116 days. [Mercury's actual mean gain on the Earth, measured by mean angular velocity round the sun, is 3° 6′ 24·220″ daily.]

[2] The apparent brightness of Mercury is not precisely proportional to the apparent area of his illuminated portion; for the intrinsic lustre is different in different parts of the disc. If he were a smooth (but not polished) globe, having what the French call a *mat* surface, his lustre would be greatest at the point of his sphere which is the illuminated 'pole' of the 'great circle' bounding the illuminated hemisphere, and would diminish from that point down to nought at that

(1028.) Mercury and the Earth have the comparative dimensions indicated in fig. 302. The mean apparent dimensions of the sun as seen from Mercury and from the Earth are as the circles $E_1E_2E_3E_4$ and $m_1m_2m_3m_4$ in fig. 300.

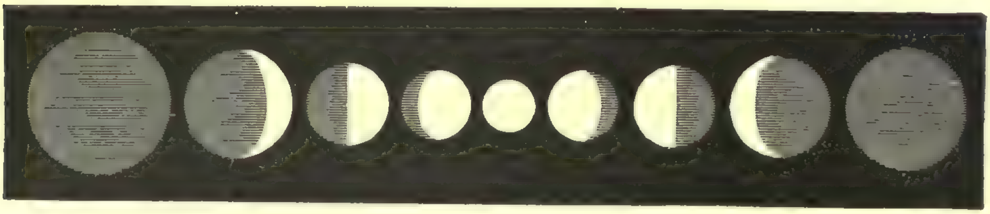


FIG. 301.—Varying phases of the planet Mercury.

(1029.) The apparent dimensions of the sun as seen from Mercury at his least and greatest distances may be conveniently compared as follows, a method available in all similar cases :—

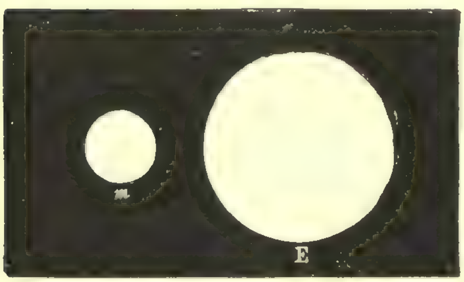


FIG. 302.—The globes of Mercury, m, and the Earth, E, on the same scale.

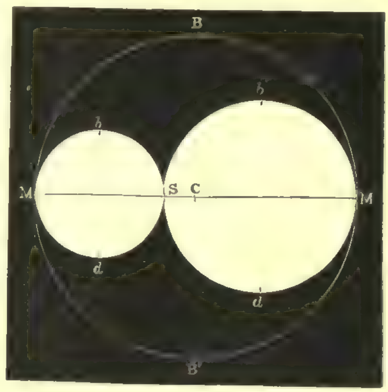


FIG. 303.—Disc of the Sun as seen from Mercury in perihelion and in aphelion.

Let M B M′ B′ (fig. 303) be the planet's orbit round the sun at S ; C its centre ; M, M′, the perihelion and aphelion respectively. Then the circles M′ b' S d' and M b S d represent the relative apparent discs of the sun as seen from Mercury at perihelion and at aphelion respectively.

(1030.) The telescopic study of Mercury has never been rewarded by any

bounding circle or terminator. In that case it would be no very difficult problem to determine the relative apparent lustre of Mercury in his different phases, though it would be by no means the simple problem commonly supposed. But as the surface of Mercury has doubtless a number of irregularities akin to mountains, hills, valleys, and ravines on the Earth, to say nothing of such varieties as distinguish the light-reflecting qualities of different soils, as well as of land and water, it would be idle to enter on calculations whose very exactness, considered mathematically, would ensure an inexact result.

[1] This method of comparing the sun as seen from two planets, P_1 and P_2, by noting that the sun's discs are as the areas of the orbits (appreciably circular) swept out by the planets P_2 and P_1, respectively, is worth noticing. It is only available, of course, as between two planets ; but since we only take interest in comparing the disc of the sun as seen from any planet with the sun's disc as we see it, we have in pictures of orbits such as are given in Plates IX. and X. all that we need for recognising how much larger or smaller the sun looks as seen from any other planet than as seen from the Earth.

interesting results, from the time when the telescope first revealed the planet's phases. It is noteworthy that with Mercury, as with Venus, the inferior telescopes of former times have seemed to show more than modern observers with the finest instruments can recognise. The significance of this can hardly be doubted. We seem obliged to regard most of the apparent peculiarities of appearance presented by Mercury under the scrutiny of Schröter and some of his contemporaries as due in large part if not wholly to imperfect definition. Schröter thought he had recognised an occasional blunting of the southern horn of Mercury's crescent when the planet was observed in the inferior or nearer portion of his orbit. Regarding this as due to the shadow of a mountain (11 miles high), and noting the intervals between the recurrence of this peculiarity, he assigned to the planet the rotation-period (24h. 5m.) and inclination (20°) already mentioned. His assistant, Harding, recognised the same peculiarities.[1] But Sir William Herschel, studying the planet with superior telescopic power, saw nothing which confirmed Schröter's observations.

(1031.) The following peculiarities have been noticed from time to time on the disc of Mercury under careful telescopic scrutiny, and may be regarded as indicating the probable existence of permanent features, varying in aspect as the planet rotates on his axis and passes through different phases :—

The breadth of the illuminated part of Mercury sometimes appears less than theoretically it should be. Mädler attempted to explain this by the assumption of a dense atmosphere reducing the lustre near the *terminator* (the boundary between the light and dark parts of the disc) ; but the effect of such an atmosphere would be the reverse of that thus attributed to it. The only valid explanation appears to be the existence of broad smooth tracts of dark soil, akin to what we recognise in the lunar seas (so called). The terminator of the crescent, half full, and gibbous moon is almost lost (so softly is it shaded off) across some of these tracts ; and this, combined with the inferiority of the light there, makes the terminator appear to recede, narrowing the moon's apparent breadth.[2] This, which appears simply like an irregularity in the moon's case, because the dark tracts are relatively limited in extent, would narrow the illuminated part in the way observed in Mercury's case were the dark areas of greater extent, and the orb observed with inferior telescopic power (corresponding with the effects of Mercury's greater distance).

(1032.) The occasional blunting of the planet's horns noted by Schröter has been observed, or at least suspected, by modern observers, as by Captain Noble and Messrs. Burton and Franks. This may be taken as proving that there are irregularities on

[1] Bessel deduced a rotation-period of 24h. 0m. 53s. and an inclination of 20° from Schröter's and Harding's observations. (The inclination understood is that of the equator of Mercury to the plane of the planet's orbit.)

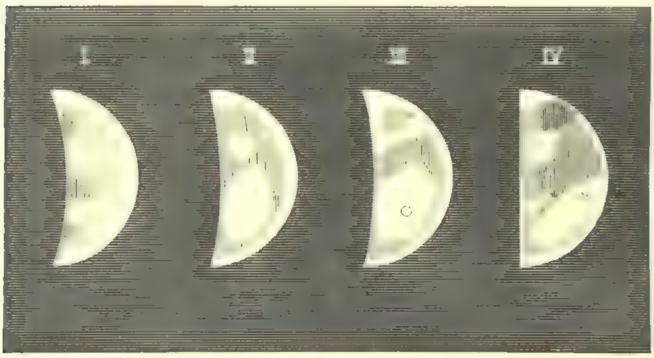
[2] The effect referred to is singularly conspicuous when the terminator falls athwart the sea of showers in the moon's third quarter. The narrowing is sometimes so conspicuous when the moon is observed in early twilight, that it has been mistaken (and by observers of considerable experience) for the shadow of some body between the moon and the sun.

the planet's surface. Such irregularities on the surface of the moon cause her cusps to be modified in shape in a similar way.

(1033.) Some observers have noticed a gradual fading off of Mercury's light towards the 'terminator,' while others claim that the terminator is sharply defined. If we consider how the moon would look if set so far off as to appear no larger than Mercury, we find an interpretation of Mercury's varying aspect in this respect, since the moon's terminator would fade off gradually and look ill-defined when it crossed the more level tracts, and be well-defined when it crossed the crater-covered and otherwise uneven parts of the moon's surface.

(1034.) The four drawings, figs. 304, 305, 306, and 307, by Mr. Denning, of Bristol, give a good idea of the appearance of Mercury under considerable telescopic power. They were made with a Browning reflector, ten inches in diameter, and a power of 212. Mr. Denning remarks that the planet was observed under great disadvantages on November 6 and 7 (figs. 304 and 305), much better on November 9 (fig. 306), when the different details were seen with a delightful neatness of definition, and at moments very neatly on November 10 (fig. 307). The features shown in these pictures are worth studying: some of them seem clearly to belong to the planet, but probably changes in their appearance are due rather to the difficult conditions under which Mercury was observed, and to changes in our atmosphere, than to changes in the condition of the planet's surface.

Fig. 304. Fig. 305. Fig. 306. Fig. 307.



The planet Mercury in November 1882 (*Denning*).
(From 'L'Astronomie.')

I. November 6, from 6.20 to 6.50 A.M.
II. November 7, from 6.25 to 6.55 A.M.
III. November 9, from 6.35 to 7.30 A.M.
IV. November 10, from 6.40 to 7.39 A.M.

(1035.) The most suggestive and instructive circumstance certainly known respecting Mercury is that his surface has small light-reflecting power, or, in other words, that its substance is dark. Mercury looks very brilliant when favourably seen, insomuch that the Greeks called him ὁ Στίλβων, the Sparkler; and though some have recognised a slightly rosy tint in his telescopic disc, his light is by most observers regarded as white: yet there are sufficient reasons for believing that the average tint of his surface is no nearer to whiteness than that of our darker sandstones. I have shown in my 'Other Worlds than Ours' that when seen under favourable conditions Mercury ought to appear brighter than Jupiter in opposition, whereas, when Jupiter is seen as a morning or evening star (and therefore much less bright than in opposition), beside Mercury, he far outshines his small fellow-planet. Mercury has been seen, however, under conditions which render the darkness of his surface unmistakable. On September 28, 1878, Nasmyth saw Venus and Mercury in the same telescopic field of view, and found the surface of Venus much brighter than that of Mercury. 'Venus looked like clean silver,' he said, 'Mercury more like lead or zinc.' Now at this time the Earth, Venus, and Mercury were situated as shown at *e*, *v*, and *m* respectively, V

being the perihelion of Venus, M that of Mercury, E_1 the place of the Earth at the autumn equinox. Both were near perihelion; Venus had a disc about three-quarters full, Mercury's disc was about half full. The distance S *m* was about $\frac{3}{7}$ths of the distance S *v*,[1] so that if the planets' surfaces turned Earthwards had been of equal average whiteness or general light-reflecting power, the surface of Mercury should have looked about $5\frac{1}{3}$ times as bright as that of Venus. We may fairly assume from Mr. Nasmyth's account that, as a matter of fact, the surface of Venus appeared at least twice as bright as that of Mercury. But let us take only what is absolutely certain—that Venus looked brighter than Mercury, instead of Mercury looking $5\frac{1}{3}$ times as bright as Venus, comparing the apparent intrinsic lustre of the surfaces. Certainly, then, the 'whiteness'[2] of Venus exceeds not less than six times the whiteness of Mercury.

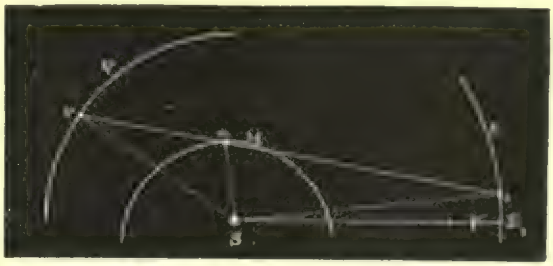


Fig. 308.—Situation of Mercury and Venus when seen by Mr. Nasmyth in the same field of view, September 28, 1878.

(1036.) Now whether we suppose Venus wholly or in great part cloud-covered, the most natural and probable interpretation of her relative whiteness (since clouds reflect about 66 hundredths of the light incident upon them), or imagine that Venus is in the state in which, probably, are the planets Jupiter and Saturn, and therefore partly self-luminous, we cannot assign to her a greater whiteness than 660, where 1,000 represents absolute whiteness and 780 the whiteness of driven snow.[3] Thus we cannot assign to Mercury a greater whiteness than 110, which is very much less than the average whiteness of the moon (173·6) and almost exactly equal to that of quartz porphyry. If we suppose Nasmyth to have compared the average brightness of Mercury with the average brightness of Venus, we may, perhaps, slightly reduce the

[1] The actual proportion is 10,000 to 23,108.

[2] By 'whiteness,' here and in similar passages, I refer to what Zöllner calls the *albedo*, meaning the proportion which the apparent illumination of a surface shining by irregular reflection bears to the light which has fallen upon that surface.

[3] The following table presents the results of Zöllner's measurement of the average reflecting power or degree of whiteness (the *albedo*, he called it) of the orbs mentioned—perfect whiteness being called 1:—

	Whiteness
The Moon	0·1736
Mars	0·2672
Jupiter	0·6238
Saturn	0·4981
Uranus	0·6400
Neptune	0·4648

The estimates of the whiteness of Mercury and Venus are unsatisfactory, having always to be made under unfavourable conditions. Probably we shall not be far wrong in adding to the above list.

	Whiteness
Mercury . . .	between 0·08 and 0·16
Venus . . .	,, 0·50 ,, 0·68

For comparison, the following table should be carefully studied:

	Whiteness
Snow	0·783
White paper	0·700
White sandstone	0·237
Clay marl	0·156
Quartz porphyry	0·108
Moist soil	0·079
Dark grey syenite	0·078

It will be understood that diffused reflection is here in question. The following list for regular reflection may be appended, though we have not to deal with regular reflection in the case of any planet:—

Mercury	reflects	0·648	of directly incident light.	
Spec. metal	,,	0·535	,,	,,
Glass	,,	0·040	,,	,,
Obsidian	,,	0·032	,,	,,
Wate	,,	0·021	,,	

relative superiority of Venus; for Venus (as we can see from fig. 308) was nearly full,[1] while Mercury was barely half full. But assuming such irregularities on both planets or on Mercury alone as exist on the moon, the reduction on this account would be but slight. In reality we are entitled to assume a much greater increase of Venus's superior whiteness; for Venus's lustre, as observed by Nasmyth, was much greater than we have supposed in the above reasoning. In any case we cannot obtain of Mercury an average *albedo* (see note on preceding page) greater than 150—about that of moist clay marl.

(1037.) To understand the significance of this, we should be able to form an opinion as to the probable existence of oceans on Mercury, and as to the probable extent of any atmosphere there may be round the planet. It will be seen hereafter that we have sound reasons for supposing the smaller planets to have shorter stages of planet life than the larger, so that (*cæteris paribus*) the smaller planets would be older in development, even though perhaps younger in years, than the larger. Mercury, according to this view, would be older than any primary planet in the solar system, and second only to our moon in age among all the orbs we are able to study with the telescope. As our moon has long since passed the stage when a planet ceases to have waters outside its solid surface, and when the atmosphere has either wholly disappeared or become very rare, we may suppose that Mercury has reached a kindred stage, and is a dead world, without seas and with a rare and tenuous atmosphere.

(1038.) This would not explain the fact that Mercury is darker than the moon, probably much darker. If, however, we inquire what parts of the moon are the darkest, and consider what would probably be the relative extent of such darker tracts on the surface of Mercury, we may perhaps find at least a probable explanation (more we can scarcely expect) of the greater average darkness of Mercury's surface. It will be seen later that there are strong if not demonstrative reasons for supposing that the darker tracts on the moon are the floors of ancient lunar seas. The moon would look darker (as a whole) if these floors were of relatively greater extent. Now there are reasons for supposing that in the case of Mercury the floors of seas now dried, and therefore the seas themselves in the times when they existed, were of greater relative extent than on the moon. The various substances, elementary or otherwise, forming the material of different planets are probably proportioned in about the same way in all planets.[2] Assuming this to be the case as between Mercury and the moon (the comparison with the Earth will be made later), we see that since the mass of Mercury is to the moon's (see

[1] The breadth of the illuminated part of Venus bore to a full diameter the ratio of (2 − vers. $S v E$) to 2, and the vers. $S v E$ was less than $\frac{1}{10}$, so that Venus was more than $\frac{19}{20}$ full.

[2] The idea that the lighter elements or materials are present in greater proportion in the outer planets, and the heavier in the inner, has long since been rejected by all astronomers capable of rendering reasons in physical matters.

Art. 722) as 6,600 to 1,238—say 533 to 100—the water on Mercury probably exceeded in about this degree the water on the moon. But the surface of Mercury exceeds the surface of the moon only as the square of 3,010 exceeds the square of 2,160, or about as 194 exceeds 100. Hence the quantity of water per square mile of the surface of Mercury, probably exceeded (at any corresponding stage in the history of both planets) the quantity of water per square mile of the surface of the moon as 533 exceeds 194, or about as $2\frac{3}{4}$ exceeds 1. Hence, though we may place very little reliance on these numbers regarded as exactly expressing the ratio in question, we may assume with some confidence that the oceans on Mercury occupied a considerably larger extent of the planet's surface than those on the moon. If they were twice as extensive, or even only half as extensive again, and in the old age of the planet, when they had been withdrawn (soaked up as it were) within the planet's interior, this surface of their floors had the same relative darkness that we recognise in the tracts which have the characteristics of sea-floors on the moon, the relative average darkness of Mercury's disc would be fairly explained. And as this explanation involves no considerations which do not accord in quality and degree with the conclusions to which theories otherwise suggested have led, while we have to take into account no physical processes or conditions with which the study of our own Earth (as will be seen later) and of other planets has not made us more or less familiar, this interpretation of the appearances presented by the planet Mercury may be regarded as that which accords best with the evidence: it seems indeed that which the totality of the evidence will alone permit us to accept.

(1039.) It seems certain that Mercury does not at present possess both air and oceans; for, if he did, great quantities of vapour would be raised from the waters and condensed over wide tracts into clouds. These clouds would reflect a large proportion of the light falling on them; and thus the average whiteness of the planet would be raised much above that of the moon, instead of being far below. The whiteness of Mercury would be probably at least equal to that of Mars (see note to Art. 1036) if, like Mars, he had an atmosphere and seas.

(1040.) Observations of Mercury as an evening star present no features which can be referred to an atmosphere round the planet, all the peculiarities which have been so interpreted being explained more satisfactorily as due to the way in which the planet is always observed through parts of our own atmosphere illuminated by the sun's light and disturbed by the sun's heat.

(1041.) When Mercury is in transit observers have suspected the existence of an atmosphere. A luminous aureola has been seen round the planet

on such occasions, which has been interpreted as due to an aërial envelope. Mr. Huggins describes the appearance as he saw it during the transit of November 5, 1868, in the following terms :—

The planet appeared as a well-defined round black spot, surrounded by an aureola of light a little brighter than the sun's disc, and having a breadth equal to 'about one-third of the planet's diameter. The aureola did not fade off at the outer margin, but remained of about the same brightness throughout, with a defined boundary. The aureola was not sensibly coloured, and was only to be distinguished from the solar surface by a very small increase of brilliancy.' Others have recognised only a dusky border to the black disc of Mercury, the tint merging gradually into that of the solar disc, not attaining a greater brightness.

(1042.) We must not attempt to get rid of the atmospheric explanation as some have done by the consideration that rays passing through the atmosphere of a planet between the Earth and the sun would be deflected all round the planet so as to cross between the planet and the Earth and not reach the terrestrial observer.[1] This doubtless is the case with rays from the part of the sun immediately behind the planet as seen in transit. But the only inference from this is that, if a planet in transit is surrounded by an atmosphere of appreciable refractive power, the sun-light which seems to come from a part of the sun lying behind a point of the planet's edge does not really come from that part, but from a part measurably remote from it. Suppose, for instance, we are looking at a planet m, fig. 309, in transit, and not far from the edge S S′ of the sun's disc. Suppose p and q to be two points on the edge of the planet's disc as thus seen. Then if the planet has no atmosphere we know that the light seen at p on the planet's edge comes from the point p on the sun. But if the planet has a refracting atmosphere the light from p on the sun could not be seen at p on the planet's edge, being deflected downwards there, and not reaching the observer's eye. But the rays from some point such as P on the sun (on $p\,m$ produced) will be so much refracted in passing over the planet's surface at p as to be just deflected to the eye of the observer on Earth.[2] From some point such as Q on the sun, light passing round the part q of the planet's surface will reach the eye of the observer on Earth. Where he seems to be looking at the arc $p\,q$ of the sun's

[1] It is singular how many even among those acquainted with optical laws have fallen into this particular error. The above statement of the difficulty is quoted from Mr. Huggins's account of the transit of Mercury in 1868. Mr. Russell, the Government Astronomer of Sydney, N.S.W., falls into the same error, and it is repeated by Professor Newcomb (in relation to Venus transits) in his *Popular Astronomy*, 2nd edition, p. 303.

[2] This is, in fact, only another way of saying that the line of sight directed to p, passing through the refracting atmosphere of the planet, will be bent downwards above the part p of the planet's surface to some point such as P, on the line $p\,m\,$P, so that the observer, who seems to see at p the point p of the sun's surface, is really looking at the point P. It is generally convenient thus to reverse the direction of light-rays reaching the eye in problems of this sort; for we know that we are on the right track in considering the course of a beam of light which actually reaches the eye, if we trace its course *from* the eye; and we can thus determine where it came from: whereas, if we consider the course of rays of light starting from some luminous source and undergoing various reflections or refractions, we have to determine on what course any particular rays must set out to reach the eye. Thus considering the planet m, we know that the light from the point p on the planet comes to the eye: we have then the known part of the light's course (the line joining the eye and the point p) and can at once determine the course along which the light must have travelled to p; in order that, passing thence after atmospheric refraction, it may reach the eye of the observer.

surface (as well as of the planet's disc), he is really looking at the arc P Q of the sun's surface.[1]

[1] If the whole of the circle with centre *m* and radius *m* P (determined by the greatest refractive action of the planet's atmosphere) lies on the sun's disc, the real outline of the planet's disc will be seen, slightly magnified by the refractive action; but if a part only of this arc lies on the sun's disc, those parts only of the real outline of the planet's disc which correspond with this arc (as *p q* corresponds with P Q) will be actually seen; the rest of the apparent outline of the planet's black disc will really belong to the lower parts of the planet's atmosphere, the part of the atmosphere forming the apparent edge of the disc being higher and higher above the real surface of the planet, according as the distance of the sun's edge measured across the centre of the planet's disc is least.

For instance, if the circle around *m* as centre with radius *m* P cuts the sun's edge in A and B, then the arc *a c b*, between the points where A *m*,

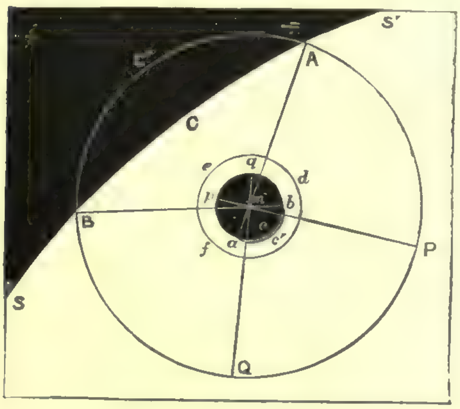


Fig. 309.—Illustrating the action of the Atmosphere of a Planet in transit.

B *m* produced meet the outline of the disc, does not indicate the true outline of the planet (magnified as above mentioned), but the height of the atmosphere at which rays from various parts of the edge A C B are so refracted as to reach the eye of the terrestrial observer. The arc *a c′ b* indicates the position of the curve in the atmosphere of the planet at which the apparent boundary falls; but it must be understood that the space within the arc *a c′ b* looks as black as the rest of the disc. [The terrestrial observer may be said to be looking at the black region A C B C′ through the atmospheric region *a c b c′*.]

Many seem to find singular difficulty in understanding the effects of a refractive envelope round a planet in transit. The difficulty I have already considered is by no means the only one which has perplexed even persons acquainted with optical laws. It has been argued that if the light received through a ring of atmosphere such as *a p b* were really received from such an area as a circle with radius *m* P (in Venus's case a much larger area—indeed, from the estimated refractive power of Venus's atmosphere, it would follow that if Venus were on the centre of the sun's disc the whole surface of the sun would send light through Venus's atmosphere to the observer's eye), this atmospheric ring ought to look very much brighter than the rest of the sun's disc. This difficulty, like the other, and like most others of this class, can be removed by considering the deflection of lines of sight as well as the deflection of rays of light. Suppose G, fig. 310, an opaque globe of any sort (planet or ball), surrounded by a refracting medium *a b c′*, either of uniform density (greater than that of the surrounding medium), or, like an atmosphere, growing rarer with distance from the globe's surface. Let E be the eye of an observer, A B part of a large luminous surface seen beyond G. Then, looking towards the part *a c* of the refracting medium, the eye at E receives light from the whole breadth A B of the luminous surface, determined by the consideration that a ray from A to E just touches the circle *a a′* at *a*, while a ray from B is bent round the globe G (along the course B *b c b* E), just touching it at *c*, to reach the eye as shown. [If the medium is of uniform density the path of the ray inside it will be straight, and in the figure would be a chord of the circle *a a′b′*.] Similar remarks apply to the light received by the eye at E looking towards *c′b′*. At a first view, then, it seems as though the eye at E received a concentrated supply of light from the parts *a c* and *c′b′* of the refracting envelope. Yet the exact reverse of this seems as clearly to follow, if we consider that rays from the point P (of the luminous surface) to *a* pass on to A′, while those from P which, bending through the refractive medium, pass along the course *q c q*, touching the globe's edge at *c*, go thence to B′, so that they are widely dispersed, and the same for rays from P through *c′b′*; so that the eye at E receives but a portion of the rays from P, and one would say the part P of the luminous surface would look much fainter as seen through *a c* and *c′b′* than it actually is. In reality, there is neither a concentration nor a dilution of the light, or rather there is for every part of the surface A B, as seen through every part of the refracting ring *a a′c′*, exactly as much dilution by wide-spreading as there is concentration by the increase of surface brought into view. The part A B is reduced in apparent span from the angle it really subtends at E to the small angles *a* E *b*,

(1043.) It is clear that if the part P Q of the sun is brighter than the part of the sun on which the arc appears to be projected, the part $p\,q$ of the ring $p\,a\,b$ will appear brighter than the solar background. But it is equally clear that other parts will appear darker than that background. So that a complete aureola of brighter light round the disc of a planet cannot be explained by assuming that the planet has an atmosphere. This applies to Venus, which probably has a dense atmosphere, as well as to Mercury, which probably has only a rare atmosphere, if he has any atmosphere at all.

(1044.) Most probably the ring seen round Mercury in transit is an optical phenomenon due to several causes, and variable not only according to the telescope employed and the conditions of atmosphere and illumination, but with the quality of the observer's eyesight. In the first place we must consider the effects of diffraction, which would tend to produce a fine ring of light (not an aureola) round the planet's disc. This ring would be nearer to the disc than the boundary of the aureola as seen by most observers. But then irradiation has to be considered, which would throw the apparent outline of the disc inwards from the bright ring, leaving the latter unchanged in position. An atmosphere around the planet would affect the appearance of the ring-encircled aureola thus presented, making it appear more uniform in tint. There should be a difference between the apparent luminosity of the aureola in different parts of its extent if an atmosphere had any important part in producing the phenomenon. But the great extent of the aureola, which, in the case of Mercury, has appeared from one-third to one-half of the planet's diameter in breadth (that is, from 1,000 to 1,500 miles in height, and much broader than the ring $d\,e\,f$ in fig. 309), precludes our supposing this, and leaves us free to accept the conclusion that the phenomenon is in the main optical, though it may be slightly modified by the refractive action of an atmosphere. In the case of Venus in transit, as will be seen farther on, we are bound to take into account the action of an atmosphere; but there

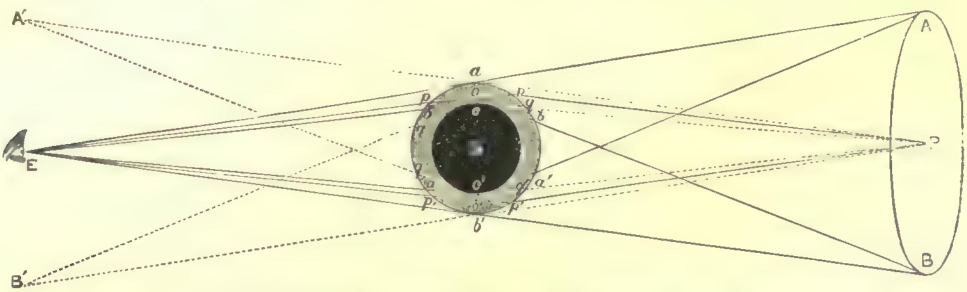


FIG. 310.—Illustrating the Refraction of Light by a spherical Shell surrounding an opaque Globe.

a E b', but the eye at E receives a proportion of the light which is less than the whole light passing from A B through $a\,c$ and $c'b'$ in exactly the same degree. The refracting ring will therefore neither look brighter nor darker than the surface A B, except in so far as absorption may act, and diminishing the number of rays passing through $a\,c$ and $b'c'$, may make the ring appear somewhat less brilliant than the bright background (the luminous surface A B) on which both the globe and its refracting envelope appear to be projected.

[Of course the surface A B has here been understood to be of uniform brightness; if parts are brighter than the rest, parts of the ring $a\,a'c'$ will appear brighter than the rest, and may appear brighter than the background on which they are projected. Suppose, for instance, the region around B is brighter than the region around A; then clearly the part of the ring near b, through which the luminous region round B is seen by the eye at E, will appear brighter than the luminous background outside a, which belongs to a less luminous region.]

also the apparent extent of the aureola, corresponding to a height of about 500 miles, shows that the atmosphere can only modify the aureola, and cannot be the chief cause of the phenomenon.

(1045.) It should be noticed that if Mercury has any atmosphere at all, it would not be shallow, though it must be exceedingly rare. The force of gravity on the planet's surface is but about $\frac{5}{11}$ that of gravity on the surface of the Earth. Hence instead of the atmospheric pressure halving for each $3\frac{1}{2}$ miles of ascent from the planet's surface as in the Earth's case, an ascent of about $7\frac{3}{4}$ miles would be required to halve the pressure, and therefore the density. Now at a height of about 17 miles above the Earth's surface the atmosphere would be reduced to about $\frac{1}{30}$ of its density at the sea-level, and the mercurial barometer would show a height of only one inch. If there were an atmosphere on Mercury as rare as this at the planet's surface, the planet would yet be clothed with one corresponding in density with all that part of the terrestrial atmosphere which lies outside a spherical layer 17 miles high. And as the atmosphere of Mercury would halve in pressure for $7\frac{3}{4}$ miles of ascent, the density at a height of 31 miles would be $\frac{1}{16}$ that at the mean surface, while at a height of 14 miles above the 17 just mentioned, or 31 miles above our sea-level, the atmospheric density is also $\frac{1}{16}$ that at the sea-level. Hence with the assumed small density at Mercury's mean level, the density of the Mercurial air at a height of 31 miles would be the same as the density of our air at the same height above the sea-level. At greater heights the Mercurial air would be denser than ours.[1]

(1046.) Some observers have seen a small white or greyish spot on the disc of Mercury in transit, the spot being sometimes central, sometimes not, sometimes round, at others irregular (to my eyes it appeared decidedly triangular during the transit of May 6, 1878); while some have recognised movement in the spot as if carried round by the planet's rotation. Some observers have seen two spots, and this at the same moment when one spot only was seen by others, and no spot at all by yet others. There can be no doubt whatever that the phenomenon is an optical product, though it may be difficult to say in any given case whether it is due to telescopic faults, to faulty vision, or to optical illusion.

(1047.) The conditions under which transits of Mercury occur are akin to those already considered in the case of Venus (Arts. 591 to 639). To deal with them thoroughly would require more space than can here be spared. But the general conditions of the problem may be mentioned. Mercury and the Earth coming into conjunction (Art. 1025) at intervals of 115·877 days, Mercury comes three times into inferior conjunction with the sun (that is, passes between the sun and the Earth) in 347·631 days, or about 17·62 days less than a year. Hence after a conjunction of Mercury

[1] Of course these calculations are in no sense exact. It would be idle to base exact calculations on such mere assumptions as we have to make about the Mercurial atmosphere. It is important, however, to notice the general principle involved in the above reasoning, and the general inference that the smaller planets, though they probably have rarer air at their surface than the larger, and less air above each square mile of surface, have nevertheless atmospheres which extend farther, comparing layers having densities much smaller than the surface densities. Or this inference may be expressed thus. If two planets are unequal in mass, the atmospheric density at the mean level is probably greater for the larger than the smaller; but there is a certain height above the mean level at which the two planets have equal atmospheric density, while for any height greater than that, the atmosphere of the smaller planet has the greater density.

and the Earth as at m_1 E_1, fig. 311, two more occur along conjunction-lines situated as m_2 E_2, m_3 E_3,[1] and then a fourth as at m_4 E_4, about $17\frac{5}{8}$ days before the Earth reaches the position E_1 again. It follows, since 17·62 is contained 20·73 times in 365·26, that line passes back from m_1 E_1 by 17·62 days' motion each year; this (third) conjunction-line loses one complete circuit in 20·73 circuits, or (since losing a circuit means the loss of one year's motion) in 19·73 years. But there are three conjunction-lines, m_1 E_1, m_2 E_2, and m_3 E_3, to be considered. Hence the average time in which any given direction from S is crossed by a conjunction-line of Mercury and the Earth is one-third of 19·73 years, or 6·577 years.

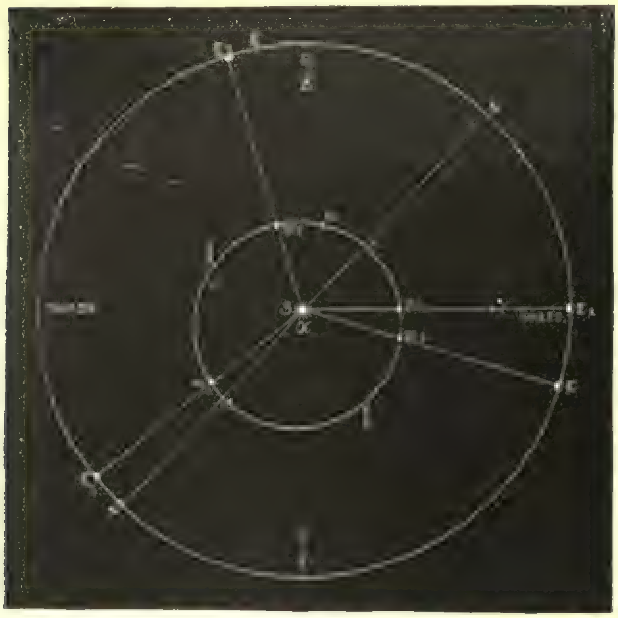


Fig. 311.—Illustrating the recurrence of Mercury's transits.

(1048.) Now the plane of the orbit of Mercury, inclined almost exactly 7° to the plane of the Earth's orbit, cuts this plane along the line *n n′*, the part *n m n′* being north and the part *n′ m′ n* being south of the plane of the Earth's orbit. Seen from any point in the Earth's orbit, the orbit of Mercury appears as an ellipse, always of great eccentricity, with the sun occupying a more or less eccentric position; only when the Earth is at or near N or N′ does the orbit of Mercury appear like a line or nearly enough like a line to intersect the sun's disc as seen from the same point. If Mercury passes that part of his orbit which is between the sun and the Earth, when the Earth is near enough to N or N′ for Mercury's orbit thus to intersect the sun's disc,[2] a transit occurs. A conjunction-line travelling round in the way considered above will not at each return to the neighbourhood of *n* N or *n′* N′ come to the same position (the interval not being an exact number of years); moreover it will be obvious on a slight consideration of the conditions that conjunction-lines will be set nearer together towards *n* N than towards *n′* N′ (the more rapid motion of Mercury near perihelion at *m* causing any difference of direction of Mercury and the Earth from the sun to be attained or corrected (as the case may be) in a shorter time—that is to say, while the Earth is traversing a shorter arc). Thus November transits occur more frequently than May transits. In about four out of seven passages of a conjunction-line past *n* N a transit occurs—that is, there are about four November transits of Mercury

[1] In fig. 311 the conjunction-lines are presented as for the true motions of the Earth and Mercury, though in the text average motions only are considered.

[2] I have dealt with the problem of the transits of Mercury in this particular way partly for the sake of variety, having already dealt with transits of Venus in another way, partly because the student will find many interesting geometrical considerations involved in this method. For further details of the general problem, I would refer the student to my *Studies of Venus Transits*.

in 46 years (more exactly, in 45·962 years). These usually occur at intervals of 13, 13, 13, 7, 13, 13, 13, 7 years, with occasionally an interval of 6 years replacing the interval of 7. May transits, on the other hand, never occur at intervals of 6 or 7 years; an interval of 33 years (13 + 13 + 7), or occasionally 32 years, (13 + 13 + 6), is about as common as an interval of 13 years, and occasionally there is an interval of no fewer than 46 years between two May transits.

(1049.) These relations are indicated in the following table :—[1]

NOVEMBER TRANSITS OF MERCURY.

Dates	Interval in years	Dates	Interval in years
November 1, 1605		November 12, 1815	13
„ 4, 1618	13	„ 5, 1822	7
„ 7, 1631	13	„ 7, 1835	13
„ 9, 1644	13	„ 9, 1848	13
„ 2, 1651	7	„ 12, 1861	13
„ 4, 1664	13	„ 5, 1868	7
„ 7, 1677	13	„ 8, 1881	13
„ 10, 1690	13	„ 10, 1894	13
„ 3, 1697	7	„ 12, 1907	13
„ 6, 1710	13	„ 6, 1914	7
„ 9, 1723	13	„ 8, 1927	13
„ 11, 1736	13	„ 12, 1940	13
„ 5, 1743	7	„ 13, 1953	13
„ 6, 1756	13	„ 6, 1960	7
„ 9, 1769	13	„ 9, 1973	13
„ 2, 1776	7	„ 12, 1986	13
„ 12, 1782	6	„ 14, 1999	13
„ 5, 1789	7	„ 8, 2006	7
„ 9, 1802	13	„ 10, 2019	13

MAY TRANSITS OF MERCURY.

Dates	Interval in years	Dates	Interval in years
May 3, 1615	—	May 8, 1845	13
„ 5, 1628	13	„ 6, 1878	33
„ 3, 1661	33	„ 9, 1891	13
„ 6, 1707	46	„ 7, 1924	33
„ 2, 1740	33	„ 10, 1937	13
„ 6, 1753	13	„ 9, 1970	33
„ 4, 1786	33	„ 7, 2003	33
„ 7, 1799	13	„ 10, 2016	13
„ 5, 1832	33		

It will be noticed that there are 55 transits recorded here; and taking the intervals from transit to transit (instead of from November transit to November transit, and from May transit to May transit) there are 54 intervals between November 1, 1605, and November 10, 2019, or 414 years 9 days, say 414·025 years. Thus the average interval between transits of Mercury is 7·667 years—or almost exactly $7\frac{2}{3}$ years.

[1] It will be noticed that when thirteen years bring about a transit, either in May or November, the date falls two or three days later; but when, in the case of November transits, seven years intervene, the date goes back five or six days, while when six years intervene, the date goes ten days forwards; for thirty-three years (May transits) the date goes two or three days back; for forty-six years the date goes three or four days forwards. Of course the irregularities which seem to affect these changes are due simply to the fact that in years differently related to the intercalated day of leap-year the same almanac dates correspond to different actual positions of the Earth in her orbit.

(1050.) For convenience, I add in their true sequence the dates of transits of Mercury during the next 130 years:—

THE NEXT EIGHTEEN TRANSITS OF MERCURY.

1891	May 9	1937	May 10	1986	Nov. 12
1894	Nov. 10	1940	Nov. 12	1999	„ 14
1907	„ 12	1953	„ 13	2003	May 7
1914	„ 6	1960	„ 6	2006	Nov. 8
1924	May 7	1970	May 9	2016	May 10
1927	Nov. 8	1973	Nov. 9	2019	Nov. 10

(1051.) VENUS, the second planet in order of distance from the sun, moves in an orbit having a mean distance of 67,110,000 miles (or about $\frac{723}{1000}$ of the Earth's mean distance) in a period of 224·70 days, or rather less than $7\frac{1}{2}$ months of terrestrial time. Her path is the least eccentric of all the orbits of the primary planets, the eccentricity being less than ·00685, so that the greatest mean and least distances are as 100,685, 100,000, and 99,315 respectively, or roughly as 1,007, 1,000, and 993, or still more roughly as 141, 140, and 139. Hence the heat and light received from the sun, when Venus is at her greatest, mean, and least distance, are approximately as 142, 140, and 138, or as 71, 70, and 69. At her mean distance Venus receives more light and heat from the sun than the Earth (comparing square mile for square mile squarely illuminated by the sun) in the same degree that the square of 1,000 exceeds the square of 723, or as 191 exceeds 100. The diameter of Venus is about 7,707 miles, and there is no measurable polar compression of her globe. The surface of Venus is about 186,600,000 square miles, only some 11 millions of square miles less than the Earth's. The volume of the Earth exceeds the volume of Venus about as $108\frac{2}{5}$ exceeds 100, or as 100 exceeds 92. But the mean density of Venus is somewhat less than the Earth's; so that instead of Venus's mass being $\frac{92}{100}$ of the Earth's, it is rather less than $\frac{78}{100}$. Gravity at the planet's surface is about $\frac{82}{100}$ of terrestrial gravity (more exactly, $\frac{82297}{100000}$). We have no satisfactory determinations of either the inclination of Venus's axis to the plane of her orbit or of the planet's rotation-period. Some observations assign 75° as the planet's inclination (that is, the inclination of the plane of its equator to the plane of its orbit—in our Earth's case only 23° 27′ 15″), while De Vico made this angle 53°.[1]

[1] Attention has been called, but as I think unnecessarily, to the use of the expression 'inclination of axis,' to indicate the angle between the axis and a perpendicular to the plane of a planet's orbit. The idea of those who object to this expression seems to be that if a planet's axis is at right angles to the plane of its orbit the inclination should be called 90°. But I apprehend that most readers who should be told that a planet's axis is inclined 90° to the plane of its orbit would suppose that it was bowed from uprightness to coincidence with that plane; whereas, if told that it was not inclined at all to the plane of the orbit, they would suppose it upright: if this is what the general reader would understand, the usage objected to is justified. And surely it is much more natural to speak of the inclination of the axis to the plane of the orbit than of the angle between the planet's equator and its orbit-plane.

Rotation periods ranging from 23h. $2\frac{1}{2}$m. to 23h. 21m. (we may reject altogether Bianchini's rotation-period of over 24 days) have been assigned to Venus; and possibly the rotation-period may be regarded as fairly established. But it would be idle to discuss the probable nature of the seasons of Venus where results so widely discordant have been obtained for the planet's inclination.

(1052.) The motion of Venus may be dealt with as we dealt with the motion of Mercury in Art. 1025; or (slightly modifying the method) we may consider the Earth as at rest at E, fig. 312, and conceive Venus to move through all the relative positions shown at V_1 (*superior conjunction*) or V_2, V_3, V_4 (*maximum easterly elongation*), V_5 (*inferior conjunction*), V_6 (*maximum westerly elongation*), V_7, V_8, to V_1 again, in her synodical period of 583·921 days; noting that though these are the relative motions considering the Earth as at rest, the real motions are those which have been fully considered in Arts. 593 to 595. The aspects of the planet, as seen from these different positions, are shown at 1, 2, 3, 4, 5, 6, 7, 8, 1, fig. 313, corresponding to Venus's relative positions at V_1, V_2, V_3, V_4, V_5, V_6, V_7, V_8, and V_1, respectively. At V_1 Venus is invisible, being lost in the sun's light; from near V_1 over the arc V_2 V_3 V_4 to near V_5 Venus is an evening star, being on the east of the sun, and therefore setting after him; at V_5 Venus is again invisible, being lost in the sun's light; and from near V_5 over the arc V_6 V_7 V_8 to near V_1 Venus is a morning star, being on the west of the sun, and therefore rising before him. (In other words, Venus is an evening star from superior to inferior conjunction, a morning star from inferior to superior conjunction; but she is invisible

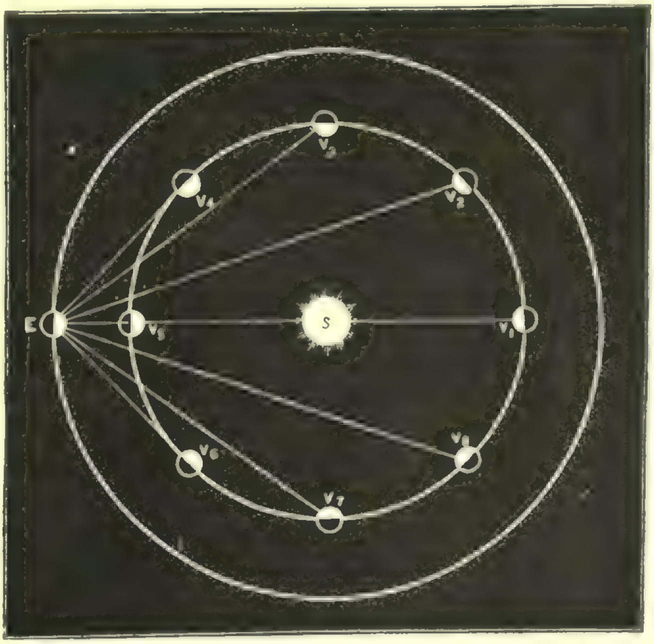


FIG. 312.—Motions of Venus with respect to the Earth supposed at rest.

The question may be compared to the following: If one were to speak of a tree as not inclined to the Earth's surface, would one mean that the tree was upright or horizontal? And again, if one spoke of a tree as slightly inclined, would one mean that it was slightly slanted from uprightness or very nearly prostrate? If a tree described as not inclined would be understood to be upright, and a tree described as slightly inclined would be understood to be nearly upright, the usage objected to is justified; and those who insist on regarding the inclination of a planet's axis to its orbit-plane as signifying the angle between the axis and the orbit-plane, and not the amount of inclination (from uprightness) towards that plane, ought carefully to explain that they are adopting a method of expression not consistent with ordinary usage.

except in the telescope for a certain distance on either side of both conjunctions, or for a certain time before and after each—just as the moon is invisible for a certain time before and after her conjunction with the sun, or the time of 'new moon.')

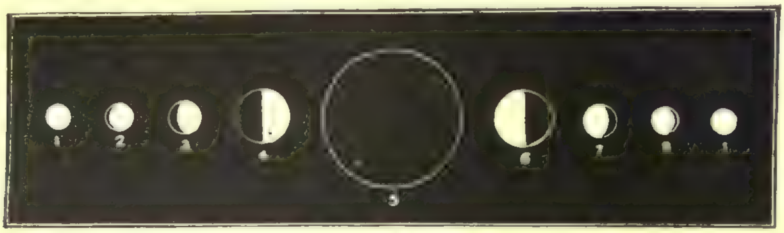


FIG. 313.—Corresponding phases of Venus.

(1053.) The recognition of the phases of Venus was among the earliest achievements of Galileo's telescope. This treatise would (I fear) be regarded as incomplete if I dared to omit the thousand-times-told tale of Galileo's announcement respecting the discovery that Venus has phases, which he concealed, following the fond custom of his age, under the anagram—

Hæc immatura a me jam frustra leguntur, o. y.

('These incomplete [observations][1] are as yet read by me in vain'), the anagram being developed in the fulness of time into the poetical line—

Cynthiæ figuras æmulatur Mater Amorum

('The Mother of the Loves imitates the aspects of Diana'—that is, Venus shows phases like the moon's).

The discovery was important in that day, as showing that the celestial bodies do not shine with their own glory (as had been supposed when men had hardly shaken off the superstitions of star-worship), each pouring its own special influences on the inhabitants of the Earth.

(1054.) Telescopic observations of the planet Venus have not been altogether satisfactory; yet it appears to me a mistake to reject the observations which have indicated irregularities of surface or varieties of brightness on her surface merely because they have not been confirmed when the planet has been studied with very powerful telescopes in the hands of official astronomers. The directors of Government observatories have usually been much less successful in studying planetary details than those zealous amateurs who take delight in the study of the heavenly orbs and are ready to wait and watch for favourable opportunities.

[1] Galileo published his anagram in September 1610, having then only observed a slight gibbosity in Venus. Any doubts he may have felt (as suggested in his anagram) must have been quickly removed, since Venus had already passed to her half-full stage on December 15 (new style), and was afterwards passing through her crescent (or rather decrescent) phases to inferior conjunction on February 24, 1611.

(1055.) There are reasons for supposing that the visibility of the markings on Venus depends more on the state of our air than either on the quality of the telescope employed or on changes taking place in Venus herself. The most striking evidence on this point is that given by Dominic Cassini, who at Bologna recognised one bright spot and several dusky ones clearly enough to determine from their movements the planet's rotation-period, which he estimated at 23 hours 21 min., while at Paris he could never see them; nor could J. J. Cassini or Maraldi. In Rome, Bianchini observed spots several times, from whose apparent motion he deduced a rotation-period of 24 days 8 hours. But J. J. Cassini showed that Bianchini's observations, made on successive evenings at nearly the same hour (with a telescope 22 yards long and $2\frac{1}{2}$ inches in diameter), and lasting but for a short time, because, owing to the small light-gathering power of his telescope, he could not observe Venus by daylight, could be interpreted as indicating 25 rotations instead of one only in 24 days 8 hours, making the rotation-period 23 hours $21\frac{1}{2}$ min., or about $23\frac{1}{3}$ hours; and with such a period his father's observations and Bianchini's can be satisfied; whereas if Bianchini's period of 24 days 8 hours is accepted, the elder Cassini's must be dismissed as worthless. In 1789 Schröter, following the same method which he had employed in Mercury's case, discovered with his reflector (7 feet long) a luminous projection on the dark hemisphere of Venus, which indicated the presence of a high mountain. From observations of the return of this projection, he deduced a rotation-period of 23 hours 21 min. 19 sec. In 1842 De Vico, assisted by Palomba and five other assistants, made many thousands of observations of Venus, most of them for the determination of the rotation-period. The instrument employed was a fine refracting telescope by Cauchoix. They were able to confirm the accuracy of Bianchini's observations in all respects, except that one spot which they recognised had not been seen by Bianchini. The glare of the planet on a dark sky sufficed to render the micrometer useless, and thus to explain sufficiently the mistake of Bianchini as to the planet's rotation. The rotation-period deduced by De Vico and his assistants was 23 hours 21 min. 22 sec.

(1056.) It does not seem reasonable to reject observations so definite as these, made under such favourable conditions, by different observers, none of whom would probably be prejudiced in favour of the results obtained by the others, on the strength of the failure of some observers who, with possibly superior telescopes,[1] have failed to see in skies less clear the delicate features recognised by Cassini, Bianchini, De Vico, and several assistants. The suggestion that the close agreement between the rotation-period obtained for Venus and the length of our terrestrial day is suspicious, because appearances due to optical illusion or telescopic imperfections might repeat themselves several days in succession at the same hour, is too feeble to be worth considering when the circumstances of the observations, especially of De Vico's, are taken into account. Vogel's observations of Venus, which have been cited as effectively disproving De Vico's, were made at Bothkamp, where the chance of success in detecting the delicate features of Venus would be less even than in England.

(1057.) The markings of Venus recognised by Bianchini, and confirmed by De Vico and his assistants, are shown in fig. 314, which is a copy (borrowed from Webb's

[1] Probably the delicate markings on Venus would be less favourably seen with very powerful telescopes than with weak ones, because while the absolute increase in the quantity of light collected by large telescopes is unfavourable in the case of so brilliant an object as Venus, the effect of the absorption exerted by the thick object-glass in reducing relative differences of illumination would scarcely be less unfavourable.

excellent little work, 'Celestial Objects for Common Telescopes') of Bianchini's rough map of Venus on a cylindrical projection. No useful purpose would, I think, be subserved by discussing here the various observations and drawings which have been made of Venus. All that can as yet be asserted with confidence is that, while the markings of the planet are evidently exceedingly delicate and difficult of detection, and probably variable, not only with the condition of our own atmosphere but with changes in the atmosphere of Venus, the features shown in Bianchini's drawing, confirmed as they have been by the many thousands of observations made by De Vico and his assistants, must be regarded as real.

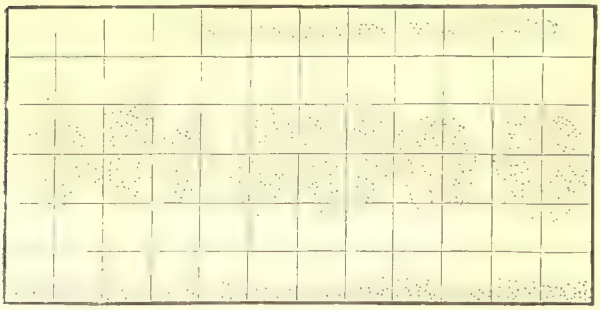


FIG. 314.—Bianchini's Map of Venus.

(1058.) Fig. 315 affords a good idea of the aspect of Venus when half full.

(1059.) The evidence that an atmosphere surrounds Venus may be regarded as decisive, though little reliance can be placed on the calculations which some astronomers have made in regard to the refracting power and density of that atmosphere. It is almost certain that the sun illuminates rather more than a hemisphere of Venus, or than the area (somewhat more than a hemisphere) which he would illuminate were there no atmosphere around the planet. The twilight zone, which should theoretically be recognised beyond the 'terminator,' has not actually been seen; but the extension of the crescent of Venus beyond a semicircle has been greater than could be attributed to the circumstance that the sun as seen from Venus is not a point, but has a disc larger than that of the sun as we see him. The diameter of the sun as seen from Venus is no less than $44\frac{1}{4}'$, and consequently a zone of more than $22'$ wide outside the sunward hemisphere of the planet is illuminated by direct sunlight. If the planet has an atmosphere equal in refractive power to the Earth's, a zone about $34'$ beyond the zone just mentioned is illuminated by actual sunlight. It would follow that if the planet were so situated that the line of sight from the Earth were inclined not more than $56'$ $(34' + 22')$ to a line from the sun through Venus's centre, a complete ring of light would be seen round Venus, though it would be everywhere exceedingly fine, and of course finest of all on the edge of Venus farthest from the sun. Moreover the *quantity* of light received where this arc was finest would be very small, because the *quality* of the light would only be that corresponding to the illumination and the *albedo*

FIG. 315.—Venus when half-full (*Folache*).

(see Art. 1036, note). An exceedingly fine arc, only illuminated in this way, would not probably be discernible at all from the Earth. At any rate—and this is the point to which I wish specially to direct attention—this fine arc of light round the part of Venus farthest from the sun would be so very much less conspicuous than the arc of real sunlight actually brought into view there when Venus is situated as assumed, that it would not be discernible at all. All estimates of the refractive power of Venus's atmosphere, based on the idea that when Venus is near the sun in inferior conjunction the arc of light on the part of her limb remotest from the sun is merely sunlight *reflected from her surface after refraction through her atmosphere, are fallacious.*

(1060.) For example, Venus was seen surrounded by a complete circle of light at inferior conjunction in December 1866 (by six or seven observers). Now, the position of Venus when in inferior conjunction in December 1866 was such as is shown in fig. 316, I, at V, the planet's centre being about 38′ from the centre of the sun, or about 22′ from A, the nearest point on the sun's disc, and about 54′ from B, the farthest. Draw through Venus's centre two lines $v\ s$, $v'\ s'$, touching the sun's edge. Now, it is evident that unless the refractive power of Venus's atmosphere is much less than the Earth's, the whole disc of the sun will be brought into view above the part $v'v$ of the edge of Venus's disc. For the refractive power of our air is sufficient to raise into view a star or a part of the sun's disc really 34′ below the horizon (Art. 583). Rays thus bent 34′ in passing from the sun to reach the Earth's surface, touching it horizontally, would pass on through the air and be equally bent on their way outwards; [1] so that to anyone observing the star or sun from without, as from the planet Mars, the total refraction would be twice 34′ or 1° 8′. Since the middle of the arc $v\ v'$, fig. 316, is less than 55′ from B, a distorted image of the sun would be brought into view above the arc $v\ v'$, unless the horizontal refracting power of Venus's atmosphere is less than about 27′. But even with much smaller refracting power the whole arc $v'v$ would be full of sunlight, for $s\ v$ is much less than V B. (I take for the moment no account of the apparent diameter of Venus, though, being about 1′ 4″ when she is in inferior conjunction, it measurably strengthens the argument.) A refracting power of about 18′, as compared with the refracting power of 34′ possessed by the Earth's atmosphere, would suffice to supply the whole arc $v'v$ (II) with sunlight.[2]

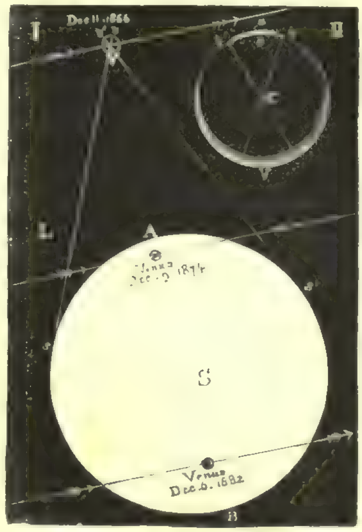


FIG. 316. Illustrating the Illumination round Venus when she is in inferior conjunction.

(1061.) It is to be remembered that the light here referred to is not merely such sunlight as Venus reflects, but the sun's own light brought by refraction to us through the atmosphere of the planet. The relative difference is as great as that between the

[1] Fig. 310 illustrates the way in which a ray of light passing from a point such as P through a planet's atmosphere is bent as it enters the atmosphere, and equally as it passes out of it. This figure and the note relating to it should be studied in connection with the text and articles which follow.

[2] It may be added that for the inferior conjunction of December 13, 1858, a refracting power of 30′ would have brought the whole arc corresponding to $v'v$ of fig. 316 (II) into view by actual sunlight, i.e. by sunlight refracted through the atmosphere of Venus, not merely reflected from the planet's surface.

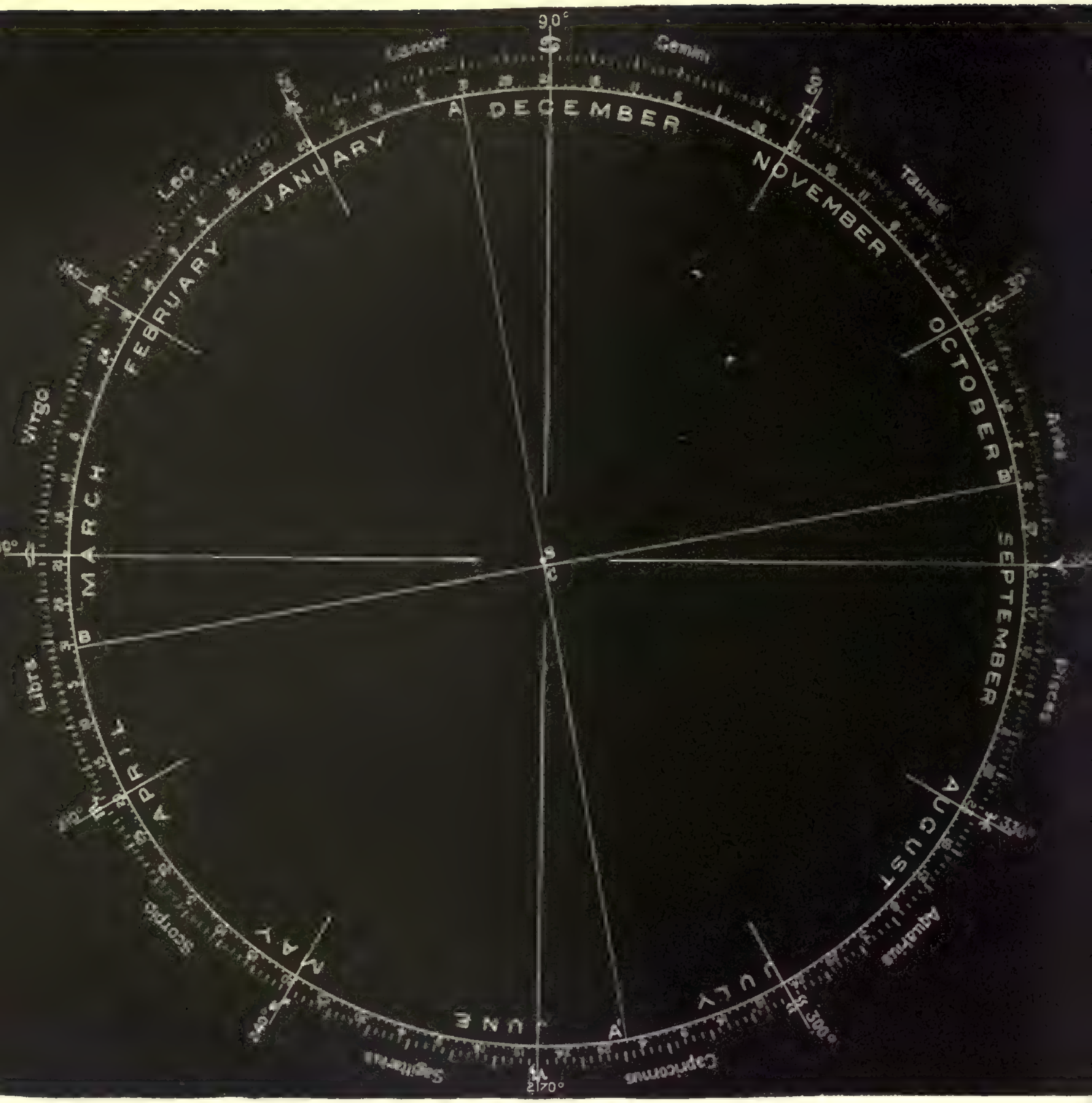
brightness of the sun as seen just raised above the terrestrial horizon by refraction and the brightness of the ground illuminated by that seemingly setting sun. It is noteworthy also that we have subjective phenomena to consider in dealing with observations such as were made in December 1866. The observer who had not studied the optical problems actually involved would naturally regard the part a of the seeming circuit of Venus's disc as that where the arc of light might be expected to be faintest, and the arc $v'a\ v$, fig. 316, II, as fainter than the rest of the circuit. But this is just the part of the boundary of Venus where real sunlight would alone be received, or sunlight only in such degree fainter than actual sunlight as the setting sun is fainter than the sun in the mid-heavens. Seeing this part of the circuit of Venus (where he expected least light) filled with light, the observer would naturally suppose that the whole circuit was illuminated, even though for a short distance on either side of v' and v the arc of light was too faint to be discerned if carefully noted. The result, so far as our estimate of the refracting power of Venus's atmosphere is concerned, would not be very different. The greatest distance at which A, the nearest part of the sun, would be brought into view at a, fig. 316, II, the part of Venus round which rays would be most effectively bent by refraction, would still determine the maximum amount of the refractive power of the planet's atmosphere; but very different inferences would be formed from the visibility of the sun himself thus brought (even partially) into view at a, and from the visibility of the arc of sun-illuminated surface of Venus of which most observers of the phenomenon we are considering have spoken. As for the idea that something akin to our *twilight zone* has been in question (or as the ill-informed, ever fond of long words, generally prefer to call it, the *crepuscular light*), that, of course, is altogether out of the question. Nothing but the effect of actual solar illumination of the surface of Venus, no effect merely due to the illumination of the planet's surface by the sunlit air, could possibly be detected from our remote station. What has been mistaken for a twilight zone is simply the zone of gradually diminishing light, corresponding to the fact that the sun is not a luminous point but a disc, and a disc as seen from Venus about 44′ in diameter.

(1062.) No well-authenticated observations of Venus when in or near inferior conjunction seem to indicate a greater refractive power for her atmosphere than 40′ for points on the surface of the planet, corresponding to 1′ 20′ for the maximum total refraction through the air.[1]

(1063.) The planet Venus undoubtedly has an atmosphere, and probably one of a greater density even above the cloud-layers enwrapping the planet, than our Earth's. In approaching the sun at the time of transit Venus has displayed an arc of light corresponding to $v'a\,v'$ of fig. 316, II, widening to a full semicircle (the greatest range it can have), and in reality, as I have said, presenting up to the time of external contact an image of the sun distorted

[1] Some writers refer to this point as one depending on formulæ and calculation. It is a matter depending solely on observation. Formulæ are required to determine what the refraction of our own air is at different heights; and formulæ would be required to determine from the refractive action of the atmosphere of Venus, as determined (let us assume) by observation, the pressure and density of the air at Venus's sea-level. But formulæ have nothing to do with the determination of the refractive action itself.

PLATE XVII.



THE EARTH'S PATH ROUND THE SUN.

[The dots outside the circle AA′ may be regarded as showing the earth at intervals of one day, on her annual course, only her globe is relatively very much smaller.]

into the form of an exceedingly fine crescent of light. When Venus is fully entered on the sun's face the refracting power of this atmosphere is of course in action, bringing into view (though details are not discernible) the whole surface of the sun beyond Venus. It may be well to consider what actually takes place.

(1064.) I have already pointed out the fallacy underlying Professor Newcomb's remark that no indications of an atmosphere could possibly be seen when Venus is in transit, for the reason that the light passing through its denser portions would be refracted entirely out of its course, so as not to reach an observer on the Earth at all. It might as well be said that no indications of the absorptive action of our Earth's atmosphere on the setting sun could be perceived, for the reason that the light passing through its denser portions would be refracted entirely out of its course. There is a time, as Venus approaches contact with the sun, when the sun's light through the very densest part of her atmosphere is brought to the observer on Earth, and even when Venus is fully entered on the sun's face parts of the sun's light are brought to the Earth through the denser, if not through the very densest, part of the planet's atmosphere. The action of the planet's atmosphere in absorbing sunlight might be expected to be sensible to observation conducted with sufficient care.

(1065.) Suppose for simplicity and symmetry that Venus is at the centre of the sun's disc, and let C, fig. 317, be the centre of the disc of Venus (considerably exaggerated in size). Let the atmosphere around Venus (also greatly exaggerated in extent) be represented by the ring whose outer boundary is the circle P Q′P′Q. Then the point P on the sun is seen at P, the point C on the sun is raised to such a position as c, and the point B is raised to the position b', which is not on but close to the outline of the disc of Venus (the black disc whose centre is at C). In like manner the point C on the sun is brought to c', to c, and to c', on the corresponding lines, or, taking all directions round C, to the circle $c\ c'c'$ c. The point A is brought into view at a, D at d, and D′ at d'. The whole disc of the sun is seen in the ring between P Q P′Q [1] and $c\,c'c\,c'$; while also the whole disc of the sun is seen, turned as it were inside out, in the narrow ring between the circle last named and the circle $b'd\ a\ d'$.[1] The corresponding relations when, as usual in transit, Venus

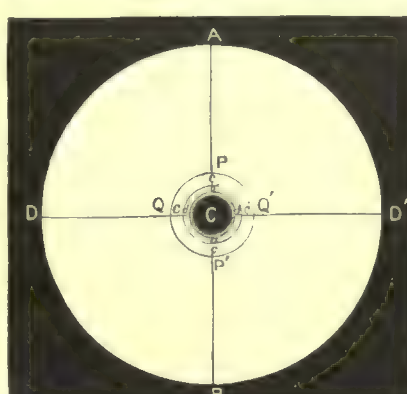


Fig. 317.—Illustrating the refractive action of the atmosphere of a Planet in transit across the Solar Disc.

[1] Between this circle $b'd\ a\ d'$ and the true outline of the disc of Venus, the background of the sky is brought into view, in the ring region shaded in the figure but really black, since the background of the sky could not but appear black. It is a curious thought that in what looks like the

is eccentrically situated on the sun's disc can be readily dealt with in the same way: we obtain corresponding rings round Venus, but they are no longer concentric. Wherever Venus is situated on the sun's face, the whole solar disc is brought into view (turned as it were inside out) round the planet's edge, and in addition the part of the sun behind the disc of Venus: so that as the rest of the sun's disc is visible directly, every part of the sun is twice visible.

(1066.) In reality, however, there would be no chance of recognising any characteristics of the atmosphere of Venus when the planet is on the sun's face, for the simple reason that the whole depth of atmosphere which could produce appreciable absorption would at the utmost be but five or six miles in depth, and even of this depth but two or three miles would act to bring the sun into view by refraction (as explained above) within the ring *c d c′d′*. A depth of five miles on Venus in transit would be the 1540th part of the diameter of Venus; or, as Venus in transit has an apparent diameter of 64″, would correspond to a breadth of only $\frac{1}{24}$ of 1″ of arc. No telescope yet made would ever show such a breadth, far less enable an observer to measure or examine it.

(1067.) Yet by means of the spectroscope the very constitution of the air of Venus has been in some degree ascertained.

It is evident that when the planet is very near the sun, as in fig. 316, I., or (nearer still) at the beginning and end of transit, the arc of sunlight as *v′a v*, fig. 316, II., or extending through a greater range when the planet is partly on the sun's face, is admirably suited for spectroscopic analysis (see Art. 865), since it is finer than even the finest line of light which could be admitted through the slit of a spectroscope. The light from this arc is sunlight after passage through the denser parts of the atmosphere of Venus; so that, by analysing it, the observer may be able to determine what vapours are present in the atmosphere of the planet. Taking advantage of the opportunity thus afforded, Signor Tacchini examined the spectrum of the arc of light round the part of Venus outside the sun between external and internal

outer edge of Venus's black disc in transit, the observer is really looking at the depths of space almost as if he were looking through the sun.

The actual luminosity of the various parts of the rings Pc′ P′c and c d c′ d would depend partly on the absorption of the atmosphere of Venus, partly on the portion of the sun from which the illumination came; but the apparent luminosity would be affected by diffraction, irradiation, and by the optical effect of contrast. Close to the circle *a′d a d′* the absorption of Venus's atmosphere would be greatest; and as the light there would come from the part of the sun's disc near the sun's edge A D′B D where the sun's lustre is least, this part of the ring round Venus would in reality be the least luminous. But it would be difficult to distinguish the fading-off of the light near the circle *a′*d *a* d′ from the effect of irradiation, extending inwards and softening off the boundary of light, while the contrast of this light against the darkness inside the circle would with most observers make this part of the ring around Venus seem at least as bright as the rest. Round the circle *cc′ c′*c the light comes from the central and brightest part of the sun's disc, but the slight superiority of the solar brightness over the solar brightness round P Q′P′ Q would be more than counterbalanced by the excess of the absorption round *cc′ c′*c over the absorption round P Q′P′ Q. There would also be a bright diffraction ring at a small distance (varying with the telescopic aperture) from the dark edge *a′d a d′*, which would altogether mask any difference of illumination arising either from difference of absorption or from difference of solar brightness.

contact during the transit of December 1874, and was able to recognise a decided darkening of the lines due to aqueous vapour as compared with those in the solar spectrum observed in the usual way at the same time. This darkening was doubtless due to aqueous vapour in the atmosphere of Venus strengthening the absorption due to aqueous vapour present at the time in the Earth's atmosphere.

(1068.) The supposed satellite of Venus is among the most perplexing enigmas in astronomy. In 1672 and 1686, using a refracting telescope 54 feet long, the elder Cassini saw what he believed to be a satellite attending on Venus, and having the same phases as the planet (as an attendant moon would necessarily have.) It was not so bright nor so well defined as Venus, was separated from her by a distance equal to about two-fifths of her diameter, and appeared to have a diameter equal to one-fourth of hers. Mr. Short, a well-known optician, made a similar observation in 1740, using two reflecting telescopes, and using with the second three different eye-pieces, magnifying 60, 140, and 240 times. Mayer saw the same appearance in 1759; Baudouin Rödkier, Horrebow, and three others saw the enigmatical attendant phenomenon at Copenhagen, and Montbarron at Auxerre in 1764. Montaigne observed it in 1791. Cassini's observation might be perhaps explained by reflection within the long focused single object-lens, and some of those made by the less experienced observers by reflections in the eye-pieces. It is quite impossible to suppose that Cassini was deceived in this way. But Short's observations, which were continued during a full hour with two reflectors and at least four different eye-pieces, cannot be explained in either of these ways. Yet it is certain that Venus has no satellite of the size of this apparent attendant. The orbit assigned to the supposed moon by Lambert in 1777 as reconciling all the observations would give to Venus a mass ten times as great as she is known to have. During transits no moon has been seen beside Venus (Scheuten said he had seen such a body during the transit of 1761, but as none of many observers of that transit, some of whom had much better telescopes, saw such an attendant body, we may safely conclude that Scheuten was in some way misled). Certainly an object of the size described would be quite easily seen even with small telescopes in our time, if it had any objective existence. Neisten's suggestion that there is a small planet travelling in an orbit which passes very near that of Venus, so that at certain times the two bodies are in close proximity, will not bear examination. The notion that some vaporous or cloud-like mass, varying in condition and therefore in visibility, travels round Venus seems as inadmissible as the idea that Venus at times expels vaporous masses from her interior, which, passing higher, grow larger and more tenuous, disappearing

in the process. The observations are most perplexing. It is, however, certain that, while on the one hand they cannot all be explained away as due to mere optical illusions, Venus has not an attendant satellite two thousand miles or so in diameter.

(1069.) The physical condition of Venus appears to correspond to a stage of planet-life through which our Earth has long since passed. In other words, Venus appears to be the younger planet. As Venus is considerably smaller than the Earth, we might expect her to be older, if the careers of the two orbs began at about the same time, since the stages in the life of the larger of two planets must necessarily be longer than the corresponding stages in the life of the smaller. But it is antecedently probable that the Earth, the chief of the terrestrial planets, would begin her career as a planet before any of the other members of that family. It will be seen that Jupiter, the chief of the family of giant planets, is probably farther advanced in planet-life than Saturn, though had they begun their careers as planets together, Jupiter would now be the younger (in development). Be the explanation what it may, Venus presents an aspect which suggests that apart from the much greater supply of heat which she receives from the sun, she retains a larger proportion than our Earth of her original heat. Her atmosphere seems to be denser and more moisture-laden, even above the layers of dense cloud which enwrap her globe, covering both sea and land (but not perhaps in equal degree) at all times.

To the data already given respecting transits of Venus may be added the following, from which any student who cares for the work may construct such illustrative charts of transits of Venus to the year 3000, as I have constructed for her transits in the years 2004 and 2012.

TRANSITS OF VENUS BETWEEN THE YEARS 2100 AND 3000.

Year	Day	Middle			Duration	
		h.	m.	s.	h.	m.
2012	Dec. 10	15	6	37	4	46
2125	Dec. 8	3	18	40	5	37
2247	June 11	0	50	23	4	16
2255	June 8	16	53	56	7	12
2360	Dec. 12	13	59	9	5	25
2368	Dec. 10	2	10	2	4	59
2490	June 12	3	58	35	2	4
2498	June 9	20	21	2	7	33
2603	Dec. 15	12	54	16	5	53
2611	Dec. 13	1	11	12	4	30
2733	June 15	7	23	56	Very close approach, but no transit	
2741	June 12	23	43	59	7	46
2846	Dec. 16	11	53	15	6	14
2854	Dec. 14	0	13	29	3	48
2976	June 17	19	23	30	Close approach; no transit	
2984	June 14	3	2	22	7	52

CHAPTER IX.

THE PLANET EARTH.

(1070.) THE Earth is the third planet in order of distance from the sun. She is the fifth in size among the eight primary planets, but chief of all the planets of her own order—the so-called *terrestrial planets.* She is the only member of the solar system which is accompanied by a companion planet, the moons attending on Mars, Jupiter, Saturn, Uranus, and Neptune being too small compared with their several primaries to be regarded as having any such character. The Earth travels at a mean distance of 92,780,000 miles from the sun. The eccentricity of her orbit is ·01677, so that her greatest, mean, and least distances are as the numbers 1·01677, 1, and 0·98323. The distance of the sun from the centre of the Earth's orbit is 1,556,000 miles, corresponding to almost exactly $\frac{1}{60}$th of the mean distance. The greatest and least distances are 94,336,000 miles and 91,224,000 miles. The *ellipticity* of the Earth's orbit—that is, the proportion which the excess of the major over the minor axis bears to the major axis (it may be called the *compression of the Earth's orbit*)—is a much less conspicuous feature than the *eccentricity.*[1]

(1071.) In Plate XVII., AB A′B′ represents the path of the Earth round the sun at S. C is the centre of the orbit, S C the eccentricity in actual distance, A C A′ the major axis, and B C B′ the minor axis. At B and B′ the Earth is at her mean distance from the sun, S B and S B′ being equal to C A and C A′. B C is less than A C by about 13,060 miles, or by

[1] By one of the fundamental properties of the ellipse the square of the $\frac{1}{2}$-minor axis is equal to the square of the $\frac{1}{2}$-major axis diminished by the square of the eccentricity (measured in distance): that is, putting the $\frac{1}{2}$-major axis equal to unity

$$(\tfrac{1}{2}\text{-minor axis})^2 = (1)^2 - (\cdot 01677)^2$$
$$= 1 - \cdot 00028123 = \cdot 99971877$$
$$\therefore (\tfrac{1}{2}\text{-minor axis}) = \cdot 9998593 = 1 - \cdot 0001407$$

It follows that the ellipticity of the Earth's orbit is ·0001407, corresponding to an excess of the major axis of the orbit (185,586,110 miles) over the minor axis by only 26,110 miles. Thus A C A′, Plate XVII., must be supposed to exceed B C B′ by 26,110 miles, A C exceeding B C by only 13,055 miles. On the scale of that figure, the average breadth of the line representing the Earth's track represents a distance of about 260,000 miles, or ten times the difference between the axes. Thus the ellipticity cannot possibly be represented on the scale of Plate XVII.

about $\frac{1}{120}$th part of S C, which, as we have seen, represents a distance of 1,556,000 miles.

(1072.) The 366 little white dots just outside the orbit AB A′B′ may be regarded as representing the Earth at successive stages of her circuit round the sun. It will be seen that, starting from the position corresponding to the Earth's place on March 20, they indicate 365 spaces and a quarter, so that if the dotting were continued through a second, third, and fourth circuit, the fourth set of these 365 spaces would be almost exactly one space from the starting-place, the 1,366th dot of that set falling almost exactly at the starting place for the four circuits. This corresponds to the circumstance that in four years there are nearly 365 + 365 + 365 + 366 or 1461 days. It must be understood that small though the dots are which are thus supposed to represent the Earth, they are in reality enormously exaggerated in size. The smallest (for the engraver could not possibly make them exactly equal) would represent a globe about 80,000 miles in diameter, or more than a thousand times larger than the Earth.[1] It may be added, that if we imagine the little dots round A B A′B′ enlarged until the diameter of each was equal to about half the distance separating dot from dot, and the disc S replaced by a dot less than the minutest of those round A B A′B′, this minute dot would represent the Earth, the 366 others representing the sun in positions corresponding to the 366 positions of the sun shown in the twelve maps, figs. 87 to 98.

(1073.) I have already dealt with the daily progress of the sun over the sky, and with the seasons in so far as they depend on the varying path of the sun, as day by day the year proceeds. The phenomena of day and night and of the seasons may also be considered from what may be regarded as the opposite point of view. Instead of considering how the sun looks as seen from the Earth, we may consider how the Earth looks as seen from the sun. Indeed, the seasons cannot be rightly or fully comprehended unless studied in this way also.[2]

(1074.) As day and night, and the various seasons, depend on the way in which (as it were) the sun looks upon the Earth, we will at this point imagine an observer watching from S the Earth traversing the path A B A′B′, Plate XVII. The observer at S would see the Earth passing from point to point (shown in Plate XVII.), day after day turning on her axis in such sort that the face seen at the end of each of these daily intervals would be almost exactly the same as that seen at the beginning, but not quite, because of the inclination of the axis of the Earth's rotation to the plane

[1] It will serve to give the student a startling idea of the real relative dimensions of the Earth, the planets generally, and the solar system, to note that if we conceive the orbits in Plate X. enlarged in such degree that the small circle representing the Earth's orbit became as large as the circle A B A′ B′ in Plate XVII., Jupiter, the giant of the solar system, would be represented on his large path by a point less than the least of those supposed to represent the Earth in Plate XVII.

[2] It is from this point of view that the seasons are usually discussed in popular treatises on astronomy; but unfortunately an explanation which was perhaps sufficient for the astronomy of two centuries ago has been repeated again and again in our books with scarcely an attempt at development or improvement.

of her motion. The return of the same face (apart from the effects of inclination) day after day, as judged by our solar observer, would correspond to our way of measuring time by solar days. The Earth being in a changed direction day after day as seen from the sun, this return of the same face does not indicate an exact rotation, but rather more. In a quarter of her annual revolution, the Earth (rotating in the same direction) has obviously to make one extra quarter rotation to bring the same face towards the sun ; in half a year half an extra rotation ; in a year a full extra rotation. In other words, 365¼ solar days are equivalent to 366¼ true rotations, or sidereal days.

(1075.) Let us next suppose the observer at S (Plate XVII.), to note the changes in the apparent axial pose of the Earth as she traverses the orbit A B A′B′. As her real axial pose remains unchanged during her entire circuit (for we may neglect that slow reeling movement considered in Arts. 528–32, since it produces no such change in a single year as need be considered here), it is manifest that the apparent axial pose will vary precisely as though the Earth remained at rest, and our solar observer travelled round her. Or the changes would equally be obtained if the Earth remained at rest so far as her bodily translation was concerned, and the solar observer remained at rest too, but the Earth's globe, besides rotating once a day, rotated once in a year around an axis perpendicular to the plane of the ecliptic.

(1076.) Thus suppose P E P′E′ (fig. 318) a view of the Earth on about March 20, when the polar axis is as shown, and the equator seen like a straight line across the centre of the disc, as E O E′, the arrowed line K O L, representing the Earth's motion as watched from the sun in the direction S♎, Plate XVII.[1] Then E will be brought into view and E′ carried out of view as the Earth goes on ; P, the north pole, being brought within the visible part of the Earth, and P′, the south pole, being carried to the invisible part. It is clear that we simplify our study of these changes, without any loss of completeness, by supposing the Earth's globe to turn round the upright axis A B (square to K O L).

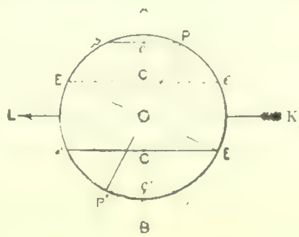


Fig. 318.—Illustrating the Earth's axial rotation.

It is obvious that E will be carried along the line E C *e* parallel to K L, and E′ along the corresponding line E′C′*e*′ on the unseen half of the Earth. The pole P will be carried along the line P*p* on the visible half, and the pole P′ along the corresponding line *p*′P′ on the unseen half of the Earth, P*p* and P′*p*′ being parallel to K O L.

(1077.) The forty-eight pictures of the Earth in Plates XVIII. to XXI. show the Earth in twelve aspects for each year, viz., at the solstices and equinoxes, and a month before and after each, and at four hours in each day, viz., at 6 A.M., noon, 6 P.M., and midnight, Greenwich mean solar time. The progress of the annual change in the Earth's aspect for any hour can be traced on from month to month by following the

[1] It will be observed that whereas the signs ♈, ♋, ♎, ♄, are usually associated respectively with spring, summer, autumn, and winter, they are put in Plate XVII. opposite the Earth's place in autumn, winter, spring, and summer respectively. This which has been regarded as a mistake, is perfectly correct. From the Earth at the vernal equinox the sun is seen towards ♈, the first point of the sign Aries ; but at this time a straight line from the sun to the Earth is directed towards ♎, the first point of the sign Libra ; and so with the other seasons and signs.

pictures horizontally, while the progress of the diurnal change for any month in the year can be traced through quarter-day periods.[1]

The student who wishes thoroughly to understand the Earth's varying presentations towards the sun should consider the apparent aspects of the latitude-parallels and meridians of our Earth thus viewed.

Let P e P′e′ (fig. 319) represent the Earth rotating yearly about an axis a O *a′*, to which the polar axis, P O P′, is inclined. Suppose the rotation such as (at the commencement of rotation) to bring P *towards* the observer, from its position as shown on the edge of the globe—the observer's distance being supposed very great compared with the dimensions of the globe. In other words, let the time be the vernal equinox of the northern hemisphere, or about March 20.

Note the following relations:

First.—Every point on the *globe* P e P′e′ or within it describes in reality a circle around the axis a O *a′*, in a plane at right angles to that axis. Seen, therefore, from a distant point of view so placed that the line of sight to O is at right angles to the *circle* P e P′e′, any point on or within the globe (sup-

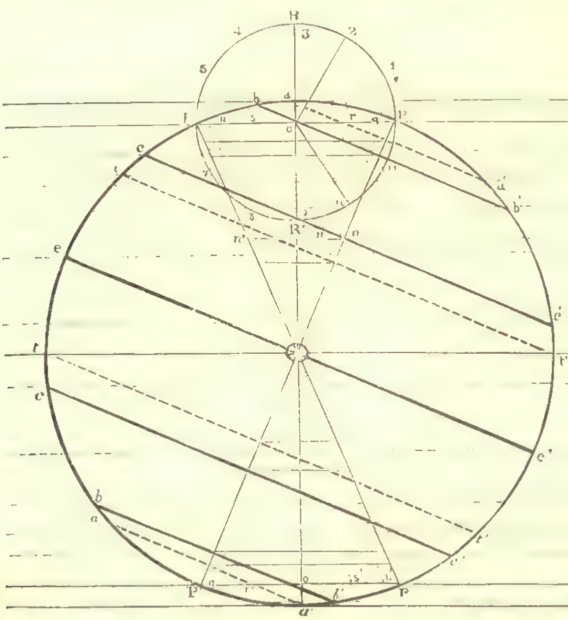


Fig. 319.

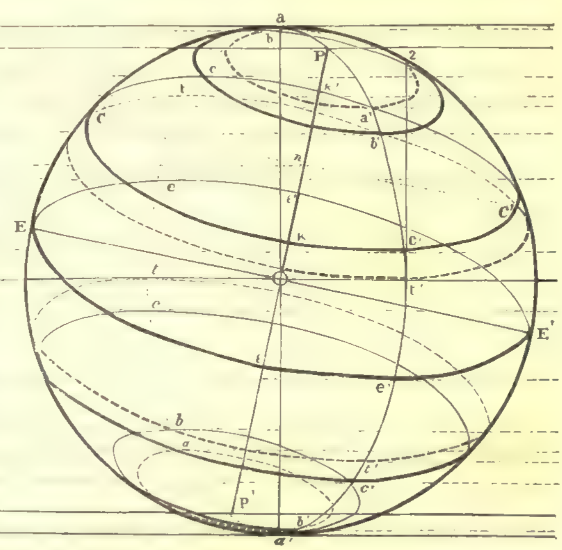


Fig. 320.

Illustrating the construction for orthographic projections of the rotating Earth.

posed for the moment to be transparent) will appear to travel backwards and forwards in a line parallel to *t* O t′, moving from right to left in the nearer half of its course, and from left to right in the farther part.

Secondly.—The rate at which such a point would appear to move in different parts of its line is very easily determined. Take, as an instance, the point P, and draw P o p at right angles to a *a′*. Then the line P o p is that along which the point P would appear to move backwards and forwards. But we know that in reality the point P is describing a circle, of which P o p is the projected view. Suppose this circle opened out by being rotated about the diameter P p—into the circle P R p R′. Then, if the points 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12 are equidistant points around the circumference of P R p R′, it

[1] Any face of the Earth shown for any named date and hour represents the face of the Earth turned *from* the sun at that hour *six months later or earlier* than the dates named on the maps; or the face of the Earth turned directly *towards* the direction in which the Earth is travelling, at that hour, *three months later*; or the face turned directly *from* the direction in which the Earth is travelling, at that hour, *three months earlier*,—than the dates named on the maps.

is quite clear that the uniform rotation around a O a' will bring the point P to the apparent positions q, r, o, s, u, p, u, s, o, r, q, P (obtained by drawing the lines 1—11, 2—10, 3—9, &c.), after successive equal intervals of time. We have precisely the same construction for any point whatever, either on or within the globe. The point n, for instance, moves to n′ and back to n, at a rate exactly proportional to that at which P moves to p and back to P.

Thirdly.—Every line through a O a' describes a double cone about a O a'. The figure of this double cone for the line P O P′ is shown; and it is clear that not only would it be as easy to show the cone for any other line through O, but also for a line through any other point in a O a'. For instance, the *line* c c′, which crosses a O a' at a point near R′, describes a double cone, of which this point is the vertex, and of which the upper is the lesser portion. The line a a′ describes a simple cone of which a is the vertex. *Finite lines which would have to be produced to meet* a O a' *produced,* describe only *frusta* of cones; but, mathematically speaking, we say that every line through any point of the line of which a O a' is a part, describes a double cone around this line as axis.

Fourthly.—The lines b b′, a a′, c c′, t t′, &c., representing latitude-parallels or circles round the axis P O P′ are projected into ellipses as the rotation we are considering goes on. The points e and e′ on the equator e O e′ remain farthest from the plane t O t′ throughout the rotation. Therefore, if the lines e e,

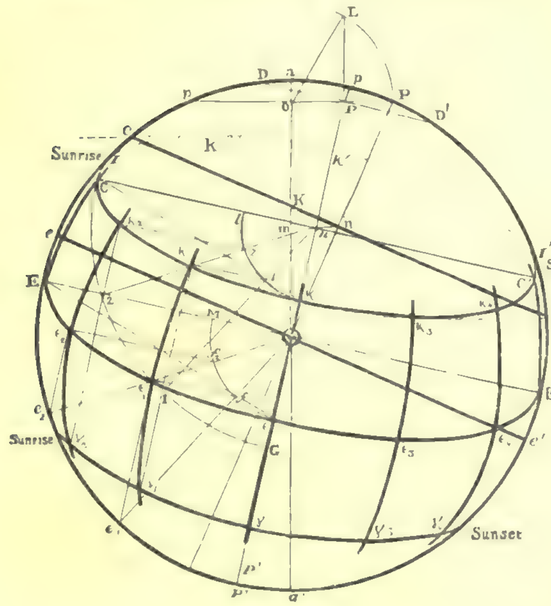


Fig. 321.

Fig. 322.

Illustrating the constructions for orthographic projections of the rotating Earth.

e′e′ are drawn parallel to t O t′, the ellipse representing the equator must always touch these two lines, and by following the plan already described for P, we immediately find the points at which, for a given amount of rotation, the ellipse touches the lines e e and e′e′. The greater axis of the ellipse is of course at right angles to the polar axis of the globe, already known. Suppose, for instance, that the rotation around a O a' has taken place through an angle equal to the angle P o 2, then the north pole has come to r, and the south pole to r′, therefore r O r′ is the position of the polar axis. In fig. 320 P O P′ is drawn in this position and E′ O E at right angles to P O P′ is the major axis of the ellipse representing the equator. We draw an ellipse having this line for major axis, and touching the lines e e and e′e′, getting the outline E e E′e′ in fig. 320.[1] The case of a latitude-parallel is just as easily examined. When P is at r, n, the centre of the parallel c n c , is at n, and a line through n at right angles to r r′, equal in length to the *line* c c′, and bisected at n, is the major axis of the ellipse representing the latitude-parallel c c′.

[1] In this case E e′E′, fig. 320, is the nearer part of the ellipse, the lighter half of the ellipse lying on the farther half of the globe, supposed for the moment to be transparent; but if we had supposed the rotation to have been through an angle P o 10 (not the acute angle, but the angle measured by the arc P R p 10), we should have obtained the same shape and position for our ellipse, only the lighter part would have been on the nearer half, and E e′E′ on the further half.

Also, c will be the point farthest from, and c′ the point nearest to the *plane* O t t′, throughout the rotation we are considering; and the ellipse will therefore always touch the lines c c, c′c′, figs. 319 and 320. If, therefore, in fig. 320 we take *n*, determined by drawing *n* n *n′* parallel to O O, to meet P P′, fig. 320, and take C C′ in fig. 320 equal to c c′ in fig. 319, at right angles to P P′ and bisected in n, an ellipse, C c′C′, having C C′ as greater axis and touching the lines c c, c′c′, represents the latitude-parallel required.[1] The part C c′C′ is the nearer, and it is clear that more than one-half of the parallel lies on the hemisphere turned towards the observer.[2] In this way every latitude-parallel is determined.

(1078.) A brief description of the geometrical processes involved in the construction of these figures may have an interest for the mathematical reader.

Describe a circle, a P *a′* (fig. 321), to represent the outline of the disc, and take the angle a O P, equal to the obliquity of the ecliptic (or nearly 23° 30′). Draw P o at right angles to a O, and describe the arc P L with centre o and radius o P. On this arc take the angle P o L, equal to the angle described by the Earth around the sun from the vernal equinox, at the moment considered. (For instance, for the four sun-views one month after the equinox in my 'Seasons Pictured' the angle would be 30°; for those one month before the Midsummer solstice the angle would be 60°; and so on.) Then L *P*, drawn at right angles to o P, gives *P* the pole of the earth. Thus *P* O *P′* is the polar axis, and E O E′ at right angles to *P* O *P′* is the major axis of the ellipse which will represent the equator. The half-minor axis O ϵ is equal to L *P*.

[This is easily shown; for the globe may be supposed to have assumed its present position (with *P* O′*P′* for polar axis) by having been rotated about E O E′ in such a manner that *P* moved along the *arc p P* (the foreshortened view of an arc equal to *p* D′ or *p* D obtained by drawing D *P* D′ at right angles to O *P*). By this motion the equator, originally seen as a line E O E′, would open out into the ellipse E ϵ E′; and since the *arc* O ϵ would necessarily be the same as the arc *p* D, the *line* O ϵ would clearly be equal to *P* D. But *P* D is equal to *P* L, since (by Euc. III., 3) the square of either of these lines is equal to the rectangle p *P*, *P* P.]

To describe an ellipse, having given axes, is a very simple matter. Since it is necessary, for our purposes, that we should have *longitude-circles* as well as *latitude-parallels*, the following method which gives points of both curves at once—that is to say, gives the points where the longitude-circles and latitude-parallels we wish to draw, intersect—is the most convenient:—

A circle, of which ϵ M is a quadrant, is described round O as centre with radius equal to *P* L. The quadrant E *p′* is divided into three equal parts in e_1 and e_2; and lines are drawn from O to these points, meeting the quadrant ϵ M in f_1 and f_2. Then lines $e_1\epsilon_1$, $e_2\epsilon_2$, parallel to *P P′*, and lines $f_1\epsilon_1$, $f_2\epsilon_2$, parallel to E E′, give by their intersection the points ϵ_1 and ϵ_2, on the ellipse representing the equator, and *also* on the longitude-circles we require.

To determine the ellipse corresponding to another parallel originally appearing as the line c n c′, a very similar method is available. The line n *n* parallel to P o gives the point *n* on O *P*, which is the centre of the ellipse we require; C *n* C′, equal in length to c c′, and at right angles to (and bisected by) *P P′* is the major axis; and since it is clear that this parallel, being (as its name implies) parallel to the equator, must be opened to exactly the same proportional extent, we get the half-minor axis *n* κ by drawing the line C κ parallel to E ϵ. Describe circles of which C G and *l* κ are quadrants, and divide the quadrant C G into three equal parts in 1, 2, as in the former case; then, the lines *n* 1 1, n 2 2, being drawn, the lines through 11 and 22 parallel to the axes give the points κ_1, κ_2, on the ellipse and also on the longitude-circles we require. The quarter-ellipses Cκ, Eϵ, and the arcs $\kappa_1\epsilon_1\gamma_1$, $\kappa_2\epsilon_2\gamma_2$, of th two ellipses representing the longitude-circles we require, may now be drawn in: and by carrying on this process for other latitude-parallels and longitude-circles we get the complete sets of lines shown in the figures of Plate XVIII. The quarter-ellipses ϵ E′ and κ C′, the portion of an ellipse *I*κ′*I′* beyond C and

[1] It is easily seen that the points of contact of all the ellipses with their corresponding pairs of parallels lie on the ellipse a t′*a* t (fig. 320); since these points of contact, originally lying on the circle to a e′*a*′e, must be brought by the rotation of this circle around a O *a′* lie on an ellipse having a O *a′* as axis, and passing through the points P and P′. Also, it is obvious that the minor axis *t* O t′ of the ellipse a t′*a t* is easily determined by taking the angle a O 2, equal to the angle R o 2 (fig. 319), and drawing 2 t′ parallel to P P′.

[2] The parallel a a′ presents a peculiarity worthy of notice. Since this parallel has upon it a point, a, which is the uppermost point of the globe, and since this point cannot but remain uppermost throughout the motion, the ellipse representing this parallel always appears to touch the outline of the globe's disc in the point a.

C′, and $\epsilon_1\gamma_1$, $\epsilon_2\gamma_2$, $\kappa_3\gamma_3$, and $\kappa_1\gamma_1$, parts of longitude-circles crossing these, are added to show the relation of that part of the construction which has been gone through to the complete figure.

Of course, the careful construction of the ellipse C κ C′κ' would give correctly the points I and I' in which this ellipse meets the circle a E a'E′ – points corresponding to the place of sunrise and sunset for places on the latitude-parallel C C′. But it is well to determine these important points by a construction founded on the following simple considerations. Since it is clear from what has been said about straight lines passing through a O a' that the plane of the latitude-parallel c c′ (fig. 321) always meets O a in the same point K (fig. 321) and since the line of intersection of this plane with the *plane* a E a'E′ must be parallel to the line E O E′ (which is the intersection of the plane of the equator with the plane a E a'E′), the line I K I' parallel to E O E′ is the line of intersection of the latitude-parallel C C′ with the plane a E a'E′; and the points I, I', thus determined are clearly those we require. For only that part of the latitude-parallel C C′ which lies on the nearer half-globe can be visible, and the circle a E a'E′ separates the visible from the invisible hemisphere.

Fig. 322 is added to show the appearance of a transparent globe traced with latitude-parallels and longitude-circles, and presented in the same way as fig. 320, which is also the presentation dealt with the constructions illustrated by fig. 321.

(1079.) To appreciate the full effect of the seasonal changes thus dealt with, and illustrated in the forty-eight figures of Plates XVIII. to XXI., it will be well to compare

Fig. 323.—Great Britain, France, &c., at noon (Greenwich solar time) in midwinter (Greenwich mean time).

the aspect of some known portion of the Earth as supposed to be seen from the sun at noon in midwinter, in spring, (or autumn) and at midsummer. Figs. 323, 324, and 325 show the British Isles, the Netherlands, Denmark, and parts of France, Germany, &c., at these seasons respectively. The heat received by any country or tract shown

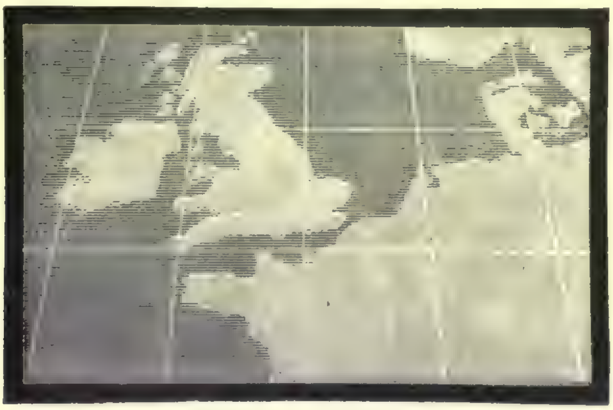


Fig. 324.—Great Britain, France, &c., at noon (Greenwich solar time) in spring or autumn (Greenwich mean time).

in these several figures, is proportional to the apparent area of the country or tract in the figure. We see how much larger England looks in fig. 324 than it does in fig. 323; in a corresponding degree does the amount of heat received by England in

a given short interval of time at noon from the sun on the day of the vernal or autumnal equinox, exceed the amount received in the same time by England at noon

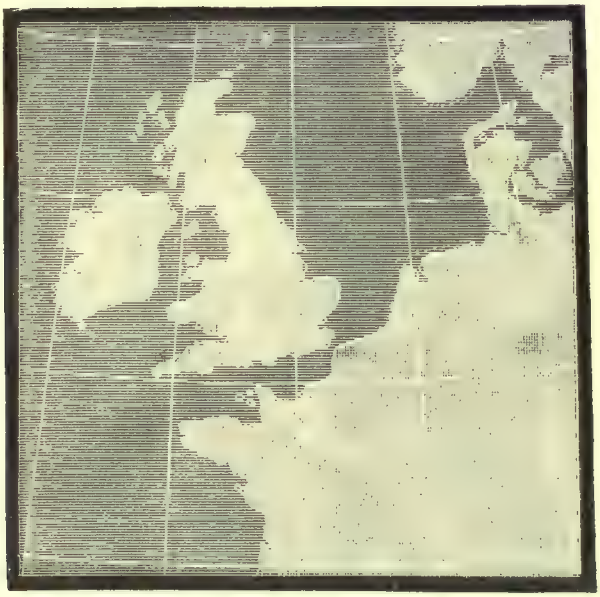


Fig. 325.—Great Britain, France, &c., at noon (Greenwich solar time) in midsummer.

on the day of the winter solstice—the atmospheric conditions being supposed similar. In like manner, comparing fig. 325 with fig. 324, we see how much more heat is

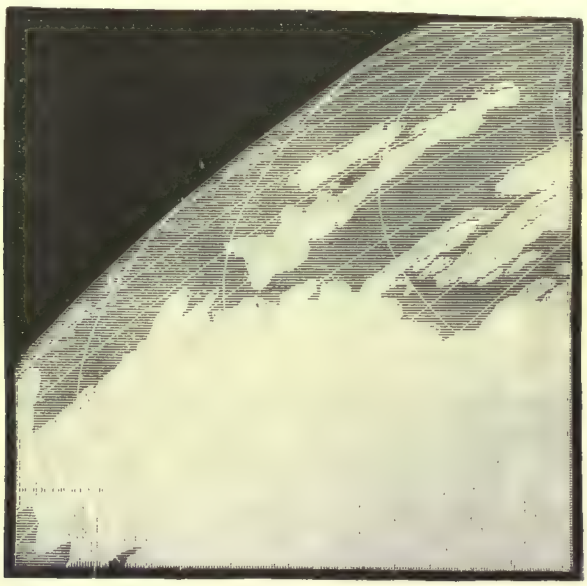


Fig. 326.—Great Britain, France, &c., at 6 A.M. (Greenwich solar time) midsummer.

received by England at noon on Midsummer's Day than at noon on the day either of the autumnal or vernal equinox.

(1080.) Of course this is not the sole reason of the increased warmth when the midday sun is higher above the horizon. For the length of the day is also

greater. This is shown by the forty-eight pictures in Plates XVIII. to XXI.; but the constructions dealt with in Arts. 1077–78 and illustrated by figs. 319, 320, 321, and 322, exhibit the varying length of the day according to the varying presentation of the Earth towards the sun more fully in regard to detail. Figs. 326 and 327 show the actual presentation towards the sun of the British Isles, at 6 A.M. and at 6 P.M. of Greenwich solar time. England looks measurably larger as seen from the sun at either of these hours on June 20 than she does at midday on December 21. She looks in fact about as large as seen from the sun at those hours as she does at midday on the 5th or 6th of February, or on the 7th or 8th of November.

(1081.) The Earth is at her nearest to the sun during the midwinter of the northern hemisphere, or more exactly on December 31 or January 1. When at her nearest to the sun, the Earth's distance is less than the mean by about one-sixtieth part, and less than the greatest by about one-thirtieth part (Art. 1070). It follows (since the place of the Earth at the winter solstice is within 12° of the Earth's aphelion), that a square mile of the Earth squarely exposed to the sun's rays receives about one-thirtieth more light and heat at midwinter of the northern hemisphere than at spring or autumn, and about one-fifteenth more than at midsummer. But it is clear from figs. 323, 324, and 325 that the varying presentation (in our English latitudes at any rate) affects the supply of heat much more importantly. England in fig. 324 looks larger than in fig. 323 by much more than one-thirtieth, and looks larger in fig. 325 than in fig. 323 by much more than one-fifteenth. We see then why in our latitudes the winters are colder than the summers although the Earth is more than three million miles nearer the sun in our midwinter than he is in our midsummer—apart from the change in the length of the day which increases the disproportion in the amount of heat received from the sun.[1]

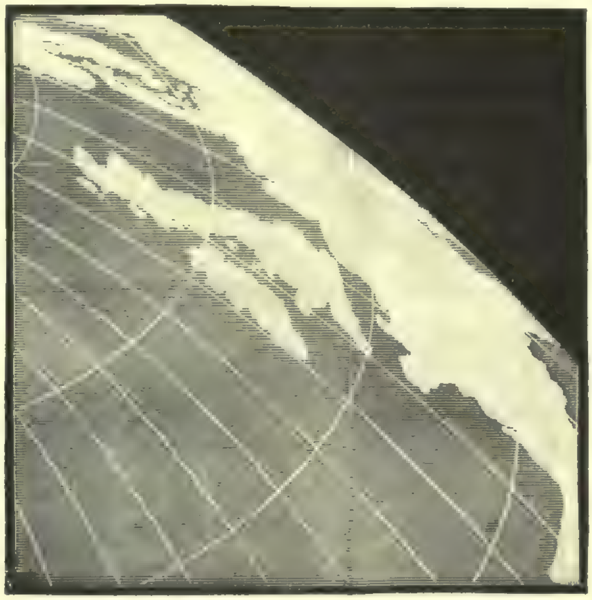


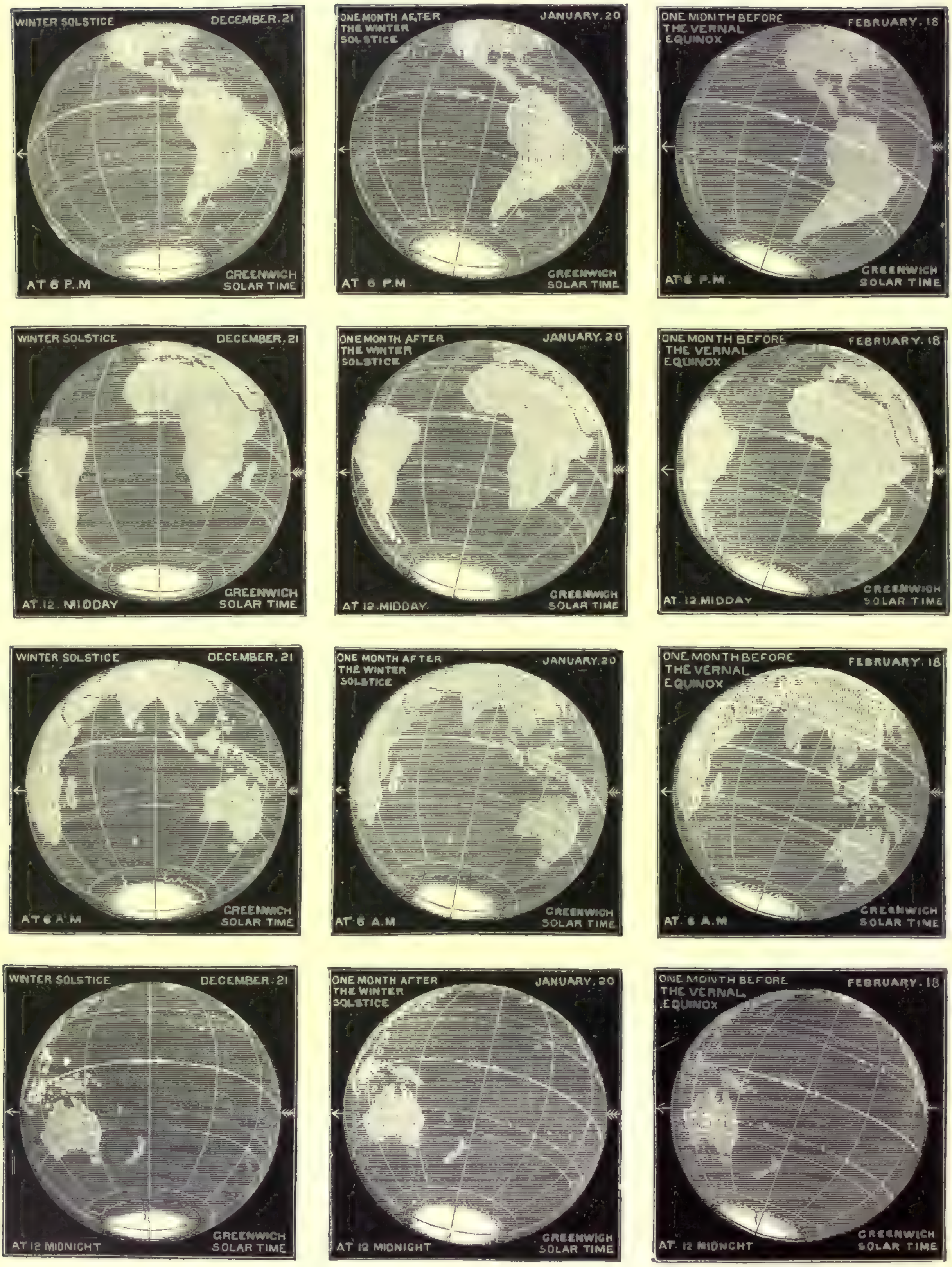
FIG. 327. Great Britain, France, &c., at 6 P.M. (Greenwich solar time) midsummer.

(1082.) The interval from the autumnal equinox through winter to the vernal equinox is less than the interval from the vernal equinox through summer to the autumnal equinox, since the earth moves more rapidly when in or near perihelion than when in or near aphelion.[2] At present the intervals are respectively 186 days 11 hrs., and 178 days 19 hrs., the difference being 7 days 16 hrs. But when the perihelion corresponded with the winter solstice, the difference amounted to more than

[1] The quantitative problems involved can be treated with any amount of attention to points of detail; but the labour involved in estimating the precise amount of heat received each day of the year, in different latitudes, would in reality be almost wholly wasted; since geographical considerations affect the actual climates of different places much more importantly than the astronomical relations involved.

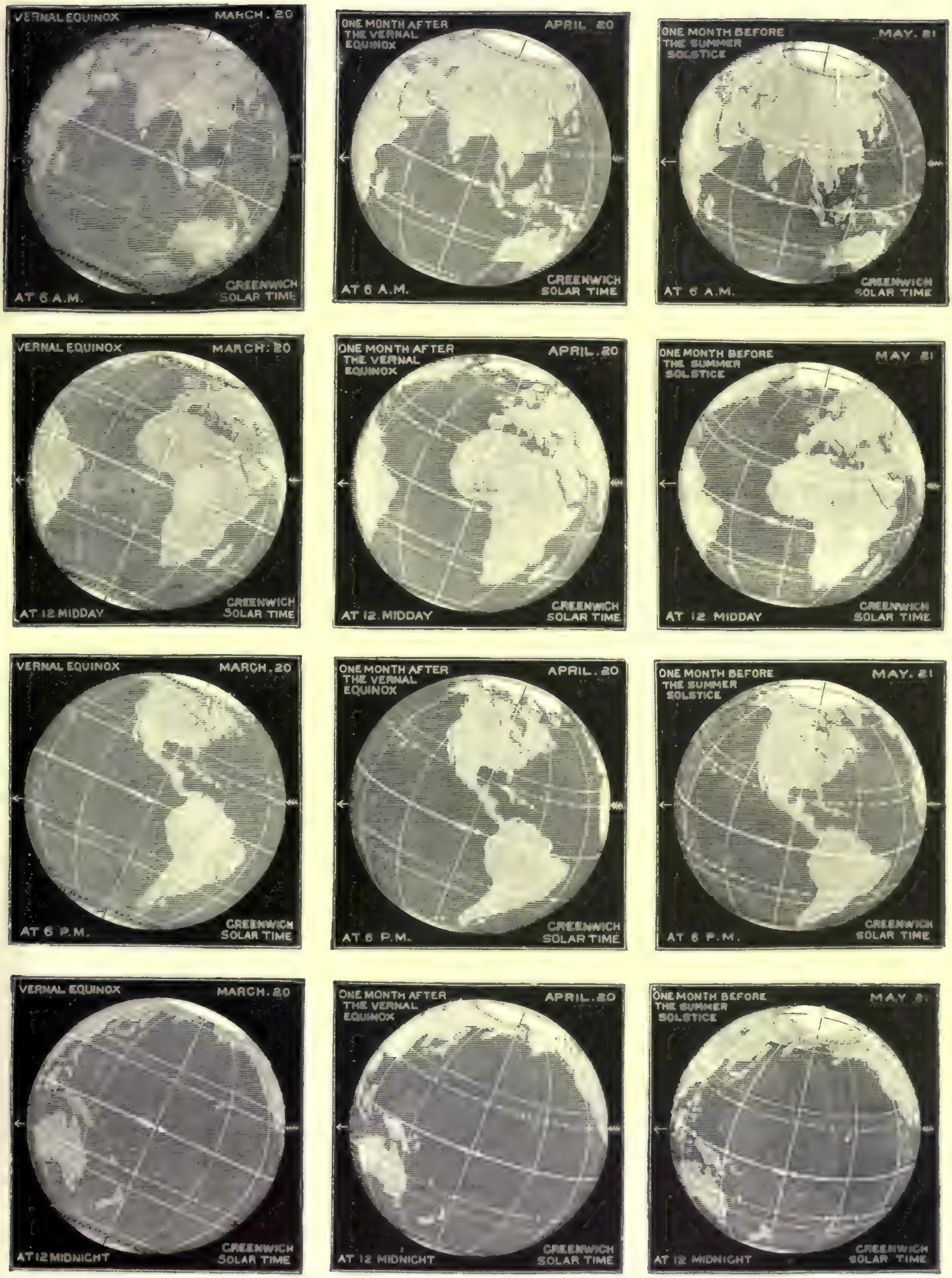
[2] The line ♎ S ♈, Plate XVII., manifestly cuts

PLATE XVIII.



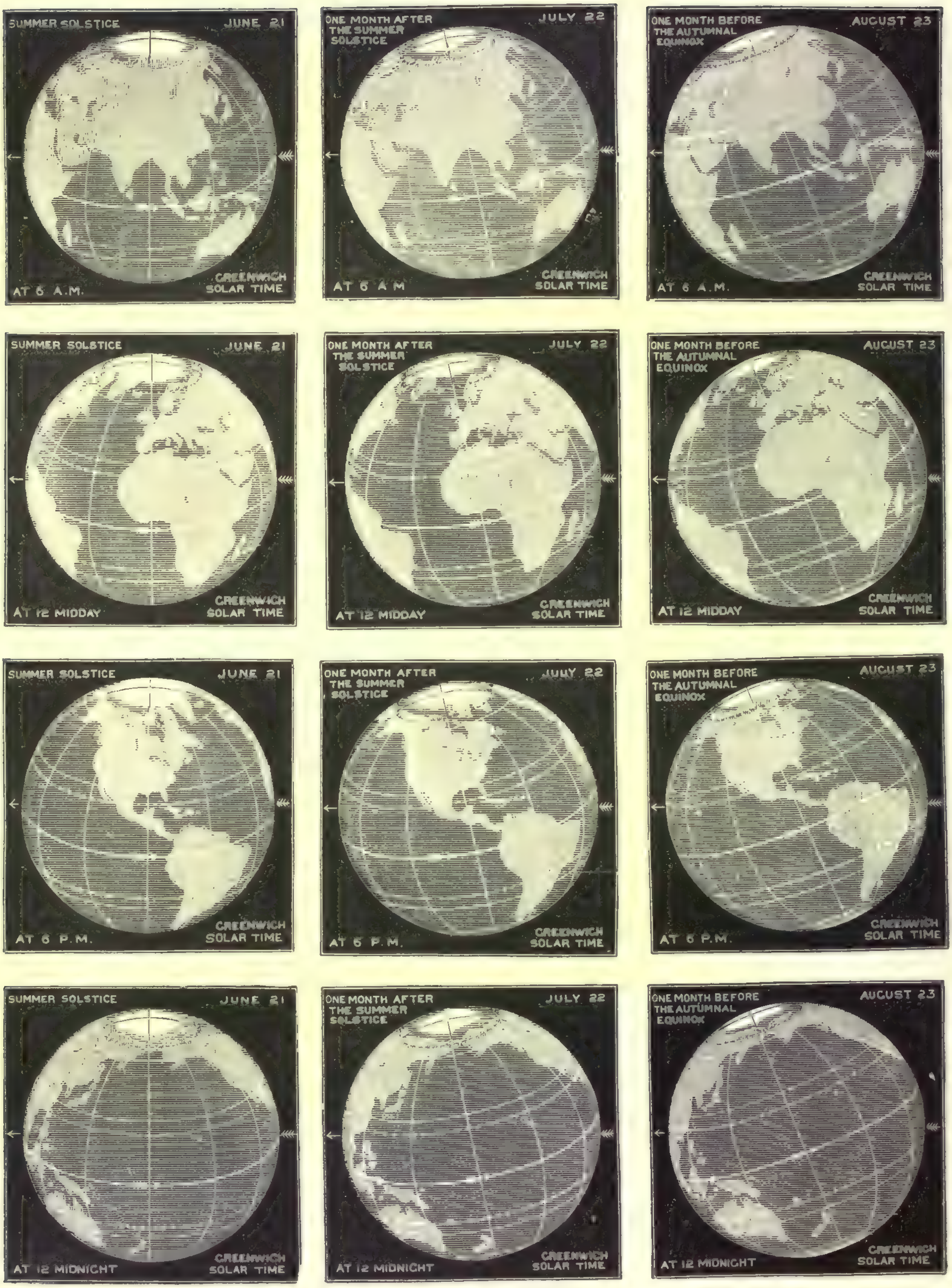
SUN VIEWS OF THE EARTH IN DECEMBER, JANUARY AND FEBRUARY.

PLATE XIX.



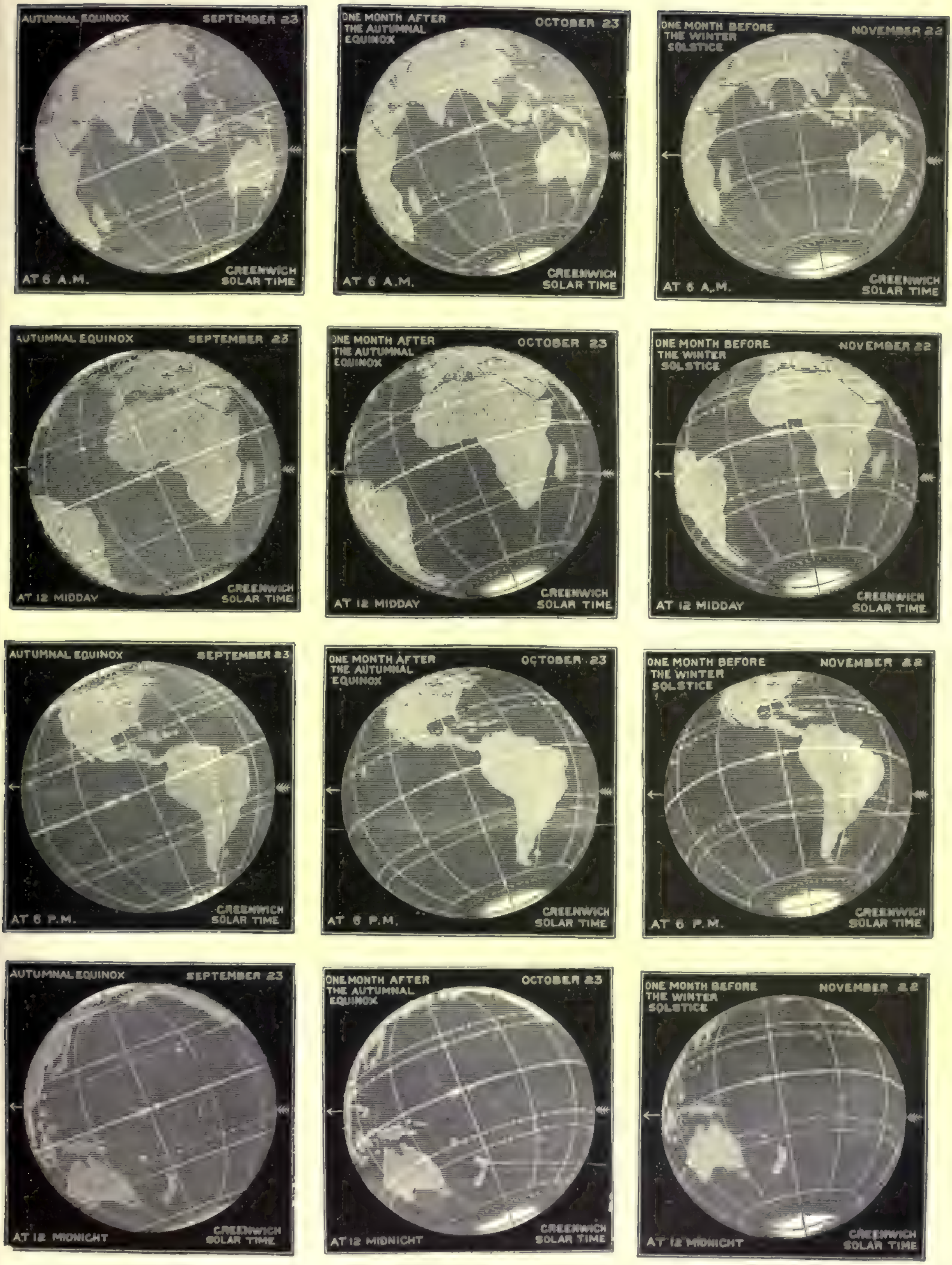
SUN VIEWS OF THE EARTH IN MARCH, APRIL, AND MAY.

PLATE XX.



SUN VIEWS OF THE EARTH IN JUNE, JULY, AND AUGUST.

PLATE XXI.



SUN VIEWS OF THE EARTH IN SEPTEMBER, OCTOBER, AND NOVEMBER.

8 days 2 hrs. With the present eccentricity of the earth's orbit this is the greatest difference which can accrue.

(1083.) The effect of the existing state of things is that for the northern hemisphere (outside the tropics) the winter is made less severe by the Earth's approach towards the sun, and shortened because of her more rapid motion, whereas the summer is less intensely hot than it otherwise would be, and is correspondingly lengthened. In the southern hemisphere, on the contrary, the extremes both of summer heat and of winter cold are made more intense, and while winter is lengthened summer is shortened. Apart from geographical causes, the effect is obviously to make the contrasts of the seasons greater in the southern than in the northern hemisphere. In so far as contrast is concerned, the effect of the intenser heat of the southern summer is not diminished by its shorter duration, while the effect of the greater cold of the southern winter is increased by its longer duration. But geographical causes play so important a part in regard to climate that the differences which astronomical causes considered alone would tend to impart are in many cases wholly masked. For instance, it appears (from observation) that the excess in the quantity of water in the southern hemisphere so far tends to equalise temperature throughout the year as to render the contrasts between winter and summer on the whole less in the southern than in the northern hemisphere.

(1084.) But on the other hand, although the total amount of heat received during the year in the southern hemisphere is equal to that received in the northern—nay, the amount in the short but intense southern summer equal to the amount in the milder but longer northern summer, and that in the long cold southern winter equal to that in the shorter and milder northern winter—the actual annual climate of the southern hemisphere is manifestly much severer, at least in high latitudes, than that of the northern. Southern latitudes corresponding to the latitudes of London, Edinburgh, Dublin, &c., are almost uninhabitable: witness Darwin's account of the physical habitudes of Tierra del Fuego and Southern Patagonia. We need not consider here the cause of this difference, beyond noting that though the problem is too complicated to have a simple solution, Mr. Croll is probably right in regarding the chief cause of the greater degree of cold prevailing in the southern hemisphere to be the flow of warm currents from the southern to the northern hemisphere, the warmer water thus passing northwards being replaced by cold under-currents travelling southwards. But inasmuch as the way in which the great equatorial warm currents are in larger degree deflected northwards than southwards depends on the relations between the oceans and continents, which relations, as we shall see, depend largely on the difference of climate in the northern and southern hemispheres, we must regard the cause recognised by Mr. Croll as primarily an effect. At present, however, we need consider only the fact that the hemisphere which has the sun in or near perihelion in winter—at present the northern hemisphere—is on the whole warmer than the other.

(1085.) But the conditions existing now have not existed throughout the past, and will not continue during the future over which astronomy extends its survey. The perihelion of the Earth's orbit, which is now passed ten days or so after the winter solstice of the northern hemisphere, was coincident with the winter solstice in the year 1248. The perihelion is passing away from the place of the winter solstice

off a smaller area from the circle (or ellipse) A B A′B′ on the side towards the perihelion A than on the side towards the perihelion A′; a straight line through S parallel to ♎ S ♈ would divide the orbit into equal areas.

at the rate of 61″·6 per annum—the perihelion advancing in sidereal longitude 11″·2 annually, while the winter solstice retrogrades (on account of precession) 50″·4 annually. Thus the arc separating the places of the perihelion from the winter solstice increases, at present, one degree in about 58·44 years.

(1086.) The perihelion, while advancing at the rate mentioned, by which it would make a sidereal revolution in 115,300 years, would make the complete circuit of the seasons in 21,040 years, passing from coincidence with the winter solstice of the northern hemisphere to the summer solstice (or to the winter solstice of the southern hemisphere) in 10,520 years. It is commonly stated, not only in mere compilations on astronomy, but in treatises by competent astronomers, that this actually takes place. As a matter of fact, however, the motion of the perihelion (in sidereal longitude) has not the regularity thus erroneously implied. It sometimes advances much more rapidly than it is now doing, while at other times its advance is much slower, or it even retrogrades. Thus, if we trace back its motion in sidereal longitude to the time when it last had the position which it now occupies (about 100° 31′ in 1888) we find that instead of 115,300 years we have to go back over about 232,500 years. And considering the motion of the perihelion during that circuit, we see why more than double the time was required than the present rate of advance of the perihelion would have involved. For, taking intervals of 10,000 years from the year 0, backwards to the year 240,000 B.C., we find that instead of the advance of about 31¼° in 10,000 years, resulting from the present annual advance, there were only two out of the 24 intervals of 10,000 years in which the advance exceeded 30°, only 10 in which it exceeded 20°; in 6 of the intervals the advance was between 10° and 20°; in 4 it was between 0° and 10°; while lastly in each of the remaining two intervals the perihelion retrograded by more than 20°.

(1087.) The eccentricity of the Earth's orbit also varies in such sort as to modify largely the effect due to the position of the perihelion. It may diminish to about one-sixth of its present value; but it may also become about 4½ times as large as it is now. In about 23,900 years from now the eccentricity will have nearly its minimum value, rather less than 0·003, after which it will increase again. In the past 99,000 years the eccentricity has been diminishing from about 0·0175, its last maximum, nearly six times its present value. But it must not be supposed that the progression from maximum to minimum eccentricity is uniform, or takes place in any definite time, or that the maxima and minima are usually equal. It is very seldom that so marked a minimum is attained as that through which the eccentricity will pass 23,900 years hence. On the other hand, the last maximum was far below the absolute maximum, and also below several maxima which have been attained during the last three millions of years, or will be attained during the next million years, as calculation (which has not been extended outside this range of 4,000,000 years) has shown.

(1088.) The following tables, taken from Mr. Croll's very valuable work 'Climate and Time,'[1] should be carefully studied. They serve to show the way in which the perihelion moves in longitude, advancing on the whole, but with varying velocity and occasional intervals of retrogression, and also how the eccentricity varies in amount,

[1] The tables have been calculated from Leverrier's formulæ published in 1843. No account is taken of the action of Neptune, nor are terms of the third order considered. But although the results here given would be measurably corrected were more complete formulæ employed, the seasons when maximum eccentricity has been attained would not be appreciably altered. This, combined with the fair idea which the table gives of the relative values of the eccentricity maxima, leaves the value of the tables scarcely at all affected.

ranging also in unequal periods between maxima and minima severally unequal in amount.

ECCENTRICITY AND LONGITUDE OF THE PERIHELION OF THE EARTH'S ORBIT.

Years before 1800	Longitude of Perihelion[1]	Eccentricity of Earth's Orbit	Years before 1800	Longitude of Perihelion[1]	Eccentricity of Earth's Orbit
	° ′			° ′	
1,100,000	54 12A	0·0303	650,000	141 29R	0·0226
1,050,000	4 8R	0·0326	600,000	32 34A	0·0417M
			550,000	251 50A	0·0166*m*
			500,000	192 56R	0·0388M
1,000,000	248 22A	0·0151*m*	450,000	356 52A	0·0308
990,000	313 50A	0·0224	400,000	290 7R	0·0170*m*
980,000	358 2A	0·0329	350,000	182 50A	0·0195*m*
970,000	32 40A	0·0441	300,000	23 29A	0·0424M
960,000	66 49A	0·0491			
950,000	97 51A	0·0517M	250,000	59 39A	0·0258*m*
940,000	127 42A	0·0495	240,000	74 58A	0·0374
930,000	156 11A	0·0423	230,000	102 49A	0·0477
920,000	181 40A	0·0305	220,000	124 33A	0·0497
910,000	194 15A	0·0156	210,000	144 55A	0·0575M
900,000	135 2R	0·0102*m*	200,000	168 18A	0·0569
890,000	127 1R	0·0285	190,000	190 4A	0·0532
880,000	152 33A	0·0456	180,000	209 22A	0·0476
870,000	180 23A	0·0607	170,000	228 7A	0·0437
860,000	209 41A	0·0708	160,000	236 38A	0·0364
850,000	239 28A	0·0747M	150,000	242 56A	0·0332*m*
840,000	269 14A	0·0698	140,000	246 29A	0·0346
830,000	298 28A	0·0623	130,000	259 34A	0·0381
820,000	326 4A	0·0476	120,000	274 47A	0·0431
810,000	348 30A	0·0296	110,000	293 48A	0·0460
800,000	343 49R	0·0132*m*	100,000	316 18A	0·0473M
790,000	293 19R	0·0171	90,000	340 2A	0·0452
780,000	303 37A	0·0325	80,000	4 13A	0·0398
770,000	328 38A	0·0455	70,000	27 22A	0·0316
760,000	357 12A	0·0540	60,000	48 8A	0·0218
750,000	27 18A	0·0575M	50,000	50 3A	0·0131
740,000	58 30A	0·0561	40,000	28 36R	0·0109*m*
730,000	90 55A	0·0507	30,000	5 50A	0·0151
720,000	125 14A	0·0422	20,000	44 0A	0·0188
710,000	177 26A	0·0307	10,000	78 28A	0·0187
700,000	208 13A	0·0220*m*	0	99 30A	0·0168

	Longitude of Perihelion[1]	Eccentricity of Earth's Orbit	Years after 1880	Longitude of Perihelion[1]	Eccentricity of Earth's Orbit
	° ′			° ′	
1800	99 30	0·0168	450,000	98 37	0·0231
1880	100 32	0·0168	500,000	157 26	0·0534
			550,000	287 31	0·0259
Years after 1880			600,000	285 43	0·0395
50,000	38 12	0·0173	650,000	144 8	0·0169
100,000	114 50	0·0191	700,000	17 12	0·0357
150,000	201 57	0·0353	750,000	0 53	0·0195
200,000	279 41	0·0246	800,000	140 38	0·0639
250,000	350 54	0·0286	850,000	176 41	0·0144
300,000	172 29	0·0158	900,000	291 16	0·0659
350,000	201 40	0·0098	950,000	115 13	0·0086
400,000	6 9	0·0429	1,000,000	57 31	0·0528

In this table A indicates advance; R retrogression of the perihelion; M stands for maximum; *m* for minimum eccentricity.

[1] The longitude is measured from the first point of Aries on January 1, 1800, and must not be confounded with longitude as measured from the vernal equinox at the several epochs named in the year-column.

(1089.) It will be observed that the eccentricity remains long enough near any of its maximum values for the perihelion to make the full circuit of the seasons. Even when the perihelion is retrograding, when the time required for the perihelion to make the circuit of the seasons is greatest, (since the equinoxes and solstices retrograde also, only much more rapidly) it would never be much greater than 35,000 years. But the perihelion does not retrograde when the eccentricity is near a maximum; and when the perihelion is advancing, a period ranging from about 18,000 to about 22,000 years suffices for the perihelion to make the complete circuit of the seasons, corresponding in succession with the position of the winter solstice, the vernal equinox, the summer solstice, the autumnal equinox, and the winter solstice again.

(1090.) Whatever effects then are to be attributed to the Earth's eccentricity as associated with the seasonal pose of the Earth, must be repeated at each passage of the earth's orbit through a season of great eccentricity, alternating at least once from hemisphere to hemisphere. If, for instance, we are to attribute the greater cold of the southern hemisphere during the past two or three thousand years to the position of the Earth's perihelion near the place of the winter solstice of the northern hemisphere, and if we are to suppose that the contrast between the two hemispheres is much greater when the eccentricity is greater (the winter solstice of either hemisphere being similarly situated), we may conclude that such contrasted conditions have alternated from one hemisphere to the other two or three times, if not oftener, while the Earth's orbit was passing through each of the stages of maximum eccentricity recorded in the above table. If, further, we regard the great present preponderance of water in the southern hemisphere as due, directly or indirectly, to the present position of the perihelion with regard to the winter solstice of the northern hemisphere, then we may conclude that the excess of water has flowed alternately from hemisphere to hemisphere, at intervals ranging from 9,000 to 15,000 years (according to the rate and direction of the perihelion's motion), during immense periods of time. For the eccentricity of the Earth's orbit is at present much nearer its minimum than its maximum value.

(1091.) The reasoning is physical rather than astronomical on which we must base our opinion as to the connection between the position of the perihelion and the peculiarities either of the climates or of the physical condition of the two hemispheres. The probabilities appear, however, to incline strongly towards the conclusion that the figure and position of the Earth's orbit must be regarded as at least a prime if not the chief factor in bringing about these peculiarities. It does not appear to me that the present geographical relations of the Earth can be regarded as permanent. Lyell in some degree begs the question at issue in so regarding them. If as a result of orbital and seasonal relations the waters are drawn towards one or the other pole, there must be in the hemisphere whence the waters have flowed some such peculiarities as exist now in the northern hemisphere. Details might be different, but the general relations on which the movement of oceanic and atmospheric currents depends would exist in either hemisphere partially denuded of its waters, as the northern hemisphere is now according to the view we are considering. Whether the difference between the temperature of the two hemispheres is to be regarded as chiefly caused by oceanic and atmospheric currents thus arising, or whether we should regard the geographical relations on which these currents depend as caused by the difference

between the two hemispheres in regard to the supply of heat they receive, or whether (which is more probable) cause and effect are here interchangeable, acting and reacting on each other, makes very little difference; it seems clear that the marked difference between the northern and southern hemispheres which does exist is associated closely (though the exact nature and cause of the association may be difficult to determine) with the position which the perihelion of the Earth's orbit has occupied in regard to the solstices during the last two or three thousand years.

(1092.) The most striking and obvious difference between the two hemispheres is the preponderance of water in the southern. If, as Adhémar suggested, this difference is due to the position of the perihelion of the Earth's orbit, the greater cold of the southern hemisphere resulting in the accumulation of great masses of ice over the antarctic regions, then the centre of gravity of the Earth's globe regarded as a whole must be displaced towards the south. But the general surface of the ocean will not

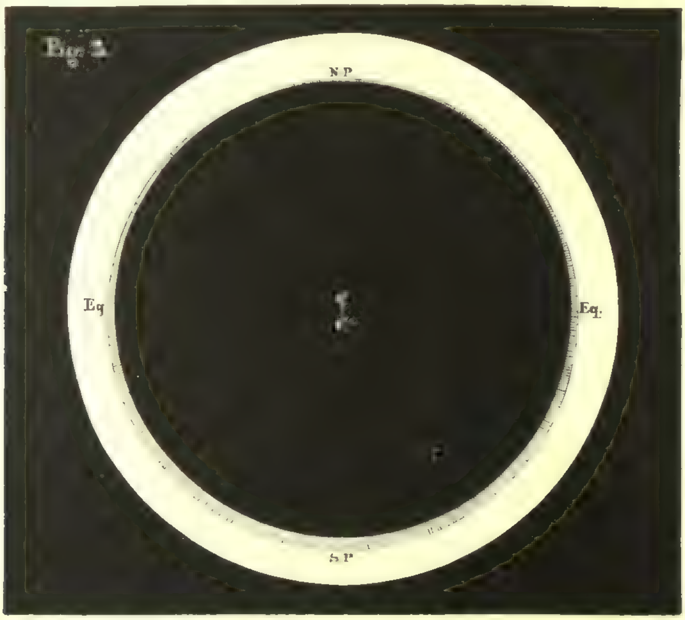


Fig. 328.—Illustrating the displacement of the Earth's centre of gravity.

have the centre of gravity as approximately its centre of figure, since the specific gravity of water, whether liquid or frozen, is but about $\frac{2}{11}$ of the mean specific gravity of the Earth.

(1093.) Thus, let C, fig. 328, be the centre of gravity of the solid earth supposed to be shown by the black disc, C′ the centre of gravity of the combined mass of the solid earth and its water envelope supposed to be displaced, as shown by the shaded ring, towards the south. Then, if the whole mass, solid and liquid, were of the same specific gravity, C′ would be the centre of the surface of the ocean, but the water being of less specific gravity than the solid earth C′, the centre of gravity of the solid and liquid masses together is nearer to C than is the centre of figure of the water's surface. In other words, the water stands out higher over southern regions than it would if it had the centre of gravity C′ for its centre of figure. If then there were another ocean of much less specific gravity outside the water surface, since this outer

ocean would have C′ for its centre of figure (that is, if we neglect its own mass, which we may on the hypothesis of its small specific gravity), it would be shallower over southern regions than over corresponding northern regions. But there *is* an ocean of just this nature, namely the atmosphere, an aerial ocean which must inevitably indicate the displacement of the Earth's centre of gravity, if the water really stands out over southern regions above the normal mean level. The atmospheric pressure should be less over the southern temperate zone than over the northern temperate zone. (The difference over the tropics would be scarce noticeable, while within the arctic and antarctic zones observations have not been numerous enough for effective comparison). We can even form an idea of the amount by which the mean barometric pressure over high southern latitudes (within the temperate zone) would fall short of the mean barometric pressure over corresponding northern latitudes. For we can estimate the limits between which we must set the probable excess of thickness of the antarctic over the arctic ice-cap. We know the density of water ($\frac{2}{11}$ of the Earth's) and the density of ice ($\frac{1}{6}$ of the Earth's), and we can thence deduce approximate formulæ limiting the probable rise of the southern seas above (and the consequent lowering of the northern sea-level below) the normal level. Sir William Thomson, making reasonable assumptions in regard to the unknown quantities of the problem, deduced 380 feet as the probable rise at the south pole and lowering at the north pole, or a difference of 760 feet between the sea-level as calculated for the two poles.[1]

(1094.) As a matter of fact the difference of barometric pressure in the two hemispheres does closely correspond with this estimate, as the following tables show:—

Mean Barometric Pressure in the two Hemispheres as estimated from Systematic Observations in the Northern Hemisphere, and from the Records of Voyagers in the Southern.

North Latitude	Mean Barometric Pressure Inches	Mean Barometric Pressure Inches	South Latitude °	′
0	29·853	29·974	0	0
10	30·002	30·016	13	0
20	30·004	30·085	22	17
30	30·069	30·023	34	48
40	30·006	29·950	42	53
45	30·011	29·664	45	0
50	29·943	29·469	49	8
55	29·960	29·347	54	26
60	29·835	29·360	55	52
65	29·623	29·114	60	0
70	29·722	29·078	66	0
75	29·863	28·928	74	0

[1] If we represent by t the thickness of the ice-crust at the south pole, and regard the ice-crust as varying in different latitudes according to the sine of the latitude, the ice being supposed removed from the northern to the southern latitude (or, which is the same thing, if we regard t as the extra thickness of ice at the southern pole, and the excess in other southern latitudes over corresponding northern latitudes as varying according to the sine of the southern latitude), and if ω be the proportion of water to land surface all over the Earth (the land being supposed distributed in flat-topped islands), ω the mean density of water, i that of ice, then the sea-level at the southern pole will be higher than the sea-level at the northern by $\frac{(1-\omega)\,i\,t}{(1-\omega)\,w}$, the excess of height in other latitudes varying as the sine of the latitude. If $\omega = \frac{2}{3}$, $t = 6{,}000$ feet, $w = \frac{2}{11}$, and $i = \frac{1}{6}$, it will be found that the rise at one pole and the depression at the other will amount to 380 feet, the difference being therefore 760 feet.

From these tables I deduced in 1867 (see an article on the Low Barometer of the Antarctic Zone, in my 'Light Science for Leisure Hours,' second series) the inference that the mean barometric pressure is about one inch lower at the south pole than at the northern. Accepting this, I said, 'it is easily calculated that the difference amounts to a difference of level of about 850 feet; in other words the surface of the water at the south pole lies farther than the surface of water at the north pole, from the centre of gravity of the entire fluid and solid globe by about 850 feet.'[1] This is singularly near to the probable value of this difference deduced by Sir William Thomson from the effects of the gathering of ice over the antarctic regions.

(1095.) It appears to me therefore probable that there is an element of truth in Adhémar's theory, speculative though the form was in which he presented it to the scientific world. There seems to be undoubtedly a present displacement of the centre of gravity of the Earth's combined solid and liquid mass, such as the probable aggregation of ice over the antarctic regions would produce. If so marked an effect has resulted from the present position of the perihelion of the Earth's orbit, small though the eccentricity of the orbit now is, it may be inferred that at each interchange of the position of the perihelion from coincidence with the winter solstice of the northern to coincidence with that of the southern hemisphere, the water has flowed over—not cataclysmally, as Adhémar imagined, but slowly and continuously—from one hemisphere to the other and back again, at intervals averaging eleven or twelve thousand years, except during those periods in which the eccentricity of the Earth's orbit has been exceptionally small. It seems to me that some of those cases of slow submergence attributed by geologists to the sinking of the land (as attested by the formation of 'atolls' and 'coral reefs' in Darwin's well-known theory[2]) may be ascribed rather to the slow inflow, or rather overflow of water proceeding from the gradual increase in the ice-masses round one or the other pole.

(1096.) The actual maximum eccentricity of the Earth's orbit was estimated by Lagrange at 0·07641, and later, with fuller materials on which to base an opinion, by Leverrier at 0·077747. Mr. Stockwell, taking into account the disturbing influence

[1] For convenience and simplicity of explanation I have made C in fig. 328 represent the centre of gravity of the entire solid and liquid mass of the Earth. This must be remembered in comparing the wording of my reasoning here with that of the fuller reasoning from which the above passage is quoted.

[2] I am aware that some few geologists regard with favour Mr. Murray's attempt (and the earlier attempt of Mr. Semper) to return to what may be called the volcanic theory of coral-reef formation; but it not only appears that the weight of authority is still in favour of Darwin's theory, but so far as a student of science viewing the matter from the outside can judge, the reasoning of Darwin, Dana, and Huxley seems sounder than that of Semper, Murray, and Geikie. I may remark here on a common misapprehension of Darwin's final position on this question. He held, as does Dana, that the evidence insisted on by Mr. Murray is not necessarily inconsistent with the subsidence theory, and many seem to imagine (it is to be hoped the Duke of Argyll does, for no otherwise can the tone of his comments be justified) that all Darwin could say, at last, for his theory, was that the evidence is not decisive against it. What Darwin really thought (though he expressed it with that characteristic modesty which seems so unintelligible to the unscientific many) was that the theory which seems demonstrably established by the great mass of evidence is not necessarily even touched by the evidence adduced against it by Mr. Murray. Taking what Darwin said about his own theory in this sense, and combining with it his remark that the evidence thus regarded as not necessarily inconsistent with his own theory is fatal, if rightly considered, to Mr. Murray's, the least thoughtful of the unscientific ought to be able to infer how remote from what the Duke of Argyll suggests was Darwin's own view of the effect of Mr. Murray's evidence on Darwin's subsidence theory of coral formation.

of Neptune, not known to Leverrier when his computations were made, reduces the maximum value to 0,069,3888 (Vol. xviii. of the 'Smithsonian Contributions to Knowledge,') and probably most of the higher eccentricities in the tables of Art. 1088 should be reduced in about this degree; but as the general reasoning of Arts. 1085 to 1099 would not be affected by such a change (see note), I have not thought it necessary to undertake the heavy labour of recomputing the table by the corrected formula resulting when the planet Neptune is taken into account.[1]

(1097.) Fig. 329 represents the Earth's orbit as it was about 852,000 years ago, at a time when it had very nearly its greatest possible eccentricity. Instead of the eccentricity being as now only about one-sixtieth, it was at that time nearly a thirteenth of the mean distance. In other words, instead of the Earth ranging about 1,556,000 miles on either side of her mean distance, she then ranged about 7,180,000 miles from and toward the sun, making a total range of distance of about 14,360,000 miles. At that time then, very much more remarkable varieties of climate must have existed on the earth than now. Taking the distance when the Earth was furthest from the sun, at mean distance from the sun, and at her least distance, as 14, 13, and 12, the supplies of solar heat when the Earth was so situated were as 11, 13, and 15, or—nearly enough—as 5, 6, and 7. So that when the Earth was in perihelion she received 40 per cent. more heat than when she was in aphelion.

(1098.) The orbit of the Earth is shown in fig. 329 with the perihelion A in the position which it had (see table, Art. 1088) when the maximum eccentricity was attained.[2] It will be understood that the position of the winter solstice is not to be supposed to be where the words are shown in the figure. In the long period, during which the Earth's orbit had the eccentricity shown in fig. 329, the winter solstice of the northern hemisphere passed through the entire circuit of the orbit, with reference to the perihelion at A, and the aphelion at A′.

The excess in the length of the two summer seasons when the winter solstice was at A is shown approximately by the straight lines S *m h* and S *n h′* through *m* and *n* the bisections of C B and C B′. For it is evident that these lines divide the orbit AB′A′B into the approximately equal sectorial areas S *h* A *h′* and S *h* A′*h′*, so that the Earth is

[1] The following table will be found useful, and interesting in many respects, but especially as showing the effect of the outer planets, Uranus and Neptune, on the estimates of the eccentricities, &c. of the other planets; for Lagrange did not take either Uranus or Neptune, and Leverrier did not take Neptune into account;—

SUPERIOR LIMITS OF THE ECCENTRICITIES OF THE EIGHT PRIMARY PLANETS.

	Lagrange	Leverrier	Stockwell
Mercury	0·22208	0·225646	0·2317185
Venus	0·08271	0·086716	0·0706329
Earth	0·07641	0·077747	0·0693888
Mars	0·14726	0·142243	0·1396550
Jupiter	0·06036	0·061548	0·0608274
Saturn	0·08408	0·084919	0·0848289
Uranus	—	0·064666	0·0779652
Neptune	—	—	0·0145066

[2] The table referred to may be supplemented by the following:

Years B.C.	Eccentricity	Years B.C.	Eccentricity
851,000	0·07454	849,500	0·07466
850,000	0·074664	849,000	0·07456

the same time in passing from h through A to h', as from h' through A′ to h. As a parallel through S to B C B′ would meet the orbit A B′A′B at the vernal and autumnal equinoxes, the solstices being at A and A′, it is evident that the duration of the two

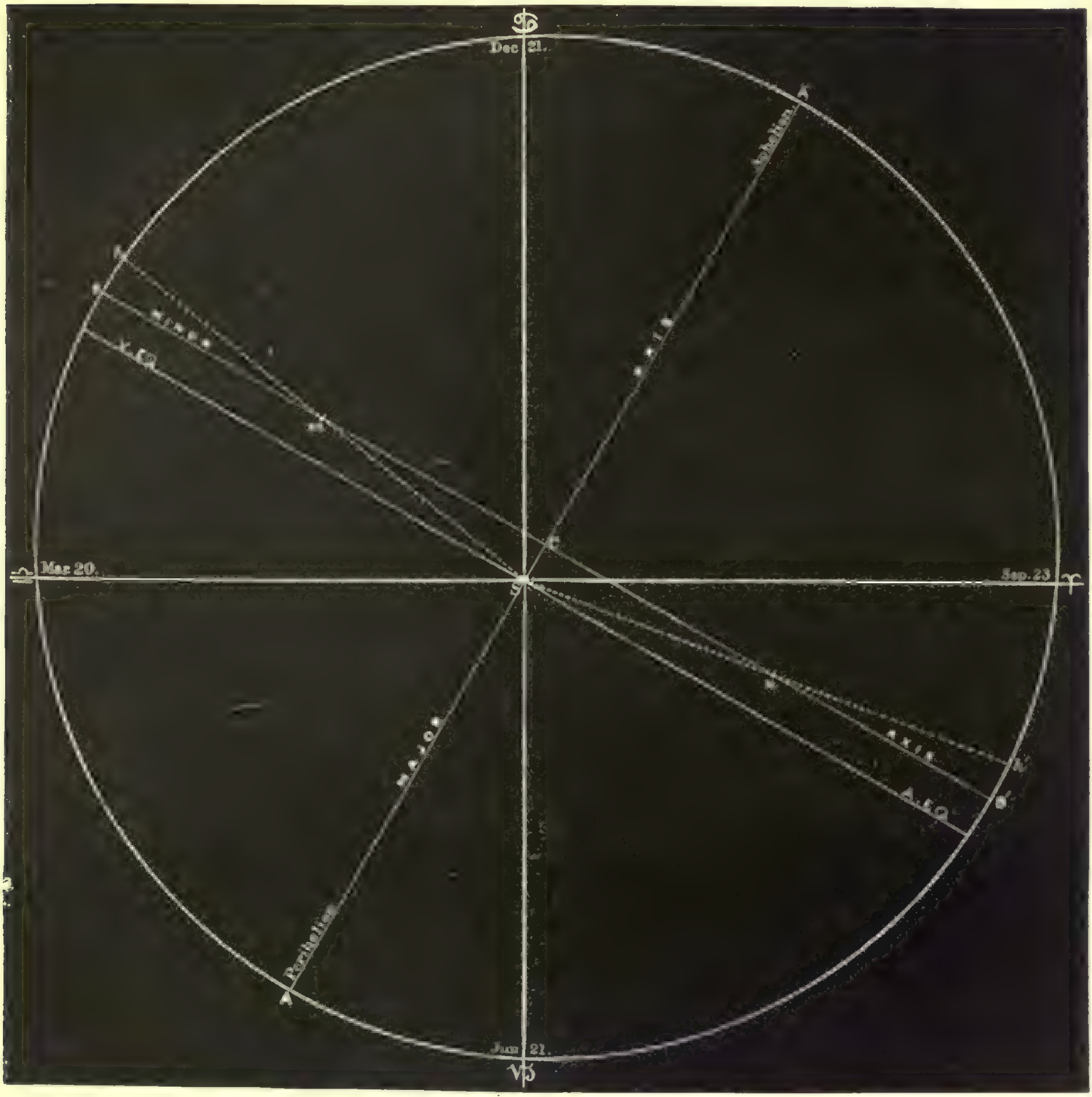


Fig. 329.—Orbit of the Earth 850,000 years before A.D. 1800.
The points h and h' are those reached at the middle of the intervals between the passage from A′ the aphelion and A the perihelion.

summer seasons taken together (when the summer solstice is at A′) would exceed half the year—the time from h to h'—by four times the interval required by the Earth for traversing (when at her mean distance from the sun) the arc B h or B′h', or (approxi-

mately) by four times the interval required for traversing an arc equal in length to S C, the eccentricity (in distance). The duration of the two winter quarters would fall short of the half-year by the same amount. Hence the two summer seasons, that is, the interval from the spring equinox to the autumnal, would exceed the two winter seasons by eight times the interval in which the Earth, when at her mean distance, traverses a distance equal to the eccentricity of her orbit. Now the Earth when at her mean distance traverses now as always (let the eccentricity be what it may) a distance of about 1,596,000 miles in a day; so that, since S C represents (Art. 1097) a distance of 7,180,000 miles, the two summer seasons exceeded the two winter seasons, when the perihelion coincided with the winter solstice, 850,000 years ago, by as many days as 1,596,000 is contained in eight times 7,180,000—that is by nearly 36 days. The real difference would be about 35 days according to Mr. Croll's table; but it must be remembered (Art. 1096, note) that the larger eccentricities in that table are for the most part in excess of the true values. Probably an eccentricity of only about 0·068 was attained by the Earth's orbit about 850,000 years ago. Fig. 329 would equally represent the Earth's orbit with that eccentricity; but the greatest difference between the two summer seasons and the two winter seasons would be reduced to about 32 days. Considering, however, the effects resulting, so far as we can judge, from the Earth's present eccentricity of only 0·0167, (the perihelion being near the winter solstice) and a time difference of fewer than 8 days, we are justified in attributing very marked effects to the eccentricities at the times marked M, in Mr. Croll's table, p. 468, even after due account has been taken of the reduction which would result from the use of the full formula and the consideration of terms neglected by Leverrier and by Lagrange.

(1099.) The effect of the varying eccentricity of the Earth's orbit on the total amount of light and heat received by the Earth in each circuit is worth noticing. The mean distance of the Earth remains all the time appreciably the same. Hence the area of the ellipse described round the sun varies directly with the minor axis, which decreases as the eccentricity increases, and *vice versâ*. The smaller the area of the path described around the sun, the greater is the amount of heat and light received from him. It may be shown that this variation is truly inverse; in other words the heat and light received by the Earth in a year are inversely proportional to the minor

[1] The proof of this, though not difficult, is not so simple as some writers appear to imagine, who have quoted the law—first noticed, I believe, by Sir John Herschel—as if it were obvious. For anything such writers could perceive to the contrary, the lingering of the earth near aphelion and her rapid motion near perihelion might neutralise or more than neutralise the effects due to the circumstance that the area of her orbit around the sun is less when the eccentricity is greater.

The law may be thus established:—

The light and heat received by a planet vary inversely as the square of the planet's distance from the sun. From the equable description of areas, it follows that the momentary angular velocity of a planet about the sun also varies inversely as the square of the planet's distance from the sun. Thus, the light and heat momentarily received by a planet vary directly as the planet's angular velocity about the sun. It clearly follows, therefore, that the light and heat received by a planet in any time are directly proportional to the angle swept out about the sun in that time.

Now compare the heat received by two planets (which let us call P and P′) revolving in orbits of different eccentricities, but whose major axes are equal. Let the minor axes of the two orbits be respectively b and b', b being greater than b'. Then, since the periods of the two planets are equal (for their mean distances are equal), it follows from the equable description of areas about the sun, that the area swept out by P in any time : the area swept out by P′ in the same time : : the area of P's orbit : the area of P′'s, or (by a well-known property of the ellipse) : : $b : b'$. Suppose both planets to be at their mean distances from the sun, that is, suppose they are equally distant from the sun; then the area swept out by P in

axis of the terrestrial orbit.[1] But as the minor axis varies very slightly even when the eccentricity varies measurably, the total supply of light and heat received by the Earth from the sun in a year varies little from era to era so far as the eccentricity of the Earth's orbit is concerned. Even with so exceptional an eccentricity as 0·07 (see Arts. 1096 and 1098) the ellipticity of the Earth's orbit would be less than 0·00245 (see last note), or the minor axis ·9982, the major axis being unity.[1] Thus the annual amount of light and heat received by the Earth would exceed what she would receive if the eccentricity had its least possible value, by less than $\frac{245}{100,000}$ or $\frac{1}{408}$ of the total annual amount.

(1100.) The effect of changes in the obliquity of the ecliptic in modifying the climate of the Earth or of either hemisphere must be regarded as measurable though not important. The obliquity of the ecliptic changes very slightly. We must not, as some have done, confound changes in the inclination of the ecliptic to the mean plane of the solar system with changes affecting the inclination of the plane of the equator to that of the ecliptic. Changes of the former kind, which are parallel in character to changes in the eccentricity, though they do not synchronise with them, may shift the plane of the ecliptic about 4° 51′ on either side of its mean position. But (as was first shown by Laplace) the equator-plane shifts in company with the ecliptic, so that the obliquity of the ecliptic does not vary (equally) with the inclination of the ecliptic to the mean plane of the solar system. Laplace, using the estimated masses of the planets known to him, found that the obliquity of the ecliptic may oscillate 1° 22′ 34″ on each side of 23° 28′, the value of the obliquity in 1801. But Mr. Stockwell has obtained more exact values (see 'Smithsonian Contributions to Knowledge,' vol. ix.): he finds, taking all the planets into account and using the best modern estimates of their masses, that the obliquity of the ecliptic ranges 1° 18′ 41″ on each side of the mean value 23° 17′ 17″, so that it can never be greater than 24° 35′ 58″, or less than 21° 58′ 36″.

(1101.) Changes in the position of the Earth's axis of rotation have been suggested as having possibly changed the climate of parts of the Earth: but it has been shown by Mr. George Darwin that no such alterations in the position of the land masses of the Earth as geologists can regard as possible would affect the position of the polar

any time : the area swept out by P′ in the same time : : b : b'; but considering the motion of each for a very small interval of time, it follows from the equality of the distances that the area swept out by either varies as the angle; hence, the angle swept out by P in any very small time : the angle swept out by P′ in the same time : : b : b' (P and P′ being each at their mean distance). In this time P and P′ receive equal amounts of heat from the sun, since they are equally distant from him; hence, from what was shown in the last paragraph, it follows immediately that the heat received by P in a sidereal revolution : the heat received by P′ in a sidereal revolution : : b' : b,—

For,—Let h be the heat received by P and P′ in the short interval of time considered; a and a', the angles respectively swept out by P and P′ about the sun in that time; H and H′ the heat respectively received by P and P′ in a sidereal revolution: then from what was shown in the preceding paragraph, it follows that—

$$\mathrm{H} : h :: 4 \text{ rt. angles} : a ;$$
$$\text{and} \quad h : \mathrm{H}' :: a' : 4 \text{ rt. angles};$$
$\therefore$ (*ex æq. in prop. pert.*)
$$\mathrm{H} : \mathrm{H}' :: a' : a :: b' : b.$$

Or generally, the major axis of a planet's orbit remaining unaltered, the light and heat received in a sidereal revolution vary inversely as the minor axis of the planet's orbit.

[1] The following way of determining the ellipticity of an orbit from its eccentricity e, when e is small is convenient. We have, if the major axis is unity (see Art. 1070)

$$\text{Minor axis} = (1 - e^2)^{\frac{1}{2}}$$
$$= 1 - \frac{e^2}{2} + \text{terms which involve } e^4,$$

and higher powers of e, and may, therefore, be neglected.

Thus let the eccentricity of the Earth's orbit at any given epoch be 0·07; then (eccentricity)^2 = 0·0049; the minor axis = 1 − 0·00245 = ·99755. [The ellipticity = 0·00245.]

axis by so much as 3°. One-tenth of the Earth's whole surface would have to be raised 10,000 feet in one quarter of either hemisphere, and an equal portion similarly depressed in another, to shift the pole 3° 17′; one-twentieth instead of one-tenth would only shift the pole 1° 46′; and, as Sir W. Thomson well remarked of such changes, even though a more indulgent sky might thus be brought to the American Arctic Archipelago, Novaia Zemlia and Siberia would be brought correspondingly nearer to the pole; and there is 'as much need of accounting for a warm climate on one side as on the other side of the pole.' [1]

(1102.) We have been led, in dealing with the changes which have taken place in our Earth's orbit, to consider periods of time so vast that what formerly was regarded as the whole duration of the universe seems but as an hour, nay, but as a second, by comparison. The study would be of little interest were it merely speculative. We might trace the Earth's motions into past and future time through hundreds of thousands of years, and yet suggest no soul-stirring thoughts, did not the Earth herself bear witness that she has existed through those remote past periods and the continuity of events lead us to expect that she will exist during kindred periods yet to come.

(1103.) It is here that the study of our Earth becomes most interesting and instructive to the astronomer. She is the one member of the solar system we can study in detail. We can not only learn what she is now, but what she has been in the past. What we have learned about her, we have learned in some degree about the other planets also. There may be, nay, there must be, differences in planetary life-histories due to differences of mass, size, position, age, and so forth, precisely as there are differences in animal and vegetable life-histories. To imagine resemblance between planet-lives in points of detail would be unwise. To pretend even to determine the exact nature of differences in points of detail would be rash. But in its general character the life of one planet must be akin to that of another, as the life of one animal is akin to that of another, the life of one tree akin to the life of other trees. And in what the Earth has taught men, within the lifetime of many now living persons, of the immense duration of her past, we have one of those general characteristics which we must regard as common to the lives of all planets.

(1104.) That the Earth's history in the past must be measured by millions of years, we know from the study of that Great Bible of Nature whose leaves are the layers and its volumes the strata of the Earth's crust. We can safely infer that apart from catastrophic change, of which there is

[1] Mr. Geo. Darwin has shown that if the whole equatorial regions to latitudes 45° N. and S. were sea, and the water to the depth of 2,000 feet were removed to the polar regions in the form of ice, the arctic circles would not be shifted by so much as a single inch; yet such a redistribution of weight would be the most favourable possible for affecting the obliquity of the ecliptic.

no sign of likelihood, the Earth's future as a living planet will continue for millions of years to come. Other planets also, then, have their lives of kindred length. The duration of some as living orbs may be measurable by but millions of years, the lives of others may last for tens of millions, and some may endure for hundreds of millions of years. The youth of one may synchronise with the middle life of another, and with the old age of another, or even continue long after yet another has passed to its death. But that the lives of all the chief orbs of the solar system, and of its fellow-systems through space, must be akin to the Earth's life in duration, in such sort that the life of a man or of a nation, or even of the whole human race, is but as a breath by comparison, we may most confidently believe.

Unspeakably solemn as well as thus instructive are the lessons taught by the Earth's strata in regard to time. Astronomy, which tells of vaster regions of space, speaks also of time-intervals far surpassing even the millions of years over which the geologic and biologic records extend. But astronomy has no tangible record of the vast periods of time of which the cyclic movements of the heavenly bodies speak. The geologist can actually handle the material produced in past ages by processes which he sees now at work. The biologist has in like manner direct evidence of the progress of development in varieties, species, and genera of animal and vegetable life. Each has material evidence of the millions of years of which he speaks, whereas the astronomer, though he may ascertain practically that the sun, moon, and stars have existed for even vaster periods, can give no direct evidence for the faith that is in him.

(1105.) How first the Earth's crust was formed we know not, for the traces of the first formative processes have long since been swept away. Probably each portion was formed again and again, solidifying and melting, and resolidifying and remelting, many times over before it finally assumed the solid condition. In some cases the solid part of the crust, being of greater specific gravity than the molten, would sink as fast as formed, to melt again, and again return to the surface, not finally solidifying in a permanent way until many hundreds of thousands, perhaps millions, of years had passed.

Whether any part of the present crust of the Earth can be regarded as representing the solid crust, fashioned when first these alternations were completed, may well be doubted. It is not probable that the Archæan rocks formed the primeval crust of the planet. A vast interval may have separated the formation of these, the most ancient rocks of the present Earth, from the first formation of a crust upon that region of the Earth where the Archæan rocks are found. Whenever the Archæan rocks, as actually studied by geologists, were formed, they have since undergone great changes; some of the Archæan rocks now exposed were for ages buried beneath strata formed later, and so underwent pressures and changes of temperature largely affecting their condition. It must be remembered also that the lower primary rocks originally deposited on these

Archæan rocks were formed from them by processes of denudation, so that we cannot expect to find anywhere the ancient face of the first-formed crust. Denudation, too, in those remote times, certainly 10,000,000, and probably more than 20,000,000 years ago, was a very much more active process than denudation as it takes place now. For the waters of the ocean were greater by probably one-half in quantity, were intensely hot, were loaded with destructive acids, were more actively moved; while the air was not the pure life-nourishing air we breathe now, but an atmosphere whose breath was fire laden with destructive vapours, swept by tremendous storms, and bearing clouds from whose bosom descended torrents of hot water, mixed with sulphuric acid, boracic acid, and other powerfully acting liquids. By these destructive agencies the first formed crust of the Earth must have been rapidly denuded, and fresh layers formed at the lower levels, to be raised while the higher levels were depressed, fresh denudation following, and fresh layers being formed, probably during millions of years, before life was possible upon the Earth.

(1106.) Passing from the Archæan ages, we come upon the beginning of those periods of which the history is recorded, though but imperfectly, in the Earth's crust. Few even among those who have studied the geologic record appreciate fully the vast periods of time to which it bears witness. In the first place we are apt to overlook the evidence attesting the vast age of the Earth. But in even more marked degree we overlook, in the second place, the signs which show how much of the Earth's record has been destroyed. It seems strange that even the least observant in our age, or that any races of men in the past, should overlook the evidence attesting the vast age of the Earth. Mountains and valleys, hills and dales, even knolls and ravines, most clearly announce the amazing antiquity of the Earth's crust. Along sea-shore and river-shore are the signs of tens and hundreds of thousands of years of past Earth-life. To the student of geology alone, no doubt, it is given to read the lesson in all its details. He alone can confidently say, this layer speaks of hundreds of thousands of years, that formation of millions, that series of strata of many millions of years. But no one who sees, and thinks of what he sees, can fail to recognise clear evidence of hundreds of thousands of years in the Earth's crust, even as disclosed to the least scientific observer.

(1107.) The beetling crag speaks in its weather-worn face of tens of thousands of years to the geologist, for he knows how slowly the work of denudation proceeds, and how long it must have required to carve that rugged mass into the form we see. Yet a thoughtful observer can see that thousands of years of weather-work and sea-work are recorded there. Looking a little closer, he sees that the rock thus worn away is formed of layers which manifestly were themselves the products of denuding forces, for he can see in them sand and pebbles and sea-shells. Recognising the height of the cliff, and therefore the depth of the originally deposited layers, he sees that tens of thousands of years must have been required for the deposition of all that mass of sedimentary matter. The geologist, better acquainted with the processes involved, may recognise hundreds of thousands of years as represented by that work of deposition. But the ordinary observer, even in his first general view of the cliff's structure, sees enough to impress his mind with the sense of vast time-intervals. Looking closer, though he may not know even the bare names of the substances he sees, he recognises yet stronger evidence of the past progress of time; for he perceives among the masses embedded in the layers of the cliff before him some which manifestly were broken off from shores formed of materials deposited, layer by layer, in remoter periods still.

(1108.) More wonderful by far, however, than even what the Earth reveals as to her age, through the evidence of strata actually in existence, is what she tells about the missing leaves and chapters of the mighty volume.

The very evidence which remains speaks of a much vaster body of evidence which has been destroyed. When we see layers whose total thickness must be measured by hundreds of yards, we should no doubt recognise the long periods of time during which those layers were being deposited. But those layers speak of evidence destroyed quite as clearly as of evidence gathered ; and the evidence destroyed must have been far greater in amount.[1]

(1109.) Whatever the actual state of the Earth's crust before the primary rocks, with the first records of life upon the Earth were formed, all the existing rocks, primary, secondary, tertiary, and recent, were formed out of that ancient crust. So that all the evidence which the various strata of the Earth's crust afford tells not only of constructive but of destructive work, and therefore of an equal amount of preceding constructive work whose records have been destroyed. This enormously increases our estimate of the Earth's duration in the past. But even this is far from being all. Many of the Earth's strata speak of destructive and constructive processes many times repeated. A recent stratum, in so far as it includes material derived from earlier strata, tells of formative processes of which the direct evidence has been destroyed. It may have been derived from preceding strata (and through more changes than one) of recent origin, these from tertiary strata, these from secondary strata, these again from primary strata (possibly through many tertiary, secondary, and primary strata), and these from the original Archæan crust out of which all the strata were formed either directly or indirectly.

(1110.) With regard to the significance of the evidence which the history thus imperfectly recorded affords in regard to the past duration of the Earth, let the following brief summary suffice :—

The last glacial era, whose records are left over large tracts of the continents of Europe, Asia, and North America—tracts not only glacierless now, but where no glaciers have been known for thousands of years—must be regarded as belonging to the yesterday of geological time. Those who take the lowest view of the time which has elapsed since that era regard it as belonging to the era of great eccentricity of the Earth's orbit, which the table, Art. 1088, shows to have come to an end about eighty thousand years ago. Others would go much farther back ; but to whatever epoch we assign the last glacial era, it belongs but to the last turned page of our Earth's history. The quaternary period of alternate glacial and non-glacial eras must have lasted hundreds of thousands of years. The tertiary period cannot have lasted less than a million years. The secondary must have lasted much longer than the tertiary. It still occupies a more important place in the geological record, though it has been

[1] We might compare the record given by any layer of the Earth's crust to a palimpsest of Augustine on a parchment which had borne a history written by Livy : a palimpsest telling us of what is ancient, but only rendered possible by the concealment of a record much more ancient still. But such a comparison would be far from fully illustrating the immensity of the periods whose record in the Earth's crust has long since been destroyed, as compared with those periods whose record remains. If the history of Livy had been itself a palimpsest over Sibylline prophecies and these over Babylonian, and these again over Accadian records, themselves written over marks scored on the original parchment by shepherds living thousands of years before the Accadian alphabet had been invented, something like the vast antiquity of the periods whose Earth-records have been used up to make the layers of a modern cliff would be suggested.

exposed to much greater wear-and-tear, and a much larger proportion of leaves have been rent from it. Of the primary formations we know even less, yet of their duration we can assert even more. Incomplete as records, their very incompleteness tells more than the fuller record of tertiary and quaternary formations. Professor Ramsay, speaking only of a few of the upper or more recent primary formations, and of the lowest of the secondary series, remarks that 'the local continental era, which began with the Old Red Sandstone and closed with the New Red Marl, is comparable in point of geological time to that occupied in the deposition of the whole of the Mesozoic or secondary series, later than the New Red Marl, and all the Cainozoic or tertiary formations, and indeed of all the time that has elapsed since the beginning of the Lias down to the present day.' 'We possess,' says Darwin, speaking of the Lower Cambrian rocks, 'the last volume alone, relating to two or three countries.' It is probable that the whole time represented by the fossiliferous rocks, from the earliest Cambrian to the most recent, has been short compared with that which preceded it in the life-history of our planet, and even these preceding ages were probably far surpassed in duration by the ages during which the first formative processes, before life began upon our Earth, were in progress.

(1111.) The biological evidence respecting the Earth's past duration, though it can tell us nothing of ages preceding the beginning of life, is even more striking than the record of the rocks, regarded merely as the products of physical processes. Now that the general law of the development of life is recognised, the teaching of the biological record has become unmistakable, its evidence decisive. Starting we know not how, coming we know not whence, the stream of life, animal and vegetable, which has flowed over the Earth during millions of past years, has left traces of its existence in all the rock strata of the Earth from the Palæozoic (primary), through the Mesozoic and Cainozoic (secondary and tertiary) periods, onward to the Pleistocene, and to those strata which are called recent, though some of them, measuring their duration as men measure time in considering the progress of races and of nations, seem of vast antiquity.

(1112.) The existence of fossil traces of life, animal and vegetable, in the lowest of the Palæozoic rocks affords decisive evidence that immense periods of time must have separated the formation of the rocks next below them from the first formation of the Earth's solid crust. For it is certain that during millions of years following the solidification of the Earth's crust life must have been impossible.[1] We may safely conclude that the Archæan rocks represent formations which had no existence with their present structure until millions of years after the Earth first had a solid surface.

(1113.) We may deduce the same conclusion from the character of the fossils found in the lowest of the primary or Palæozoic rocks. We know that whatever may have been the actual beginnings of animal and vegetable life on the Earth, the flora and fauna even of the lowest Palæozoic strata cannot have come into existence save as the product of immense periods of past time. In the Lower Silurian rocks, indeed, we have but few fossil traces of the vegetation of the remote time when they were formed; but this is mainly due to the fact that the Lower Silurian rocks are chiefly

[1] If the formation named *Eozoon* found in the Archæan limestones of Canada is to be regarded as the fossil evidence of a reef-building animal (a Foraminifer), we must throw back still farther the time of the first solidification of the Earth's crust.

of marine formation. But the animal life, even of the oldest stratified rocks, suffices to decide the question of duration in the most emphatic matter. The record is doubtless incomplete; but in its very incompleteness it is the more decisive. We find complex and advanced forms, while the simpler forms, which necessarily existed at the same time, have left no trace of their existence. We can understand, then, how all forms of life, simple and complex, animal and vegetable, belonging to earlier strata, have disappeared, leaving no fossil evidence. And the positive evidence given by the fossils we do find, while thus giving to the negative evidence its true meaning, has its own definite and unmistakable value.[1]

(1114.) But the Silurian rocks (the lowest and most ancient of all rocks known to have been life-bearing) were not without vegetable life, and the fossils telling us of the existence of vegetable life show also that immense periods of life must already have passed before that life began.[2] Some traces have even been found of the denizens of those ancient thickets. Remains of scorpions were discovered in the Silurian rocks of Sweden, Scotland, and the United States almost simultaneously in 1884; while in those of France, also in 1884, the remains of an ancient cockroach were found. Where scorpions and cockroaches abounded, not only must there have been many other forms of land animals, but animal life must have existed on land during hundreds of thousands of years. For creatures so complicated in structure as the *Scorpionidæ* and the *Blattidæ* cannot have been developed save in vast periods of time.

(1115.) In these early stages of Palæozoic time we find evidence not only of multitudinous life, but of many forms of life. Still within the Silurian era we find vertebrate forms in fishes akin to the modern sharks, the sturgeon, and the garpike. (The catfish shares with the cockroach the honour of descent from Silurian times.) Onward through Devonian time vertebrate life presents itself still only among fishes, but now in ever-increasing variety and numbers. New forms of crustaceans—the Eurypterids—having affinities also with the Arachnidæ (scorpions, &c.), began to replace the trilobites, from which probably they had descended, though after transformations requiring immense periods of time.

(1116.) Many other forms of life existed also throughout the Devonian period, while an abundant vegetation spread over the land, traces of no fewer than a hundred different species of plants having been discovered in the Old Red Sandstone. True

[1] If we consider, for example, but a single class—say the Trilobites (now extinct, though transiently represented in the early life-stages of the King Crab)—we have as decisive evidence of enormous preceding time-intervals as if we had fossils of 10,000 species of Silurian life, animal and vegetable. Those strange crustaceans existed at the remotest time to which geological history looks back, in many different forms. All through the Silurian period they continued to thrive and multiply and develop into various forms. But as the millions of ages represented by the Palæozoic strata passed on, evolution in the race of trilobites resulted in the development of other races better fitted for the changed conditions of life; we find no traces of any trilobites later than the Carboniferous age; and in that age we find only four, all of which were small. Doubtless many others existed, of which no fossil traces have been left; and doubtless, also, trilobites continued to later times than the fossil evidence attests.

[2] In the Upper Silurian strata we recognise the spores and stems of flowerless plants (*cryptogams*). Club mosses and ferns were particularly conspicuous in the flora of those Palæozoic times. The cones and spore cases of the club mosses and the tough tissues of the ferns were well fitted to withstand destructive agencies, yet few even of these have remained in fossil form. Doubtless many other forms of vegetable life existed, whose traces have long since entirely disappeared. The wonder is, considering the immense remoteness of Silurian time, that any trace whatsoever, either of animal or vegetable life, should have reached our time.

conifers now began to appear, or at least fossil traces of such trees are for the first time recognised in Devonian rocks. The forests must have been uniformly green, for millions of years were to pass before deciduous trees were to appear. Many forms of insect life existed in those evergreen forests; fossil wings of insects have been found which seem even to indicate by their size an exceptional wealth of insect life, for races attain large size only under favourable conditions. Thus, among several forms of Mayfly we find one whose wings must have had a spread of fully five inches! Strange, be it noted in passing, to find traces thus ancient—certainly not less than 10,000,000 of years old—of a creature whose very name—the *Ephemeron*—means the creature of an hour, so short is its actual life, so slight its seeming hold upon existence.

(1117.) In the Carboniferous strata, which in some places attain a thickness of nearly four miles, we find evidence of an amazing wealth of vegetation, uniform in general character—for as yet the Earth seems to have had no seasons, so far as heat was concerned—but very varied in details. The most characteristic peculiarity of animal life was the prevalence of amphibia, represented chiefly by immense creatures resembling the modern salamander, but attaining a length of seven or eight feet. Enormous sharks swept through the seas of the time, armed with teeth capable of crushing the strong incasing armour of the ganoid fishes which probably formed their prey. As the Permian era came on, the flora and fauna changed by slow processes of development, both still retaining, however (over the whole Earth, so far as can be judged), their tropical character.

(1118.) We find so marked a contrast, both in the flora and fauna, in passing from the Permian system to the Triassic—that is, from the highest of the primary to the lowest of the secondary strata—that had not experience taught us to recognise in such marked change merely evidence that many leaves are missing from the geological record, we might be tempted to believe, with the geologists of former times, that the primary forms of life had been for the most part replaced by new creations. All that geologists and botanists now infer from the great change in many forms of animal and vegetable life is that immense periods of time passed after the last of the Permian strata were deposited before the first of the Triassic rocks began to be formed. The records of these vast time-intervals have been destroyed by denudation. Some, however, of the old genera of plant life and of animal life still remained. Conifers, which had existed in the previous era, were now more numerous and in greater variety. But Cycads were the predominating form of vegetable life throughout the Mesozoic period, which has been called on this account the 'Age of Cycads.' Amphibians now increased in numbers, while lizards made their first appearance (so far at least as the geological record attests).[1] It was in the long-lasting Mesozoic or second-

[1] Deinosaurs, which may be regarded as a connecting link between birds and reptiles, appeared and disappeared during the Mesozoic era—becoming extinct, like other transitional types, within a comparatively short time, though the absolute duration of their existence on the Earth may probably be measured by hundreds of thousands of years. The footprints of some of these creatures, which walked on their hind legs, were mistaken by the earlier geologists for the traces of gigantic birds; but although birds, and gigantic ones, appeared during the Mesozoic ages, the deinosaurs were not flying creatures. When we consider the enormous size of some of them, as the *brontosaur*, whose feet left imprints a square yard in area; the *stegosaur*, whose bony back-plates were three feet across; and the *atlantosaur*, the most massive of all known creatures, probably of all creatures which have ever existed (it seems to have been about 100 feet long and 30 feet high), we may regard the power of flying as not one which deinosaurs needed or were likely to possess.

ary ages, that mammals first made their appearance, and began to obtain dominion over the Earth, though the traces of them are but few and far between.[1]

(1119.) The Tertiary, or Cainozoic, periods were in like manner supposed to be separated by a distinct line of demarcation from the Secondary, or Mesozoic. The cretaceous system, or chalk, was found to be in many places succeeded by beds of pebble, sand and clay, of entirely different character from any of the chalk formations. In these upper beds no fossils could be found which had been recognised in the chalk. But researches, at once wider and more detailed, showed that parts of the leaves which seemed thus to be missing exist elsewhere. The break in the continuity of deposits in some places shows only that denudation had either completely removed the missing strata before the higher beds began to be deposited, or else in certain regions no strata were deposited.

(1120.) Yet on the whole we find a marked change in the Earth's aspect in Cainozoic times, as well as a characteristic difference in the manner in which the crust of the Earth behaved. During the Tertiary period the continents of the Earth were fashioned nearly into the forms they have in our own time. Processes of contraction affecting a crust which, owing to increased thickness and diminished plasticity, no longer yielded easily to the pressures and strains acting upon it, resulted in the formation of the great mountain ranges, by the upheaval (through side pressures) of the thick and deep strata formed during the primary and secondary periods upon the original crust. Parts of what was sea-bed at the beginning of Tertiary time are now found three miles above the sea-level; and doubtless other portions were raised even higher, but have been carried down from the positions so reached, by the action of the denuding forces which have carved the peaks and pinnacles of mountain ranges until in many parts the inner Archæan core has been exposed.

(1121.) In the Tertiary strata we recognise first the signs of a diversity of temperature beginning to exist in different parts of the Earth. Early in Tertiary time, indeed, even the arctic regions had a mild climate, but towards the close of the long-lasting periods of the Tertiary age snow and ice had spread, not only over the whole of the arctic regions, but even over parts of the European and American continents which at present are free from them. This, however, must be regarded as indicating only a temporary extension of the northern snows and ice. Even during eras belonging to the close of Tertiary time the Earth possessed on the whole a warmer and more equable climate in high latitudes than she has now.

(1122.) The vegetation of the Earth now began to resemble closely the vege-

In this age, also, the great sea saurians throve, multiplied and died out. The ichthyosaur, with eyes a foot in diameter; the long-necked plesiosaur, the pythonomorphic (or serpentine) saurians, of which no fewer than forty varieties have been recognised, some of them being more than 75 feet in length, were among the denizens of the sea in Mesozoic time.

This was the age also of those bat-winged reptiles, the Pterosaurs, some of which were of enormous size. But these again were probably transitional forms, and are now extinct. The birds of the Mesozoic ages, which show many reptilian characteristics, represent more successful lines of development; and though none of the birds known to belong to Mesozoic times remained in later ages, the birds even of our own time afford in their structure abundant evidence of their descent from those earlier birds, or from their contemporaries.

[1] Teeth of a small marsupial animal akin to the banded ant-eater of New South Wales are found as low as the Triassic strata (the lowest of the secondary formations), while in the Jurassic, or next higher system, other forms of insectivorous marsupials are found, along with one which Owen regarded as an herbivorous placental mammal, and another which he regarded as probably carnivorous.

tation of the present day. Nearly all the genera of later Tertiary vegetation still throve on the Earth. Animal life began also to resemble the animal life of to-day much more closely than during preceding eras. Mammals not only made their appearance on the Earth, but, as usual with successful incoming types, they showed at the outset of their career a richness and fulness of development such as they do not present in these times. The pachyderms, still the largest of the land mammals, were much larger in Tertiary times than now. In Tertiary ages, also, gigantic cetacean sea mammals, the ancestors of the whales, dolphins, and kindred races of our own times, gradually took the place of the monstrous sea saurians of preceding ages.[1]

(1123.) In considering the flora of the Eocene period, we are chiefly struck by the evidence it affords of the extension of a climate still tropical over regions now temperate, and of a climate still warm over regions now intensely cold.[2] Plants now only found in the hotter parts of Asia, Africa, America, and Australia throve then in Canada, Scandinavia, and Siberia. Ferns and evergreens were numerous, but many deciduous trees—elms, hazels, willows, planes, chestnuts, &c.—had now made their appearance. The fauna of this period also indicates a generally tropical climate extending over the temperate zones, and a temperate climate extending to far within the arctic regions.[3]

(1124.) Throughout the closing part of the Eocene period, called sometimes the 'Oligocene' (or 'Few-recent'), there was a general though slow progress towards the

[1] The division of Tertiary time into Eocene, Miocene, and Pliocene periods indicates the recognition among geologists of the growth and development of modern forms of animal and vegetable life during the Cainozoic eras. For these names imply simply 'Dawn of Recent,' 'Less Recent,' and 'More Recent' ('forms of life' being understood). The lower Tertiary strata are called Eocene to show that recent forms of life begin to be recognised in those strata; in the Miocene strata recent forms of life are more numerous than they had been, but still not so numerous as the ancient forms; while in the Pliocene strata recent forms have not only increased in number but they now exceed the ancient forms, and in gradually increasing degree, till as we are passing from the upper Tertiary to the lower Quaternary or recent strata, we have to change our descriptive term from Pliocene, or more recent (than ancient), to Pleistocene, or mostly recent.

[2] It has been objected that the Earth's internal heat, though it might account for the existence of what we now call tropical life in the temperate regions, could hardly explain the existence of various forms of tropical vegetable life within the arctic regions, since such forms of vegetation could not endure the long periods of darkness which prevail each year within the arctic circle. But Sir Joseph Hooker has pointed out that palms and other tropical plants brought from the tropics survived the winter in St. Petersburg without damage, though matted down in absolute darkness for more than six months. Probably also, with the more widely extending and denser atmosphere of the Secondary and early Tertiary periods, a diffused light would be present even in high arctic latitudes, producing strong midday twilights. The aurora borealis may also have been intensely luminous in those younger days of the Earth.

[3] Reptilian life was no longer so preponderant as during Mesozoic time, the reptiles still thriving in the Eocene period being chiefly turtles, tortoises, and crocodiles, closely resembling those now existing. Remains of birds are found more freely in Eocene strata than in the lower formations, though avian fossils are naturally not abundant in any strata, the power of flight saving birds from most of those forms of death which favoured the preservation of fossil remains. True mammals now made their appearance in great numbers. Small pony-like animals appeared—the ancestors probably of the horse, ass, zebra, and quagga, but differing from the modern equine races in possessing most of the toes of each foot, whereas the Equus of to-day possesses only the middle toe complete, the side toes being represented only by rudimentary splints. Hogs of various kinds, deer and antelopes, squirrels, lemurs, and bats had now appeared. Races also now seen for the first time, but not destined to last to our own day, throve and multiplied during the Eocene period—creatures (the tinoceras and deinoceras) like the rhinoceros in structure, but having six horns instead of two, and like the elephant in size, were now the most powerful denizens of the forest.

condition found during Miocene time, during which the flora and fauna showed a marked advance towards the characteristic forms of recent geological time.[1]

(1125.) In Pliocene times, as the name implies, modern forms of vegetable and animal life had become still more common. Tropical types of vegetation were no longer found in the higher temperate latitudes, and the forms gradually approximated more and more in character to those now occupying the corresponding regions. The fauna also presented similar characteristics.[2]

(1126.) No definite dividing line can be drawn between the Tertiary and Quaternary periods. The change from Eocene to Miocene, or from Miocene to Pliocene, corresponded closely in character with the change from Pliocene to Pleistocene. Ever since the time when life, vegetable or animal, had first appeared on the Earth, multitudinous forms of life had come into existence, had risen into greater or less prominence according to their surroundings, and had in some cases died out, in others had developed succeeding races more or less closely akin to them, and in yet others had continued scarcely changed even throughout the millions of years which separate the beginnings of the Palæozoic periods from our own time. The steady advance of the stream of life, with its various waves thus either dying out, or merging into other waves, or progressing scarcely changed age after age, had gradually led to the development of more and more of those forms of vegetable and animal life which we regard as belonging specially to the present age of our Earth's history. For gradually the Earth's surface had changed from a condition utterly unlike what now exists to nearer and nearer resemblance to its present aspect. At the beginning the internal heat had extended its influence over the whole surface of the Earth, throughout the whole ocean, even throughout the whole atmosphere; and the stream of life on the Earth had corresponded in character to this uniformity of thermal condition. But as during millions of ages the internal heat gradually diminished, the Earth began, as it were, to recognise more clearly age after age the influence of the central sun, one day to become supreme in determining the conditions of life. Throughout the later Palæozoic, the earlier and later Mesozoic, and even the earlier Cainozoic periods, the sun's influence was small in determining differences of condition between different parts of the Earth. But from the middle of the Tertiary era onward to the beginning of the era which we recognise as recent—though in reality it has already lasted some 200,000 or 300,000 years—the influence of the sun has in this sense been so paramount, that it has divided the Earth into climates, corresponding generally,

[1] Still, however, even in the Miocene age, the forests which adorned temperate regions resembled rather those found in India and Brazil than the forests of middle Europe and other such regions now. Beeches, laurels, oaks, and poplars, as well as magnolias, myrtles, sumachs, mimosas, and acacias, were now abundant. Through the forests ranged giraffes, deer, antelopes, three-toed horses, wild cats, bears, sabre-toothed lions, monkeys, and apes. The deinotherium and mastodon were doubtless, however, the most powerful land animals; the deinotherium, as large as the elephant, with two immense tusks in the lower jaw, curved somewhat like those of the walrus; the mastodon, a form of elephant, but in some cases armed with four tusks, two in the lower as well as two large tusks in the upper jaw. The forms of insect life also were particularly rich, especially the wood beetles, which attained often singularly large dimensions. Frogs, toads, lizards, and snakes were also numerous in the Miocene period. Large cetaceans traversed the Miocene seas: their ear-bones are found in considerable numbers in the raised sea-beaches belonging to this part of Tertiary time.

[2] Tribes of animals roamed over Europe in the earlier portions of the Pliocene period which are now found only on the southern side of the Mediterranean. In England, even in the latter part of Pliocene time, the hyena, rhinoceros, elephant, and other animals, now limited to tropical or sub-tropical regions, must have been numerous to account for the frequency of their remains.

though not exactly, with zones limited by various latitudes—that is, with zones within which the sun's direct influence as measured by his midday height in different parts of the year has such and such definite range.[1]

(1127.) So soon as we have recognised, both from the geological and from the biological evidence, the vast duration of the Earth in the past, we perceive that in considering the Earth's future we must take into account correspondingly vast intervals of time. We have not to inquire what our Earth will be like a few hundreds, or even many thousands, of years hence, but what will be the effect of changes, now slowly taking place, after millions or tens of millions of years have elapsed. So long as men had no true conception of the time-intervals belonging to a planet's life, they naturally imagined that catastrophe of some kind could alone bring our planet's life to a close. The old Chaldean and Egyptian astronomers could only reconcile their ideas as to the limited duration of the Earth with the apparent stability of her condition, by supposing that the heavenly orbs were able to work the Earth's destruction—the planets conjoining in the watery sign Capricornus to destroy the Earth by water, and in the fiery Cancer to destroy the earth by fire.[2] But modern science looks for the end of the world through the gradual continuance of processes now at work during many millions of future years. In seeking for the probable manner of our Earth's decay and death, we must consider those causes which act continuously during the vast periods of time belonging to geological history, not those whose action alternates within periods which, though vast in human history, are but short in the life-history of our Earth.

(1128.) Thus the processes by which land has become sea and sea land, though they seem important to us, are not those by which the Earth will

[1] What we call the Quaternary period may be regarded as beginning with the time when the sun's influence thus became predominant,—that is, when the Earth's surface became divided in respect to the forms of vegetable and animal life, as well as in the astronomical sense, into the tropical and sub-tropical zones, the temperate zones, and the arctic and antarctic regions. The range of the various climates which we may call tropical, temperate, and arctic altered measurably, even markedly, from time to time. Sometimes the glacial regions invaded regions which had been temperate in climate, nay, had even presented semi-tropical forms of life; at others the ice masses retreated even within their present limits.

[2] Reference is made to this old astrological teaching in the second Petrine epistle, in the third chapter. It may be rendered thus: 'For this, they wilfully forget, that there were heavens of old, and an Earth standing out of water and amidst water, by the word of God; by which means the world that then was, being overflowed with water, perished: but the day of the Lord will come as a thief; in the which the heavens shall pass away with a great noise, and the elements shall be dissolved with fervent heat, and the Earth and the works that are therein shall be burned up.' Throughout, the Earth is evidently understood as the one sole world, the heavens as the firmament fixed and stable around us until 'being on fire they shall be dissolved.' The whole description here, as in other passages of the writings by the later Alexandrian Jews respecting the destruction of the Earth by fire (the earlier Jewish writers said nothing, and apparently thought nothing, on the subject), presents views corresponding with the oldest of old astronomy, tinged somewhat incongruously with the colouring of the later (but still most ancient) astronomy of the Egyptians and Chaldeans.

grow old. They might continue for millions of years and yet leave the Earth what she is now—a planet whose surface is distributed between land and water in unequal degree, the water largely predominating. But there are changes which, acting continuously in one direction, must largely alter the aspect of our Earth. We know that during millions of years of past Earth-life the vapours present in the atmospheric envelope of the Earth have been diminishing in quantity, certain vapours and gases once present in large amount having been altogether withdrawn, others greatly reduced in relative (as well, of course, as in absolute) amount, and even the two which remain having in all probability diminished largely in quantity within the geologic periods. The liquid portion of the Earth has also largely diminished. Doubtless within the Earth's interior parts which were once liquid have solidified, or at least made such approach to solidity as the conditions subsisting in the Earth's interior may permit. But it is liquid matter outside the Earth, now practically limited to water (oxygen and hydrogen), as the aerial matter is practically limited to oxygen and nitrogen (in mechanical admixture), which chiefly concerns us in considering the Earth's future. Geologists estimate that at least a third of the water formerly present outside the Earth's solid material has now taken its place within. Apart from chemical, vegetable, and animal processes, by which very slowly, but also very surely, water is taken up into the Earth's crust (to be restored as water in many cases, but not in all), waters pass mechanically within the solid substance of the Earth as springs of water show (to mention no other evidence), by which a portion, but assuredly not all, of the water which has found its way beneath the Earth's surface reappears above it.

(1129.) Now, slow though the withdrawal of water in such ways may be, it must in the long run, unless in some way interrupted, remove the whole mass of water now existing above the Earth's surface into the interior. To show the effect of time in making such processes effective, suppose the sea-level to sink at the rate of one foot in a thousand years (which would be no more than the thickness of a sheet of stout paper in a year), then in a million years the sea-level would sink a thousand feet, and in the millions of years doubtless belonging to the Earth's future every trace of water would be removed. Probably such processes, though they act continuously, yet as continuously diminish in their rate, and so the waters of the Earth may never wholly become waters under the Earth. Even if we should find that in some other planet which had passed through all the stages of planetary life there remains no water—as in our moon's case—it would by no means follow that our Earth in her last stage would be similarly desiccated. For, to take the case of the moon, we have reason to believe that our Earth, with probably

81 times as much water as the moon at the outset spread over a surface only $13\frac{1}{2}$ times as large, would have had six times as much water per square mile as the moon had at the same stage of the early life of each. The gradual processes of drying then (slow withdrawal as by a process akin to soaking being the chief) by which a planet parts with its waters, may not suffice in the Earth's case to remove all the water, however long they may continue, though they have proved sufficient in the case of the moon. It seems likely, however, that in her extreme old age the Earth will have a much more limited extent of water surface than she has now, such seas as she still has being then also much shallower.

(1130.) Another change taking place very slowly but continuously will doubtless produce important effects during the long periods we have to consider in dealing with the Earth's future. The Earth's rotation-rate is apparently diminishing, and the length of the day therefore increasing, through the action of the tidal wave in a direction contrary to that in which the Earth rotates (Art. 492). Whether the Earth's gradual contraction, which would tend to increase the rotation-rate, may not compensate this retardation wholly or partially—perhaps irregularly—will be difficult to determine. Certainly the evidence derived from the moon's apparent acceleration does not at present favour the belief that the Earth's rotation-rate is diminishing uniformly as the effect of the tidal wave would lead us to expect. But seeing how the moon's rotation-rate has been lengthened in remote ages until it has been brought into agreement with the mean revolution, we may assume that at any rate so long as she has seas our earth will rotate more and more slowly, though it seems wholly unlikely that her rotation-period will ever be brought into agreement with the lunar month (the time in which the Earth makes her circuit of the common centre of gravity of her own mass and the moon's). For the forces at work to retard the Earth's rotation will necessarily grow less and less effective as time passes.[1]

(1131.) If we consider the Earth's existence as measured by the duration of life of some sort upon her surface, we may probably conclude that her life may last until all physical processes of change have completed their work upon her surface. Even if long ere that the sun had died out, life in some

[1] We must reject absolutely the idea thrown out by some astronomers that the Earth's rotation-period, after being lengthened to a lunar month by tidal action will still continue to lengthen under the action of the solar part of the tidal wave and so tend to become a year in length. It has been overlooked that the moon's tidal action being stronger than the sun's would always prevent any lengthening of the Earth's rotation-period beyond the lunar month. The sun and moon would work together to increase the rotation-period to that length, after which the sun would work to still further increase the rotation-period and the moon would work to prevent such increase, and the moon, working more strongly than the sun, would prevail; so that the earth's rotation-period would thenceforth remain unchanged.

forms might remain on the Earth. Yet the higher forms of life, animal and vegetable, upon the Earth's surface would doubtless cease to exist much earlier. And if we limit our idea of the Earth's duration as an inhabited world to the duration of the human race in its more civilised developments, we must probably assign to the Earth a very much shorter life. Man regarded merely as an animal will doubtless outlast many of the higher races of animals on the Earth. Man regarded as a reasoning being, capable of understanding and appreciating the wonders which surround him, capable also of being moved by the divine sense of awe, might remain long upon the Earth, content to live on the Earth's income, not upon her garnered stores. But civilised man as he has heretofore been known upon the Earth is a creature whose evolution leads to the development of cravings which apparently must be satisfied, and which cannot be satisfied without an ever-increasing rapid exhaustion of those supplies of mineral wealth which may be regarded as our Earth's capital. The mere fighting tastes of men have required an ever-growing supply of war material, offensive and defensive, exhausting the Earth's stores already far more rapidly than they are being exhausted to meet other requirements of civilised man. Already in many parts of the Earth special stores of great value have been exhausted or are approaching exhaustion. Taking the whole Earth, and supposing the rate of exhaustion now in progress to increase in the future at the same rapidly increasing rate of increase as in the past few centuries, a few thousands of years (instead of the millions we have been speaking of) will suffice to make our Earth no longer habitable by civilised varieties of the human family. Haply ere then simpler and more peaceable, nobler and more philosophic races, will have been developed, in which case our Earth may remain inhabited by man for millions of years.

CHAPTER X.

THE MOON AS A PLANET.

(1132.) The moon has been already considered in her relation to the Earth as satellite: viz. as an important luminary, at times chief ruler of the night, as serving for the measurement of time (even regulating, significantly enough, religious festivals) and as swaying the tides. There are other circumstances connected with what may be called the moon's terrestrial relations—as the occurrence of eclipses both solar and lunar, the harvest moon, &c.—which have not yet been considered. But in the present chapter the moon is to be dealt with as a planet, a companion planet of our earth, though far inferior to the Earth in importance, yet considered in herself a massive orb, circling around the sun as the true centre of her motion, and manifestly belonging to the same family as the Earth, Venus, Mars, and Mercury—not to be relegated to an inferior order.

(1133.) Regarded as a planet the moon would share with the Earth the third position in order of distance from the sun, her mean distance and the Earth's from him being equal. She is the smallest member of the family of five planets, of which the Earth is the chief. Her mass (see Art. 722) is less than one-fifth of Mercury's; but if this difference of mass should be regarded as setting the moon in a class inferior to that in which Mercury is set, then *à fortiori* Mercury should be set in a class inferior to the Earth's, for Mercury has but one-fifteenth of the Earth's mass. The moon may be described as travelling in the same orbit as the Earth around the sun, but affected in her movements by the perturbing action of a neighbouring planet, the Earth.[1]

[1] It may interest the student to inquire what would be the actual path pursued by the moon if at any moment the Earth were suddenly removed, or suddenly ceased to exert perturbing influence on the moon. The orbit would depend on the positions occupied by the moon in her orbit round the Earth and by the Earth in her orbit round the sun. The last point may be left out of consideration here, the Earth being regarded for convenience as travelling in a circle around the sun at her mean distance when the change took place. When the moon is 'new' her actual velocity is least, because the advance which she shares with the Earth is here diminished by the full amount of her velocity in her relative orbit round the earth. If liberated then from the Earth's perturbing influence when new, the moon would travel in an orbit having its aphelion at

The effect of these (which amount collectively to the moon's relative orbit round the earth) is very different from that commonly attributed to it in treatises on popular astronomy. How little the moon's path is twisted or contorted by combining her apparent motion round the Earth (or her perturbations) with her orbital motion round the sun, may perhaps be best shown as follows :—

(1134.) In fig. 330, let an imaginary circle traversing the middle of the circular track M M′ be the path of the Earth round the sun at S. Now the width of the track M M′ represents almost exactly on the scale of fig. 330 the diameter (477,600 miles) of the moon's apparent orbit round the Earth. For the diameter M M′, about three inches, represents a distance of 185,560,000 miles, so that a distance of 477,600 miles would on this scale be about $\frac{1}{130}$th of an inch, which is less than the breadth of the track M M′. Now if we suppose the moon to be 'new' at the place numbered 0, she would be 'new' again at the points marked successively 1, 2, 3, &c., to 12; and she would be 'full' at the points marked with short lines outside the track M M′ midway between these points. Thus, conceiving the moon and Earth as most minute points travelling together round the track M M′, we must imagine the Earth as a minute disc having about $\frac{1}{60}$th the width of the track and pursuing the track's central line; while the moon

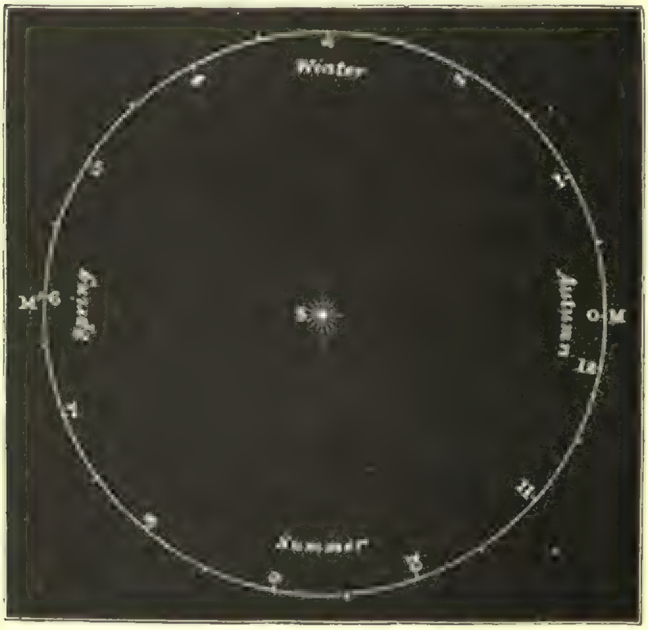


FIG. 330. - Illustrating the true form of the Moon's path round the Sun.

her point of liberation and therefore lying wholly within the Earth's present orbit. (The student can readily calculate the dimensions of the orbit, but we must not occupy more space with such matters than their intrinsic interest warrants.) If, on the contrary, the moon were liberated when 'full,' at which time she travels with her greatest velocity, the point of liberation would be the perihelion of her subsequent orbit, which, therefore, would lie wholly outside the Earth's. If the moon were liberated at her 'first quarter,' when her velocity has (appreciably) its mean value, she would move in an orbit having the same mean distance as the Earth's, but more eccentric because the moon is travelling across that mean distance with a certain thwart motion outwards (her velocity in her relative motion round the Earth). The same would happen if the moon were at her 'third quarter' when liberated, the only difference being that whereas in the former case the moon in her new orbit would be at her mean distance and passing towards aphelion at the moment of her liberation, in the latter she would be at her mean distance and passing towards perihelion. In all cases the new orbit would pass through the point where the moon had been when liberated from the Earth's perturbing influence, and the moon in her subsequent motions would continue to pass through that point until the perturbations produced by the other planets slowly drew her orbit away from its first position and modified its form.

must be conceived as a still minuter point passing from the inside edge of the track at 0 to the outside edge at the outer mark between M and 1, to the inside edge again at 1, thence to the outside at the next outer mark, thence to the inside again at 2, and so on, alternately touching the inside edge at 2, 3, 4, 5, 6, 7, &c. to 12, and the outside edge at the points indicated by short lines midway between the numbers. If the path M M′ were altered to correspond to these gentle undulations, the keenest eye would not detect the change. As for the slight difference of level, as the moon on her inclined path passes alternately above and below the plane of the ecliptic, that would be much less even than the minute difference of distance from S.[1] Pursuing the slightly sinuous course between the inner and outer edges of the circle M M′ before described, the moon must further be conceived as passing alternately above and below the plane of the circle (at distances averaging slightly less than half M 1) attaining between these crossing-places her greatest distance above and below this plane, such distances being less than $\frac{1}{22}$nd part of the width of the track M M′.

(1135.) The student should now turn to Plate XVII., and consider the moon's path around the sun as illustrated there scarcely less correctly than the path of the Earth, taking into account such slight sinuosities and changes of plane as have just been considered, by which the apparent positions of the 366 dots representing the Earth would scarcely have to be appreciably altered to cause them accurately to represent the moon from day to day (of terrestrial time) along her annual course round the sun.

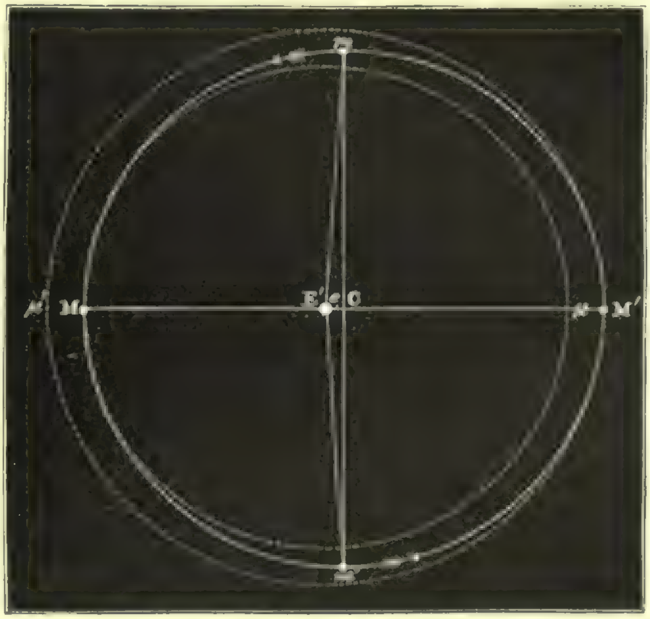


Fig. 331.—Illustrating the eccentricity of the Moon's orbit.

(1136.) Though hardly affecting the exactness of the statements just made respecting the moon's motion around the sun, the ellipticity of her relative orbit round the Earth must here be indicated. It ranges between 0·66 and 0·44, the mean distance remaining unaltered. In fig. 331, if *m* M *m*′M′ represent the moon's orbit, which is appreciably circular, C its centre, E′ would be the position of the Earth when the eccentricity has its maximum value, *e* her position when the eccentricity has its least value; a point midway between E and *e* indicating the position of the Earth when the eccentricity has its mean value. The maximum distance of the moon from the Earth (measured from centre to centre) amounts to

[1] It would be obviously less in the same degree that sin. 5° 9′ is less than unity, or about as 9 is less than 100; in other words, since the moon's range within and beyond the median line of the track M M′ is half the width of the track, the moon's range above and below the plane of the circle M M′ would be only about $\frac{9}{200}$ths, or about $\frac{1}{22}$nd of the width of the track M M′ (This would be about $\frac{1}{2990}$th of an inch.)

nearly 253,000 miles, while the least is only about 221,600 miles. On account of these changes of distance the disc of the moon ranges in apparent size from $M\mu$ (fig. 331) its least to $M'\mu'$ its greatest value.

(1137.) Fig. 332 indicates the actual relations between the moon's apparent orbit round the Earth M_1M_5 and the orbit in which she accompanies the Earth E round

FIG. 332.—The Sun's globe and the Moon's relative orbit round the Earth compared.

the sun, only a space of 7 ft. must be supposed to separate E from the disc representing the sun. This disc and the orbit M_1M_5 remain unchanged in size, but the small discs representing the Earth and moon must be supposed reduced to scarcely perceptible dots, unequal in size, the dot representing the Earth being about $\frac{1}{290}$th of an inch in diameter, while the dot representing the moon should be less even than this in the same degree that $M_{\prime\prime}$, fig. 333, is less than E.

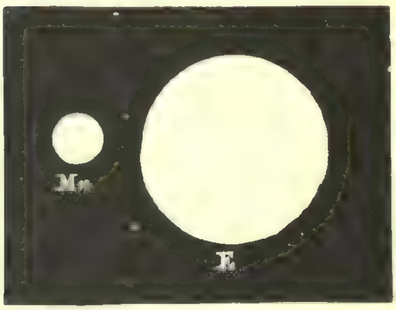


FIG. 333.—The globes of the Earth, E, and the moon, $M_{\prime\prime}$, on the same scale.

(1138.) Fig. 332 serves to explain the phases of the moon. When the moon is at M_1 her darkened side is towards the Earth and she is invisible; or as at 1, fig. 334. As she advances towards M_2, the observer at E sees her passing to the east of the sun, her western edge beginning to be illuminated and showing as a fine crescent. When she has reached M_2, fig. 332, her aspect is as shown at 2, fig. 334. By the time she has reached M_3 she is half-full, as shown at 3, fig. 334 (for from the Earth at E, fig. 332, equal parts of the dark and illuminated halves of the moon can be seen). At M_4, fig. 332, the moon appears as at 4, fig. 334, at M_5 as at 5, showing her full face; and so on, passing through decreasing light phases, her eastern edge being now illuminated, until

FIG. 334.—Illustrating the Moon's phases.

when she has again reached the position M_1 on her relative orbit she is again lost wholly to view as at 9. The moon's return to M_1, however, must be understood to be her return to the line joining the sun and Earth. As the Earth in the meantime will have advanced through nearly a twelfth of her annual circuit round the sun,

as from E to E′ round S in fig. 335, the point corresponding to M_1 will have advanced through a corresponding arc, or as from m_1 to M′ in the new position of the relative orbit of the moon round the Earth. All the intervals just considered, as from M_1 to M_2, fig. 332, from M_2 to M_3, &c., must be supposed increased in the same degree that the circuit $m_1m_2m_3m_4$, fig. 335, *plus* the arc m_1 M′ exceeds the circuit $m_1m_2m_3m_4$ alone.[1]

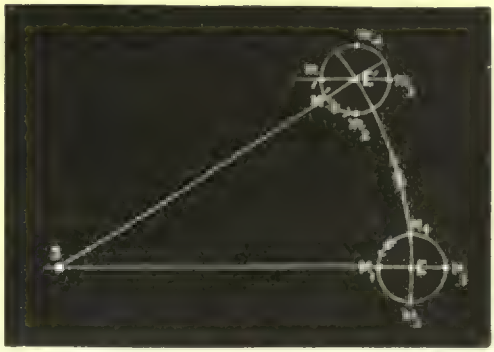


Fig. 335.

(1139.) The moon has a diameter of 2,159·6 miles. She shows no polar compression, and calculation shows that the compression due to the moon's slow rotation on her axis would not reduce the polar axis by more than 46½ feet as compared with the axis at right angles to the polar axis (and also to the direction in which we look at the moon) a difference which no telescopic measurement could detect. The axis at right angles to the polar axis and directed towards the Earth has been estimated as 232½ feet longer than the shortest, or 186 feet longer than the longest visible diameter. The explanation of this lengthening is precisely akin to that by which in the statical discussion of the tides it is shown that the water would be raised under the moon and on the Earth's opposite side. The moon's case is precisely the statical case, and the moon's solid globe yields under the press exerted by the Earth upon it. The estimates of Newton and Laplace depend in part on the density of the moon, and the probable rate of increase of this density towards the centre. Newton, who considered only the lengthening of the moon towards the Earth, did not take the moon's smaller density adequately into account; nor did Laplace in dealing with the polar compression of the moon. At present, however, the question cannot be dealt with much more satisfactorily than by Newton and Lagrange. But probably the lengthening of the moon's globe in the direction of a line joining her centre and the earth's is greater than Newton concluded. It is not likely, however, that it is greater than 250 feet.

[1] In Arts. 331 and 332 the difference between the time of circuit round the relative orbit and the interval from conjunction to conjunction with the sun (successive new moons) is considered in another way. Both ways of dealing with the problem should be studied. As above explained, fig. 335 shows in what degree the moon's synodical month or the *lunation* (the time in which the moon passes through all her phases) exceeds the sidereal month, or time in which the moon completes the circuit of her relative orbit round the earth. It is to be noticed that the length of the sidereal month is deduced from the observed mean length of the synodical month, not *vice versâ.* We know the mean length of the synodical month very accurately, because astronomy from the days when first it timed the moon's motions at all has kept count of the number of lunations, and knowing the exact number of lunations in so many hundreds of thousands of days, knows the exact mean length of a lunation, 29·53059 days. In this time, or for convenience say 29·53 days, the Earth traverses an arc equal to $\frac{2953}{36526}$ths of a circuit round the sun, which gives us the measure of the angle E S E′ or m_1 E′M′, adding which to a complete circuit we find that the arc traversed round the Earth in a synodical month is $(1 + \frac{2953}{36526})$ or $\frac{39479}{36526}$ths of a circuit. Hence,

Synodical month : Sidereal month : : 39479 : 36526

Therefore,—

$$\text{A sidereal month} = \left(\frac{36526}{39479}\right) 29\cdot53 \text{ days}$$
$$= 27\cdot32 \text{ days}$$

The calculation here is, of course, quite rough. By taking the exact known durations of the mean lunation and of the year, the duration of the synodical month may be determined with any required degree of approximation. I have here only indicated the method of the calculation.

With these dimensions the moon has a surface less than the Earth's in the proportion of 1,000 to 13,435. Thus the moon's surface is about $\frac{2}{27}$ths of the Earth's, or about 14,600,000 square miles. It is almost exactly equal to that of North and South America together, exclusive of islands. The arctic and antarctic regions together exceed the moon in area by one-ninth.

(1140.) The moon's globe is less than the Earth's as 100 is less than 4,926, her mass less than the Earth's (Art. 722) as 100 is less than 8,077. It follows that her mean density is only about $\frac{3}{5}$th of the Earth's, or (Art. 688) about 3·33,—that of water being taken as unity. Gravity at her surface is equal to about $\frac{1}{6}$th of gravity at the surface of the Earth.

(1141.) The moon rotates on her axis in exactly the mean duration of a sidereal month. In other words the moon rotates on her axis and revolves around the Earth at precisely the same rate, the former motion being almost uniform, the latter varying in greater or less degree as the moon's orbit is more or less eccentric and as the moon's motion in her orbit is more or less affected by perturbations. The length of the actual lunar day (measured by the returns of sunlight) is longer than the true period of a lunar rotation for the same reason that our solar day is longer than the sidereal day, but in greater degree. The lunar day has in fact the exact length of a mean lunation, as is obvious if we consider that it is midday on the middle of the moon's visible face at the time we call full moon, and not again midday for the same part of the moon till the time of the next full moon.

(1142.) The question of the probable cause of the great length of the lunar day, and in particular of the exact agreement of the moon's rotation-period with that of her mean revolution, can be more satisfactorily considered in these days than in the time of Newton, or during the time of the great development of mathematical analysis under Lagrange, Laplace, and their fellow-workers. For then any explanation which involved processes of slow change lasting for millions of years could scarcely be considered in the face of the settled conviction that the moon was set in the heavens less than 6,000 years ago to rule our terrestrial nights. Now that Nature's Bible has been partly read, and the conviction forced on men that the solar system has endured for many millions of years, the case is altered. We can accept some other explanation than that the moon rotates as she does because she was so set rotating.

(1143.) It has been shown that if the moon's rotation were at any time very nearly what it now is, the Earth's action would tend to make the agreement between the rotation and the mean revolution exact. But no reason has been given for any such original approach to agreement in the rotation and revolution as would lead to this result. Granted, however, that the moon many tens of millions of years ago was an intensely hot mass in large part liquid or even

vaporous, and that she continued in this condition, exposed to the Earth's action, during many millions of years, we can understand that whatever her original rotation-rate may have been, her rotation-period would gradually have been forced into coincidence with her mean period of revolution. The tidal action on our own Earth is producing a slow change of a similar kind ; but the tidal action of the Earth on the moon when the moon was in great part liquid would have been far more intense. It must not be supposed that even in the moon's case the lengthening of the rotation-period took place rapidly. On the contrary, many millions of years would have been required to change the moon's rotation-period from such a duration as our Earth's to its present value. Whatever opinion we may form as to the moon's past, and whatever length, within reasonable limits, we may suppose her rotation-period to have had in the beginning of her career as an independent body, we must regard the face (unchanging save as to its illumination) which the moon turns constantly earthwards, as telling us like so many other features of the solar system, of periods of time in the past compared with which the centuries and thousands of years by which man measures historical eras seem but as seconds.

(1144.) If the moon's revolution round the Earth were as uniform as her rotation on her axis, and if the axis of rotation were perpendicular to the plane of the relative orbit, the moon would always turn exactly the same face earthwards. Since neither relation holds, the face turned earthwards slightly varies, so that we are able to get a view of rather more than half the moon's surface :—

(1145.) When the eccentricity of the moon's orbit has its mean value, the moon in her revolution round the Earth passes alternately 6° 17′ 19″ in advance of and behind the place she would have if her motion were uniform (which place she only occupies when in perigee and apogee). But when the eccentricity has its maximum value the range on either side of the mean place may amount to as much as 7° 45′. The effect of this discrepancy between the actual motion and the mean motion, which is in fact a discrepancy between the revolution and the rotation, is to make the moon appear to gain and lose alternately six or seven degrees in rotation, so as to bring a lune of her surface six or seven degrees wide into view alternately on the east and on the west of what may be called the moon's mean face. This, which corresponds to an apparent swaying of the moon to and fro in the direction of her rotation, is called the *libration in longitude* (because the apparent swaying of the mean centre of the moon's visible surface from the apparent centre of her disc due to this cause is measured in the direction of longitudes on the moon's globe.)

(1146.) The moon's equator-plane is inclined to the plane of the path in which she travels round the sun by an angle which varies between 6° 32′ and 6° 46′.[1] The

[1] The moon's equator-plane is inclined 1° 30′ 11″ to the plane of the ecliptic, and is always so placed that when the moon is at the ascending or descending node of her relative orbit, the equator-plane is turned edgewise towards the earth, and (as seen from the Earth) is inclined descendingly

K K

effect of this is to produce changes in the moon's aspect as seen from the Earth akin to those which affect the Earth's aspect as seen from the sun, but less in amount. Moreover, the actual opening of the moon's equator corresponding to the opening of the Earth's equator as seen from the sun at midsummer and winter, never exceeds that due to an inclination of about 6° 44′. The mean angle through which the moon's equator-plane appears to open out, alternately northwards and southwards (the poles swaying alternately into view and out of view), is about 6° 39′. And thus an apparent swaying motion takes place akin to that producing the libration in longitude, the mean centre of the moon's visible surface swaying alternately northwards and southwards of the apparent centre of the moon's disc—whence this apparent swaying is called the *libration in latitude*.

(1147.) Fig. 336 shows exactly to scale the range of the moon's two principal librations, $d\,d'$ being the range of the mean centre's libration in longitude, D D′ the range of its libration in latitude, and the space A G G′A′ that over which the mean centre of the moon's surface ranges under the combined action of the two librations. The

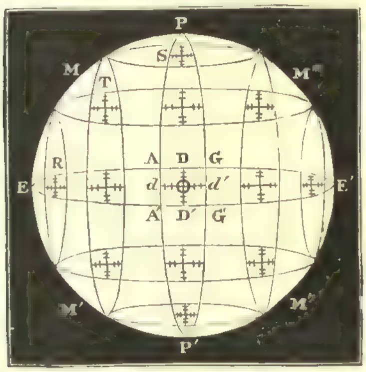


FIG. 336.

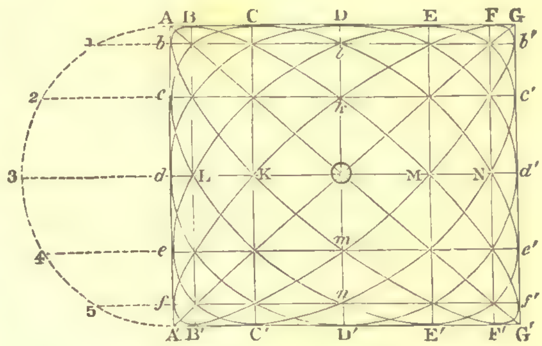


FIG. 337.

nature of its motion may be inferred from the curves shown in fig. 337, each of which represents a path which would be pursued by the moon's mean centre if the relative positions of the perigee (on which the libration in longitude depends) and of the nodes of the moon's orbit (on which the libration in latitude depends) remained unchanged during a complete revolution. As the apses and nodes are always changing in relative position, the various curves shown in fig. 337 are never actually completed, but are constantly merging from one form into another, the process varying in rapidity, being even occasionally for a while reversed, but on the whole causing the curves to change from the full opening of the ellipse D d D′d' to the slant line A O G′, thence to the full opening again and onwards to the slant line A′O G—the whole lunar theory with all its multitudinous and as yet not fully interpreted intricacies being involved in the determination of the precise paths followed by the mean centre of the moon's visible surface. Other points on the moon's surface range over such spaces as are shown at

or ascendingly (respectively) to the ecliptic. Since the average inclination of the moon's orbit to the ecliptic is nearly 5° 9′, it follows that the angle at which the moon's equator-plane is thus inclined has a mean value of about 6° 39′. The mean amount of opening of the moon's equator given above is not correctly obtained by adding the mean value of the inclination of the moon's orbit to 1° 30′, because the maximum inclination of the orbit is always attained when the moon is near a node, whereas the maximum opening due to her inclination is attained when she is farthest from her nodes.

R, S, and T of fig. 336, the paths traced by the point which ranges over such spaces being respectively such as are shown in figs. 338, 339, and 340.

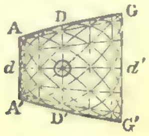


FIG. 338.

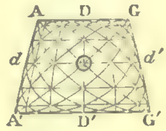


FIG. 339.

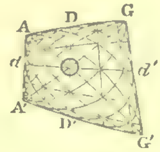


FIG. 340.

Libration Curves traced by points on the Moon's face situated as at R, S, and T (respectively) in fig. 336.

(1148.) There is another libration, called the *diurnal* libration, which depends on the circumstance that, owing to the Earth's rotation, the place of the observer is shifted with respect to the line joining the centres of the Earth and moon. The extent of this libration varies in different latitudes and at different seasons. If we imagine an observer placed at the centre of the moon's visible disc, a line drawn from him to any station on the Earth would be carried by the Earth's rotation along a latitude-parallel, and the angle which it made at any moment with a line joining the centres of the Earth and moon would correspond to the *diurnal* displacement of the moon's centre, as seen from the station at that moment. The Earth's apparent radius as supposed to be seen from the moon exceeds the moon's as seen from the Earth in the same degree that the Earth's real radius exceeds the moon's. Thus the Earth's radius subtends an angle of about $16' \times (7927 \div 2160)$ or nearly a degree. When the moon is in perigee this angle is greater than a degree. Therefore, when we have determined the fringe of extra surface brought into view by the moon's maximum librations, we can widen this fringe all round by a breadth of about one degree.[1]

(1149.) In considering the actual extent of the moon's surface which her librations carry into and out of view alternately, we need not trouble ourselves about the varying nature of the combined libration. It might seem, at first sight, as though certain parts of the moon would only be brought into view when the libration in latitude attains its maximum value,—that is, when the libration in longitude vanishes; and *vice versâ*. But as a matter of fact, if we consider the four cases where the total libration has its absolute maximum value—viz. when the mean centre is at the four points A, G, A′, and G′ (fig. 336)—we take into account every portion of the moon's surface which libration can possibly bring into view. So that all we have to ascertain is the area of the space on the sphere corresponding to the four lunes brought into view at these extreme librations. This is easily effected,[2] and we learn that the area

[1] We must not widen it everywhere by a breadth of 1° 1′ 24″, the maximum apparent semi-diameter of the Earth as seen from the moon, simply because this apparent semi-diameter is only presented when the moon is in perigee, while the moon attains her greatest total libration, as well as her greatest libration in longitude, only when she is at her mean distance. We may, however, employ even this maximum value of the horizontal parallax when the moon has her maximum libration in latitude, since there is nothing to prevent her from attaining this libration when she is at her nearest to the Earth.

[2] We know the maximum breadth of the four lunes. It is easily shown that the total area brought into view by libration bears to the whole sphere the ratio

$$2\,(7^\circ\,45' + 6^\circ\,44') : 360^\circ$$
$$= 14^\circ\,29' : 180$$
$$= 869 \quad : 10800.$$

Thus the total area brought into view by libration bears to a hemisphere the ratio of about 100 to 621. (Arago makes the ratio 1 to 7, though using 10° 24′ as the absolute maximum of libration. It is not easy to understand how an error crept into his treatment of a problem so simple.) The proportion of the part of this hemisphere

thus brought into view by libration is between one-twelfth and one thirteenth of the whole area of the moon, or nearly one-sixth part of the hemisphere turned away from the Earth when the moon is at her state of mean libration. Of course a precisely equal portion of the hemisphere turned towards us during mean libration is carried out of view by the lunar librations.

(1150.) If we add to each of these areas a fringe about 1° wide, due to the diurnal libration,—a fringe which we may call the parallactic fringe, since it is brought into view through the same cause which produces the lunar parallax—we shall find that the total area brought into view is almost one-eleventh part of the whole surface of the moon; a similar area is carried out of view: so that the whole region thus swayed out of and into view amounts to nearly $\frac{2}{11}$ths of the moon's surface.

(1151.) In fig. 341 a side-view of the moon is given. The figure is self-explanatory: but it is to be observed that m M *m* and m′ M′ *m′* are arcs of 20° 32′, corresponding to the absolute maximum libratory swayings, A O G′ and A′ O G of fig. 336: p P *p* is an arc of 13° 28′, corresponding to the maximum libratory swaying in latitude (D O D′ of fig. 336); and e E *e* is an arc of 15° 30′, corresponding to the maximum swaying in longitude (*d* O *d′* of fig. 336). It must always be remembered, however, that although such regions as p P p′ (fig. 341) are brought into view by libration, they are always seen very much foreshortened, not as presented in fig. 341.

Fig. 341.—Imaginary side view of the Moon's Globe, showing the relative extent of the areas brought into view and carried out of view by libration.

(1152.) We have assumed in the preceding articles (from 1144 to 1151) that the moon rotates with perfect uniformity on her axis, while revolving around the Earth. This, however, is not strictly the case. In the first place, it is manifest that since the moon's mean sidereal revolution is undergoing at present a process of diminution, owing to what is termed her secular acceleration, her rotation

never seen to the whole hemisphere is therefore about 521 to 621; or if we represent the whole sphere by 1, the area of the part absolutely invisible will be represented by 0·4195. (Klein, in his *Sonnensystem*, gives 0·4243, which is nearer to the truth than the value resulting from Arago's estimate, namely, 0·4286, yet still considerably in error, particularly as Klein also names the value 10° 24′ for the maximum libration.)

If, however, we take into account the effects of the diurnal libration, it can readily be shown that the portion of the moon which is never seen under any circumstances bears to the area of the whole moon almost exactly the proportion which 148 bears to 360, or 37 to 90,—that is, it is equal to 0·4111 of the whole area. The part which can be carried out of view or into view by the libration, including the parallactic libration, amounts to $\frac{8}{45}$ths of the whole surface,—or to 0·1777, if the whole area is represented by unity.

must either undergo a corresponding acceleration, or she would in the course of time so turn round with respect to the Earth that the regions now unseen would be revealed to terrestrial observers. She would, in fact, have thus turned round by the time when she had gained half a revolution. It has been shown, however, by Laplace, that the attractions to which she is subject suffice to prevent such a change, and that her rate of rotation changes *pari passu* with her rate of revolution. It must, therefore, be to this slight extent variable. A similar remark applies to all secular perturbations affecting the moon's motions. So that it is impossible that the further side of the moon should ever be turned towards the Earth unless under the action of some extraneous influence, as the shock of a mass comparable with her own.

(1153.) But a real libration, possibly recognisable by observation, affects the moon's rotation. We have seen (Art. 1139) that the diameter of the lunar spheroid which is directed towards the Earth exceeds the longest diameter at right angles to it. The exact excess is not known, but probably it is about 186 feet. It is this elongation of the moon which causes her to turn the same side towards the Earth. In any other position the action of the Earth would not maintain the lunar globe in equilibrium. Now, in consequence of the inequalities of the moon in longitude, the elongated axis is not always directed exactly to the Earth. A real libration of the moon must then take place (as Newton was the first to point out) in consequence of which the elongated axis oscillates perpetually on each side of its mean place.

(1154.) The real libration, theoretically predicted by Newton (Art. 1139), and confirmed by the analytical researches of Lagrange, has been detected by observers. It might seem, at first sight, that this libration would be most noticeable as depending on the moon's varying motion in a single revolution, since she may be so much as 7° 45′ before or behind her mean place. But, as a matter of fact, the extent of the real libration depends much more on the length of time during which the Earth's action is exerted than on the actual displacement of the moon's longer axis from its mean position. Accordingly, the lunar irregularity called the annual equation although, as we have seen, it only affects the moon's place by a small amount at the maximum, yet, as its period is a long one, enables the Earth to affect the mean rotation-rate more effectually than do any of the other lunar perturbations. In the 'Connaissance des Temps' for 1822, Nicollet submitted 174 observations by himself and Bouvard to a searching discussion. The only inequality recognised was that corresponding to the *annual equation*. Nicollet deduced for it a maximum value of 4′ 45″. Probably this is somewhat in excess of the true value.[1]

(1155.) The Earth's attraction upon the rotating moon must in some degree affect the mean rotation. In fact, all the perturbations affecting the moon's motion of revolution must be reflected (or, as it were, represented in miniature) in these variations of her motion of rotation. If the moon's rotation once took place in a period

[1] The ellipsoidal form of the moon is not only demonstrated by the existence of a recognisable real libration, but also by the continuance of the singular relation between the position of the moon's equator and orbit referred to in Art. 1146, note. It is manifest that since the *position* of the plane of the orbit is continually shifting, this plane would depart from coincidence with the plane of the moon's equator unless some extraneous force acted to preserve the coincidence. If the moon were a perfect sphere, the Earth would have no grasp upon her, so to speak, whereby to maintain the observed relation between the equator-plane and the orbit-plane. But Lagrange showed that the action of the Earth on an ellipsoidal moon would constantly maintain the coincidence. Since the coincidence is maintained, we must conclude that the moon is ellipsoidal.

not absolutely coincident with that of her revolution, the attraction of the Earth would have forced the rotation-period into *mean* coincidence with the period of revolution. The rotation would, in that case, however, no longer be strictly uniform, apart from the real librations considered in the preceding articles. The moon would librate on either side of her mean position, independently of her variable motion in her orbit. This form of real libration has not been observationally recognised.

(1156.) The most striking physical characteristics of the moon, regarded as a planet, are (1) that there is no water on her surface, and (2) that she has no perceptible atmosphere. There is not the slightest trace upon her varied surface (whose every detail has been examined with almost wearisome assiduity) suggestive of the presence of water now, or of those changes of appearance which could not but result from the presence of water. As to an atmosphere, the most tenuous atmosphere, if any such existed, could not have failed to disclose its presence under the tests which have been applied. Under spectroscopic examination the sunlight we receive from the moon shows no trace of having passed through any atmosphere. As the moon passes over the sun's face in the time of eclipse, none of the phenomena which would inevitably result if the moon had an atmosphere have ever been observed ; all the features of the sun's surface are sharply seen up to the very edge of the moon's disc, while the eclipse has been partial, and when the eclipse is total the most delicate features of the solar surroundings have remained thus visible, which certainly would not be the case if the moon had any appreciable atmosphere. The same thing has been noticed when the moon has occulted the planets, even the delicate details of the rings of Saturn remaining undisturbed when the planet has been occulted by the moon. Occasionally when the illuminated side of the moon has passed over a star, the star has seemed to cling for a moment or two to the moon's edge ; but in the first place this is not what would happen if the moon had an atmosphere (a star would in that case be altered into a fine arc of light) and in the second the phenomenon can readily be explained as the combined effect of diffraction and irradiation. It is, in fine, absolutely certain that the line of sight past every portion of the moon's edge passes through no appreciable quantity of atmospheric matter, gaseous or vaporous.

(1157.) It has been suggested that although on the visible face of the moon there may be neither air nor water, both may be present in measurable quantity on the unseen side. But although the mathematician Hansen was among the first to suggest this attractive idea, no physicist who considers the conditions can for a moment accept it. The moon's elongated figure would in the first place be symmetrical, not egg-shaped with the small end of the egg towards us, as the suggestion requires. But if it were or could be egg-

shaped the centre of gravity of the moon's globe would still be so situate as to be nearer the zone of the moon's surface which limits her apparent outline than to the middle of the remote unseen part, so that it would be towards that zone, of which we get fair though foreshortened views during the moon's libration, to which the waters most probably, and the atmosphere most certainly, would find their way. Unquestionably a barometer on the moon would be higher (if there were any atmosphere at all to give it height) along those very parts near the edge through which we see the sun (in partial eclipse) and planets and stars (at occultation) as they never could be seen if there were any air over these parts of the moon. Again, there are several depressed tracts near and at the moon's edge, where, if there were any waters on the moon, a portion of those waters could not but flow : yet even under extreme librations we never see even the faintest suggestions of water being present on these tracts.

(1158.) We have evidence, reflected as it were from the moon's surface, to show how the aspect of an air-enwrapped planet like our Earth seen from the moon, differs from that which the moon presents to us :

When the Earth passes exactly between the moon and the sun, the moon (at that time, of course, 'full') is thrown into the Earth's shadow. If the Earth had no atmosphere the moon would be for a while in full shadow or be utterly invisible, since the Earth seen from the moon looks more than thirteen times larger than the sun, and therefore hides him completely from view. But the moon at such times is not (generally) thrown into complete shadow. She continues to shine with a ruddy light, which is sometimes so bright that even at the time of total eclipse, as astronomically determined, some have imagined that there was merely a kind of haze or mist in the upper air, giving to the moon that brilliant ruddy lustre. During some total eclipses, however, the moon is not ruddy, and is barely visible.

(1159.) The reason of the ruddy light usually seen, and of its variations of lustre, including its occasional entire disappearance, can be readily understood. The Earth has an atmosphere, and this atmosphere brings the sun into view all round the Earth's disc as supposed to be seen from the moon. The clearer the atmosphere over that zone of the Earth's surface which is at the edge of her disc as seen from the moon, the brighter the ruddy light. Should there be much mist over parts of that zone, the colour will be deeper, or we shall recognise the dull coppery tint generally noticed : (for over the zone in question the sun seen from the Earth is low, and in the evening and morning haze and mist are common). If the greater part of that zone is covered by clouds, which may happen occasionally, but cannot, of course, be common, the deeper and more refractive parts of the Earth's atmosphere are

entirely prevented from refracting sunlight, and (if the cloud-layers are deep enough) no light whatever can reach the moon.

(1160.) The appearance of this ruddy light on the moon's face tells us then in effect that to an eye situate on the moon at the time of total eclipse, there comes light from the sun. It shows that instead of that sharp blotting out of the sun's light which we observe in total eclipses of the sun, the sun's disc is lifted into view either along an arc of the Earth's apparent outline when the eclipse (or while the eclipse) is non-central, or entirely round the Earth's disc in and near the time of central eclipse.[1] We only

[1] Very singular misapprehensions appear to exist in regard to the light which reaches the moon through the Earth's atmosphere at the time of total lunar eclipse. It seems to be imagined that this light is only that coming from our own air lit up by the rays of the horizon sun—light which would be practically insensible at the moon's distance, and could not produce a hundredth part of the observed effect. As a natural consequence of this mistake the wild idea has been suggested (and, unfortunately, promulgated) that the ruddy light of the eclipsed moon is due to the inherent heat of her surface as (just before) it had been heated under the sun, with no atmosphere to shield the surface from the full vigour of his rays.

The mistake has partly arisen from a misapprehension of the correct but rather cumbrous explanation given by Sir John Herschel in his 'Outlines of Astronomy,' but it has been chiefly due to mistakes regarding some of the elementary principles of optics.

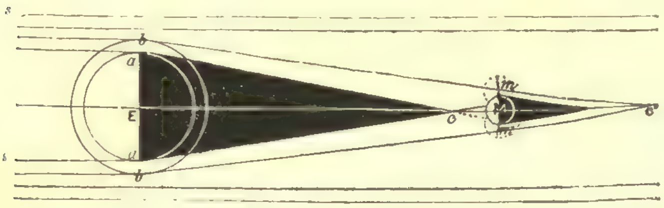


Fig. 342.—Illustrating the illumination of the Moon by sunlight during a lunar eclipse.

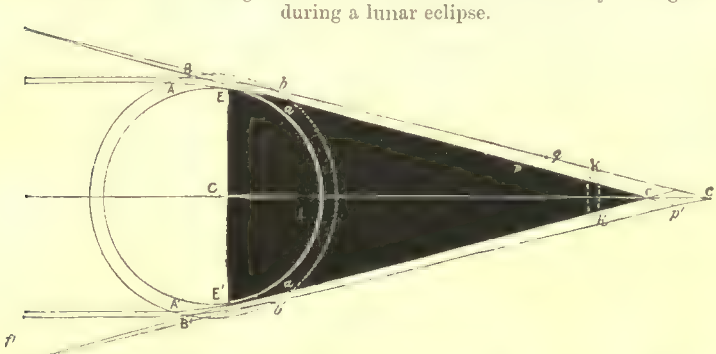


Fig. 343.—Illustrating the illumination of the Moon during a central Lunar Eclipse.

If we suppose *a a′* (centre E), fig. 342, to be the Earth's globe, m m′ (centre M) the moon's, the sun being far away on the extreme left, we see that the rays *s a*, *s′a′*, *s b*, *s′b′* from the sun (say from the centre of his disc), will be bent by the Earth's atmosphere into such directions as a c, a′c, b c, b′c′. Since the horizontal refraction of the atmosphere for us on the Earth's surface is about 34′, while clearly there is the same refraction in passing out as in passing in, it is clear that *c* is a point whence the Earth's radius would subtend an angle of 68′, for the rays bent to c touch the Earth grazingly. At M, we know, the Earth's radius subtends an angle of about 1° (Art. 1148). Hence the rays from the sun's centre converge by grazing refraction to a point nearer to the Earth than the moon is. The rays s b and s′ b′ are supposed to pass so high above the Earth's surface that they converge to a point *c′*, farther from the Earth than the moon is. (The student will notice, of course, that *a b* and *a′b′* are monstrously exaggerated. Moreover, he must not fall into the mistake of regarding the points *c* and *c′ as foci*: they are nothing of the kind, as will presently be seen.) The moon then receives rays (when at M) from the sun's centre along directions passing nearer to the Earth's surface than *b* and *b′* but not actually touching it. (I suppose the observer on the moon at the centre of her earthward-turned hemisphere.) When the moon is at m rays reach her along *b m* from the sun's centre, and similarly along *b′m′*

have to consider what face the Earth turns moonwards at the time of any lunar eclipse to learn just what parts of the Earth's atmosphere are at work when she is at m'. Note also that when the moon is at M rays from nearly a whole diameter (when the moon is in apogee from a whole diameter) of the sun's disc reach her from a part of the lines $a\,b$ and $a'\,b'$ athwart the Earth's atmosphere. Thus then the sun is converted for the observer at M into a ring of light having a certain definite apparent breadth, which, however, must be very small, because the depth of atmosphere at $a\,b$ or $a'b'$ at work in bringing the sun's diameter into view can be but two or three miles, at the utmost, a range which at the moon's distance would look a mere line. This line would touch the Earth's edge all round at the time of central eclipse.

These relations are further illustrated in fig. 343 which shows how the sun is brought into view. If the Earth's atmosphere were all of the same density and formed a spherical shell, the rays which emanate from any point on the sun and pass round the Earth at E, taking the course A E a through the air, would converge to points such as q and p', a spot such as k being the effective image of the point. (Here would be what is commonly called the 'circle of least confusion.') But the air, diminishing in density upwards, it may be shown that the rays emerging at $b\ a$ and passing towards $c\ c'$ appear to emanate from a focus as f, far beyond the Earth as seen from the moon. Rays passing along A′E′ a' appear in like manner to emanate from a focus as f'. It follows that the sun is converted for the supposed observer on the moon into a ring, situate as at $f\,f'$, not so far away as the sun really is, but much farther away than the Earth.

Fig. 344 shows how the sun is brought into view at the time of central eclipse, the sun and moon being at their mean distances from the Earth. The circle S S′ shows the position of the sun's disc beyond the Earth's dark disc $a\,d\,b\,e$. The Earth seen from the moon has a radius C a about 57′ in apparent length; C S, the sun's semi-diameter, is about 16′, therefore S a about 41′ and S′a about 73′. The terrestrial atmosphere has a refractive power at the sea-level of 34′ corresponding to 68′ for rays passing entirely through the atmosphere so as just to touch the Earth. Thus above a a portion S C A of the sun's diameter is brought into view (by a lower layer of the Earth's atmosphere, probably about 2½ miles deep), S A being about 27′ out of the 32′ subtended by the sun's full diameter S S′. The ring $s\ d\ s'e$ in fig. 344 is about 50 times wider than the solar image thus seen from the moon would appear. But though this ring of light would look like a mere thread, it would be a complete image of the sun, immensely distorted, the greater part of the sun being indeed brought into view twice over. The manner in which the sun would be thus brought into view is indeed very curious, and worth studying, especially as in considering it we shall be dealing with a modified form of the rather curious problem considered in Art. 1065, note. Fig. 346 presents, in a necessarily much exaggerated way, the manner in which the sun is brought into view. I take the case where the Earth is so far

FIG. 344.—Ring of light round the Earth, as seen from the Moon's centre at central Eclipse. S S′ size of Sun's disc.

from the moon (nearly in apogee) that the whole diameter S S′ of fig. 344 is just brought into view. The disc A B E D is supposed to represent the sun beyond the disc of the Earth $e'a'$. The disc A E is larger than the sun really would be; moreover, it is adorned with a series of spots and marks such as are never seen on the sun's face: they are to help the student in identifying the different parts of the sun with the corresponding parts of the distorted image. This image is represented with its real breadth magnified relatively many hundredfold. The sun's centre C is refracted into the circle $c\,c'$. The row of spots A B is distorted into the arc of spots $a\,b$, into which it is raised above the part c of the circle $c\,c'$. The row of

in bending the rays of the sun towards the moon. A knowledge of the actual condition of the air along the zone so determined would in every case,

spots D E is distorted into the arc of spots $d'e'$, into which it is raised on the same side of the

Fig. 345.—Arcs of light on Earth's limb. Eclipse not central.

Earth's disc, this arc of spots being an inverted view of the band of spots D E. But the band of spots A B is also brought into view, distorted and inverted into the arc of spots $a'b'$ below the outline of the Earth's disc; and the band of spots D E is also brought into view, distorted without inversion into the band of spots $d\,e$. It will be seen (since corresponding considerations apply to all parts of the sun's disc) that a greatly distorted image of the whole sun is brought into view in the form of the ring outside the circle $c\,c'$, and an equally or even more distorted and also inverted image of the whole sun between the circle $c\,c'$ and the Earth's disc. The student must imagine the circular band of light outside the Earth in fig. 346 compressed radially into a mere thread-like ring. In fig. 345 the case is considered where the sun is not centrally behind the Earth's disc. The student will have no difficulty in interpreting it. The sun's disc S K S′K′ is distorted into the strangely-shaped image $s\ k\ s'\ k'$ with a fine arc of darkness between the Earth's edge and the sun's disc. In this figure, as in fig. 346, the breadth of the sun's image is greatly exaggerated.

Many fall into the mistake of supposing that because the whole of the sun is brought into view twice over in the ring of light round the Earth, the light received from that ring should be equal to twice the light received from the full sun, or only less than that amount because of absorption in passing through the Earth's atmosphere. As a matter of fact (see note, p. 437) the ring of light seen round the Earth (as supposed to be viewed from the moon) would be no brighter intrinsically than the setting sun; and no more light would come from it than from a portion of the setting sun equal in apparent area to this ring. It is easy to compare this area with the sun's if we know the depth of atmosphere which is effective in refracting the sun's light. Calculations based on the best available formulæ —very doubtful, however, at the best —indicate a probable depth of 2½ miles now. For, in fig. 344, C a represents the 57′ and C S represents 16′, wherefore, putting $a\,s$ = 2½ miles and remembering that

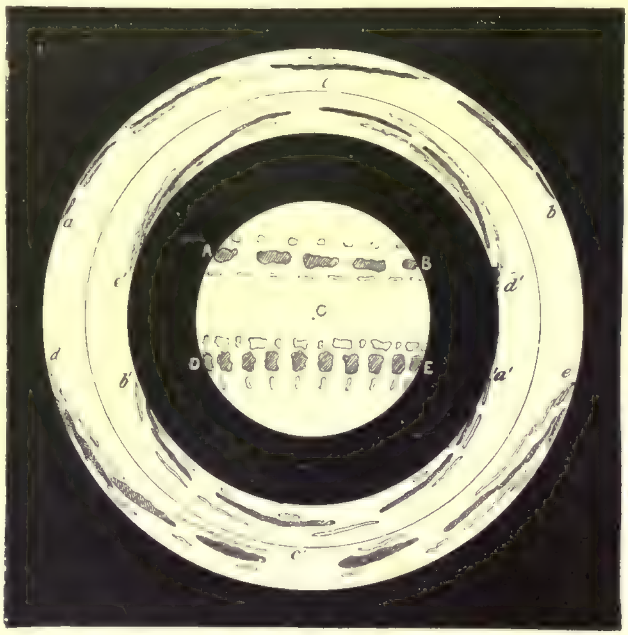


Fig. 346.—The Sun as supposed to be seen from the Moon during a total Lunar Eclipse immensely exaggerated radially.

it may safely be predicted, serve to explain the exact aspect of the moon's disc during totality,—from the brightest aspect which would only be seen when the air was clear over the whole zone, to the almost total disappearance of the moon in a 'black eclipse.'

(1161.) As an illustration of this method of inquiring into the terrestrial conditions under which a lunar eclipse has taken place, and so of interpreting the observed phenomena, the following case dealt with by me in November 1884 may be cited:

It was noticed by the well-known Belgian observer, M. A. de Boë, that at the stage of the lunar eclipse of October 4, 1884, represented in fig. 347, the shadow of the Earth presented a well-marked cusp at a; (the feature is, however, somewhat exaggerated in fig. 347).

$$\mathrm{C}\,a = 3960 \text{ miles, so that, } a\,s = \frac{5}{7920}\,\mathrm{C}\,a.$$

$$\text{ring } s\,e\,s'd' : \text{disc } \mathrm{S\,S'} : : 2\pi\,(57)^2 \cdot \frac{5}{7920} : \pi\,(16)^2$$

$$: : 3249 \times 5 : 3960 \cdot 256$$

$$\text{light from } s\,e\,s'd' : \text{full sunlight} : : 16245 : 1013760$$

$$: : 1 : 62{\cdot}4 \text{ nearly}$$

If we suppose the average absorption affecting the sun's light from the ring $s\,e\,s'd'$ to reduce its intrinsic mean lustre to $\frac{1}{300}$th of the sun's intrinsic lustre, we find the light falling on the moon at the time of central total eclipse to be $\frac{1}{187}$th of the sun's midday light. This is still 33 times the amount of light we get from the full moon. The above assumption is not unreasonable, for if the absorption of the Earth's atmosphere reduces the light of the setting sun to $\frac{1}{18}$th of its original brightness, the light which emerges from double the thickness of atmosphere after grazing the Earth's limb, will be reduced in the ratio 1 to $(18)^2$. Now an object of the moon's apparent size, lit up by full moonlight, when all the surrounding region was in darkness, would be conspicuous; and, therefore, we cannot wonder that the moon, during the time of total eclipse, shines usually with quite a bright light, and sometimes so brightly as to suggest that she is not eclipsed at all. But, of course, if clouds over the whole zone forming the edge of the Earth's disc, as seen from the moon, prevent the passage of the sun's rays through the lower two or three miles of the air, no light can reach the moon; and in such cases the moon is nearly hidden from view during central or nearly central totality. Though no direct sunlight then reaches the moon, the dust floating in the Earth's atmosphere above the region of clouds disperses the sun's light, and an observer on the moon would see the dark Earth surrounded by a comparatively broad bluish ring extending to the height at which dust floats in our atmosphere. The phenomena observed during twilight seems to indicate that such dust (probably meteoric débris) is to be found at a greater height than 40 miles (a hundred miles is probably nearer the mark). So that the Earth, when surrounded by clouds as seen from the moon, will have a zone of bluish light around it—very faint compared with the reddish sunlight which is transmitted through the lower atmosphere when clouds do not interfere—but still brilliant if we may judge of its brightness from the light dispersed by the sky in the immediate neighbourhood of the sun as seen from a mountain-top. Thus during the dark eclipses the moon is usually described as bluish, while during the bright eclipses the moon appears reddish, and the reddish light, owing to unequal absorption at different parts of the Earth's limb, is not evenly distributed over the lunar disc. The bluish light is so faint compared with the reddish transmitted light, that there are many observations of early lunar eclipses in which the moon is stated to have entirely disappeared. But in more recent times (possibly owing to the improvement in telescopes) the body of the moon has not been entirely lost sight of.

The observed shadow into which the moon plunges during its eclipse always appears larger than the geometrical shadow which would be cast by a body as large as the Earth not surrounded by an atmosphere. Mädler estimated this increase of diameter as $\frac{1}{54}$th (see the 'Astronomische Nachrichten' vol. xv. p. 29), and the English 'Nautical Almanac' uses for the purpose of predictions the fraction $\frac{1}{60}$th as representing the increase in the diameter of the observed shadow over the geometrical shadow. The edge of the shadow thrown upon the moon is not perfectly sharp, but the transition from considerable brightness to comparative darkness does not occupy a minute of arc. It is evident that the region which lies outside the geometrical shadow must be illuminated by a solar crescent of sensible thickness, and that the excess of the observed shadow over the geometrical shadow must depend on physiological causes as well as on the rapid decrease in the intensity of light close to the sun's limb.

To interpret this, we first draw a sun view of the Earth (fig. 348) for the time 10h. 2m. Greenwich mean time of mid totality; and by processes of construction which need not be here considered, we obtain first the course of the moon behind the Earth indicated by the arrow, and next:—

(i.)	e_1, the place where, as seen from the sun,	the moon first touches the Earth's disc;
(ii.)	e_2, ,, ,, ,,	the last trace of the moon is seen;
(iii.)	e_3, ,, ,, ,,	the first trace of the moon reappears; and
(iv.)	e_4, ,, ,, ,,	the moon last touches the Earth's disc.

The times for these were respectively:—

	h.	m.	
(i.)	8	15·2	Greenwich mean time (P.M.)
(ii.)	9	15·8	
(iii.)	11	48·8	
(iv.)	12	47·2	

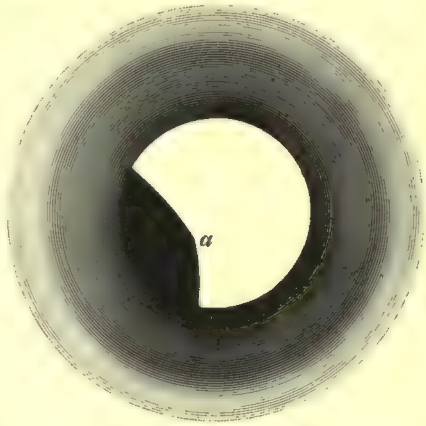


Fig. 347.—Singular appearance presented by the Earth's shadow during the Lunar Eclipse of October 4, 1884.

The time corresponding to the phase shown in fig. 347 was evidently about 8h. 52m. or $1\frac{1}{2}$h. before the time corresponding to the sun view of the Earth in fig. 348. Hence we must throw the Earth back rotationally by an hour and a half's rotation. It will be evident that since e′ is the place of first contact, the point *a*, not far from the middle (but rather south) of the terrestrial arc forming the boundary of the Earth's shadow, corresponded with the region above the western parts of the North Atlantic near the shores of French Guiana and Northern Brazil (near the mouth of the Amazons). Probably cloud-layers floating high above this region towards the time of sunset there, while the air was comparatively clear over the other parts of the Earth's sunset arc on either side of e_2e_2 fig. 348, accounted for the projection at *a*, which was too marked to be regarded as probably a mere effect of irradiation.[1]

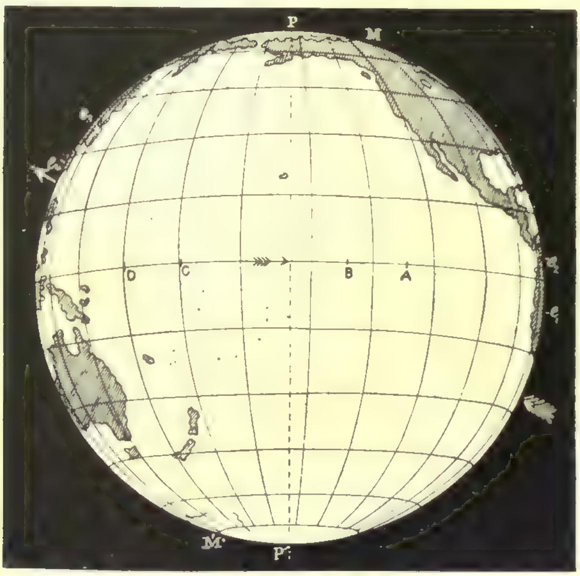


Fig. 348.—Sun view of the Earth at the time of central Eclipse. October 4, 1884, 10.2 P.M. (Greenwich mean time).

(1162.) The absence of colour in the Earth's shadow while the lunar eclipse is partial, and the rapid assumption of a ruddy tinge so soon as the eclipse becomes total, has led some students of astronomy to suggest strange theories about the moon's condition. Some have imagined an auroral light playing over parts of the moon's surface.

[1] As supposed to be observed from the moon, the night face of the Earth shown in fig. 349 would be that turned moonwards at the time of central eclipse, rotation taking place in the direction shown by the arrow, and the points e_1, e_2, e_3, e_4, corresponding with those similarly marked in fig. 348.

Others, noting that the intensity of the redness varies during the progress of totality, have imagined changes in the actual condition of the moon's surface taking place so rapidly as to be recognised even during the short duration of a lunar eclipse. These suggestions are altogether unnecessary. The known physiological laws of optics explain perfectly why so long as any portion of the fully illuminated surface of the moon remains in view, the part in shadow being so much less brightly illuminated can show no trace of colour. That the seeming blackness of the shadow is purely an illusion is shown further by the circumstance that if the shadowed part is viewed through a telescope, the white part of the disc being kept out of the field of view, the darkened part only fails to show a ruddy tinge when the totally eclipsed moon itself is not ruddy. The changes of colour during the progress of totality depend partly on the fact that near the edge the true shadow is much lighter than near the middle, and the immersion of the moon in the deepening shadow produces an effect akin to that resulting from the passage of the unshadowed part into the shadow, only not quite so marked. But these changes probably depend also in some degree on the Earth's rotation, bringing different parts of the Earth's atmosphere into action to refract the sun's light on the moon, and on changes taking place in the condition of the atmosphere thus active.[1]

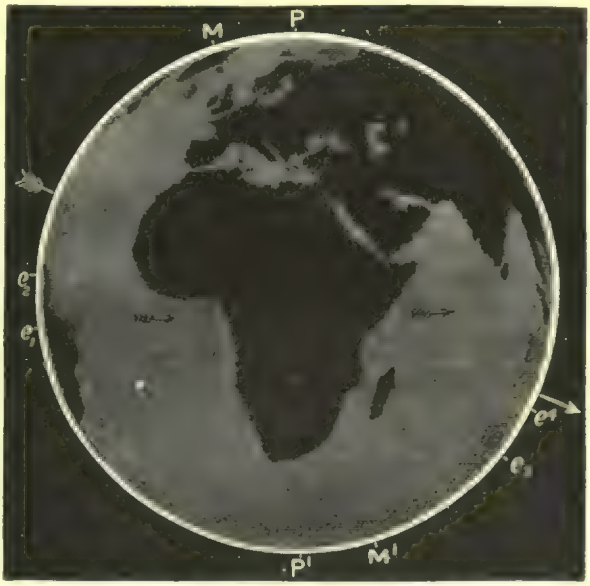


Fig. 349.—Earth's face turned moonwards at the time of central eclipse. October 4, 1884, 10.2 P.M. (Greenwich mean time).

(1163.) Photometric measurements by Zöllner show that the light of the full moon is between $\frac{1}{618,000}$th and $\frac{1}{619,000}$th of the sun's light, sun and moon being supposed at their mean distance. From the way in which the total amount of light varies with the moon's varying phases, he found (since the change cannot be ascribed to any cause but the irregularities of the moon's surface) that the average slope of the lunar inequalities (large and small) is about 52°, the light reflected from the full moon being much greater than would be reflected from a smooth but unpolished globe of the same size and the same average light-reflecting quality. Correction being duly made on account of the evidence thus given by the moon in her varying phases, it

[1] The fanciful suggestion that the moon's surface heated by the sun's action may glow with a dull red light needs of course no serious refuting here. If the fancy were needed to explain the ruddy colour of the moon, and if it were inherently possible, it would be disproved by the black or nearly black lunar eclipses which occur from time to time. But it is not needed at all, and is moreover wholly impossible.

appears that the average light-reflecting quality of her surface (its *albedo*—see note to Art. 1035), is 1,736 where absolute whiteness is represented by 10,000. This is about the 'whiteness' of grey weathered sandstone. But it is obvious that different parts of the moon have widely different degrees of whiteness. The brighter parts of the moon's surface can be compared with the whitest terrestrial rocks.[1] On the other hand, the darkest parts of the moon are as dark as our syenites and porphyries, perhaps even darker.

(1164.) With regard to the colours of different parts of the moon's surface, we do not know nearly so much as we might, if carefully conducted researches were directed to the inquiry. It would be clear from the apparent whiteness of the moon, even if it could not be inferred from physiological considerations, that the eye cannot properly recognise colour under the conditions existing when we study the moon's surface. What the naked eye cannot detect in the whole disc of the moon (which looks white, but is certainly not white) the telescope cannot enable us to determine in dealing with parts of the disc. We can recognise indeed faint traces of tinting, though the tints detected are usually only those classified as neutral; but the actual colouring of different parts of the moon's surface has not been nearly so accurately determined as it might be were proper measures taken and suitable appliances employed. Direct telescopic observations made through a series of tinted glasses, would show (by changes in the relative brightness of different parts) where different tints prevail. Photographs made under different conditions, with different processes—from those which are chiefly affected by the deepest red rays to those ordinarily used which are chiefly affected by rays from the violet part of the spectrum—would still more effectively indicate the colours and strengths of colour in different parts of the moon's surface.[2]

(1165.) The fact that the low-lying parts of the moon are much darker than the higher regions is full of meaning, though hitherto its significance does not seem to have been much noticed. Either we must assume that

[1] Zöllner states as the result of his photometric researches, 'dass der Mond an seinen helleren und hellesten Stellen aus einem Stoffe besteht, der, auf die Erde gebracht, zu dem weissesten der uns bekannten Körper gezählt werden würde.'

[2] We have the means of estimating in some degree the value of such researches by comparing the relative intensities of different parts of a photograph of the moon with the relative intensities of those parts as seen in the usual way with the telescope. Noting, for instance, that the so-called seas are much darker in the photograph than they appear in the telescope, we can infer that they are relatively ruddy in colour, since in ordinary photography the rays from the red end of the spectrum are ineffective. On the other hand, observing that the 'rays' show much better in lunar photographs than as observed with the telescope, we conclude that those parts of the moon's surface reflect relatively more of the so-called actinic rays than the rest—which is not the same as saying that they are bluish or violet, and does not indeed amount to a definite determination of their tint; they may merely be whiter than the neighbouring surface which in that case must possess a certain ruddiness of tint, or there may be no ruddiness of tint in the neighbouring surface but a certain bluishness of tint in the rays.

these lower regions, the so-called seas (certainly now dry), are old sea-bottoms, and owe their darkness to the quality of the matter deposited there in remote ages, or else we must suppose that the matter which last remained fluid when the moon's surface was consolidating was of darker material than the rest. For such matter would occupy the lowest lunar regions. There can be very little doubt which of these two explanations is the more reasonable. It seems as nearly demonstrated as a matter of the sort well can be that millions of years ago the moon had seas, occupying the regions now dark and at the low level corresponding to sea-floors—and that with the progress of time these seas disappeared beneath the moon's surface—not solely by soaking in where they had existed as seas, but by being withdrawn wherever their waters fell as rain or ran in rivers on the upraised or continental parts of the moon's surface.

(1166.) The map of the moon, Plate XXIII. will serve to show the relative position and dimensions of the various features, seas, so-called craters, mountain-ranges, &c., mentioned in what follows.[1]

In considering the general results of the telescopic scrutiny of the moon, it is well to remember the circumstances under which such scrutiny has been made.

The highest power yet applied to the moon (a power of about six thousand) brings her, so to speak, to a distance of forty miles—a distance far too great for objects of moderate size to become visible. Many of my readers have probably seen Mont Blanc from the neighbourhood of Geneva, a distance of about forty miles. At this distance the proportions of vast snow-covered hills and rocks are dwarfed almost to nothingness, extensive glaciers are quite imperceptible, and any attempt to recognise the presence of living creatures or of their dwellings (with the unaided eye) is utterly useless. But even this comparison does not present the full extent of the difficulties attending the examination of the moon's surface with our highest powers. The circumstances under which such powers are applied are such as to render the view much less perfect than the mere value of the magnifying power employed might seem to imply. We view celestial objects through tubes placed at the bottom of a vast aërial ocean, never at rest through any portion

[1] I formed it by reducing a map which the late Mr. Webb had reduced from a map by Mädler. Only four hundred objects are numbered instead of five or six hundred (I know not the actual number) in later editions of his excellent *Celestial Objects for Common Telescopes.* A line had to be drawn somewhere, and it seemed reasonable to draw it so as to exclude all names belonging to persons now living -not that this would have been necessary had something like sense been shown in nomenclature. But when Mr. Birt (a zealous student of the moon) began to name lunar features after personal friends altogether unknown in the scientific world (he gave six craters to a family of Gwynnes, Grylls, or Griggs, or the like), and others followed suit, it seemed better to return to the older list, especially as that was more than sufficient for all practical purposes. Not one lunar student in a hundred uses half or a fourth of even the 400 names given in the text.

of its depth; and the atmospheric undulations which even the naked eye is able to detect are magnified just in proportion to the power employed.

(1167.) The first features noticed in the moon are the broad dark tracts, the so-called seas on the moon. They have been called the grey plains, but are more probably somewhat ruddy (Art. 1160, note). The uniformity of curvature which marks their surfaces as a whole shows that they were once in a liquid condition; but their solidification would not be likely to have resulted in a smooth surface. On the contrary, it is probable that the true surface is marked with corrugations, or crystalline formations, or other uniform unevennesses, if one may so speak. It is a noteworthy circumstance that the lunar plains do not form portions of the same sphere, some lying deeper than others,—that is, belonging to a sphere of smaller radius.

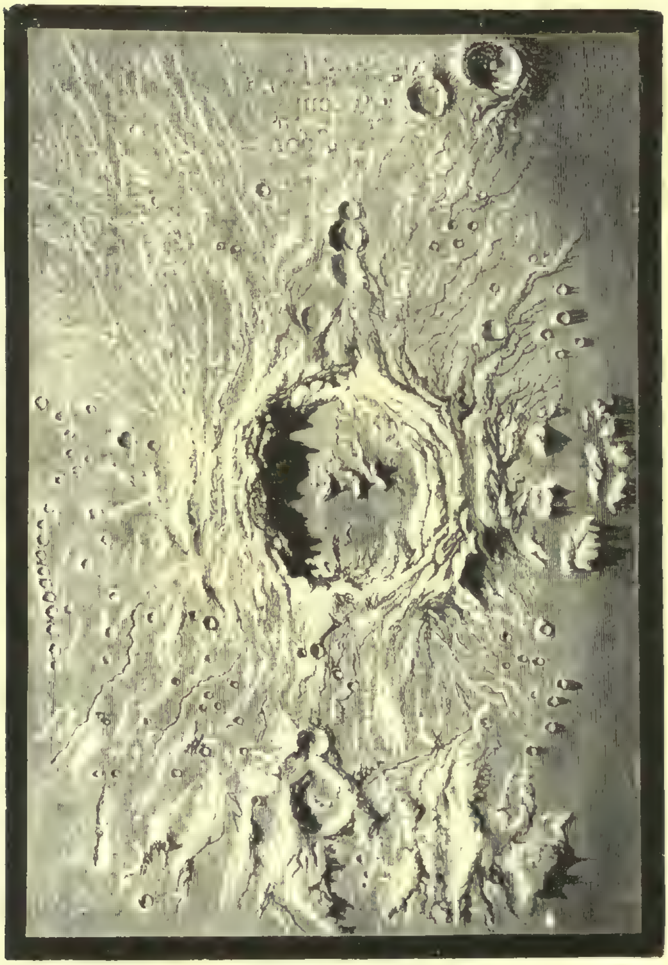


Fig. 351.—The lunar crater Copernicus, from a drawing by *Nasmyth.*

(1168.) After the dark plains the crater-shaped mountains are the most striking features on the moon. They may be conveniently divided into walled or bulwarked plains, ring mountains, craters, and saucer-shaped depressions or pits. The first are in many cases of immense size, being in some cases more than a hundred miles in diameter. The plain inside the crater ring somewhat resembles the so-called seas. Their apparent colour has been regarded as suggestive of some kind of vegetation, but it is probably rather red than green in reality, and it is antecedently unlikely or rather more than unlikely, that there can be vegetation where there is probably scarcely any air and certainly no water.[1]

[1] It has been suggested that a shallow stratum of carbonic acid gas, the frequent product of volcanoes and long surviving their activity (for instance, among the ancient craters of Auvergne, where it exists in great quantity), may in such situations support the life of some kind of plants; but the idea will not bear examination. Under the weak action of lunar gravity no atmospheric layer could possibly be shallow, and it is physically impossible that carbon dioxide could outlast the gases which must have formed the main portion of the lunar atmosphere as of our own. The

PLATE XXII

A portion of the Moon's surface from a model by Mr Nasmyth.

PLATE XXIII.

Map of the Moon, reduced from Mr. Webb's Miniature of Mädler's Great Map.

THE DARK TRACTS, OR SO-CALLED SEAS.

A, Sea of Conflicts. B, Humboldt's Sea. C, Sea of Cold. D, Lake of Death. E, Lake of Dreams. F, the Marsh of a Dream. G, the Sea of Tranquillity. H, the Sea of Serenity. I, the Marsh of Clouds. K, the Marsh of Corruption. L, Sea of Vapour. M, Middle Bay. N, Bay of Heat. O, Sea of Showers. P, Bay of Rainbows. Q, Ocean of Storms. R, Bay of Dew. S, Sea of Clouds. T, Sea of Moisture. V, Sea of Nectar. X, Sea of Fertility. Y, Smyth's Sea. Z, the South Sea.

LIST OF LUNAR OBJECTS SHOWN ON WEBB'S MAP

1. Promontorium A-
2. Alhazen [garum
3. Eimmart
4. Picard
5. Condorcet
6. Auzout
7. Firmicus
8. Apollonius
9. Napier
10. Schubert
11. Hansen
12. Cleomedes
13. Tralles
14. Oriani
15. Plutarch
16. Seneca
17. Hahn
18. Berosus
19. Burckhardt
20. Geminus
21. Bernoulli
22. Gauss
23. Messala
24. Schumacher
25. Struve
26. Mercury
27. Endymion
28. Atlas
29. Hercules
30. Oersted
31. Cepheus
32. Franklin
33. Berzelius
34. Hooke
35. Strabo
36. Thales
37. Gärtner
38. Democritus
39. Arnold
40. Chr. Mayer
41. Meton
42. Euctemon
43. Scoresby
44. Gioja
45. Barrow
46. Archytas
47. Plana
48. Mason
49. Baily
50. Burg
51. Mount Taurus
52. Römer
53. Le Monnier
54. Posidonius
55. Littrow
56. Maraldi
57. Vitruvius
58. Mount Argæus
59. Macrobius
60. Proclus
61. Pliny
62. Ross
63. Arago
64. Ritter
65. Sabine
66. Jansen
67. Maskelyne
68. Mount Hæmus
69. Promontorium Ac-
70. Menelaus [herusia
71. Sulpicius Gallus
72. Taquet
73. Bessel
74. Linné
75. Mount Caucasus
76. Calippus
77. Eudoxus
78. Aristotle
78. Egede
80. Alps
81. Cassini
82. Theætetus
83. Aristillus
84. Autolycus
85. Apennines
86. Aratus
87. Mount Hadley
88. Conon
89. Mount Bradley
90. Mount Huygens
91. Marco Polo
92. Mount Wolf
93. Hyginus
94. Triesnecker
95. Manilius
96. Julius Cæsar
97. Sosigenes
98. Boscovich
99. Dionysius
100. Ariadæus
101. Silberschlag
102. Agrippa
103. Godin
104. Rhæticus
105. Sömmering
106. Schröter
107. Bode
108. Pallas
109. Ukert
110. Eratosthenes
111. Stadius
112. Copernicus
113. Gambart
114. Reinhold
115. Carpathian Mts.
116. Gay Lussac
117. Tobias Mayer
118. Milichius
119. Hortensius
120. Archimedes
121. Timocharis
122. Lambert
123. La Hire
124. Pytheas
125. Euler
126. Diophantus
127. Delisle
128. Carlini
129. Helicon
130. Kirch
131. Pico
132. Plato
133. Harpalus
134. La Place
135. Heraclides
136. Maupertuis
137. Condamine
138. Bianchini
139. Sharp
140. Mairan
141. Louville
142. Bouguer
143. Encke
144. Kepler
145. Bessarion
146. Reiner
147. Marius
148. Aristarchus
149. Herodotus
150. Wollaston
151. Lichtenberg
152. Harding
153. Lohrmann
154. Hevelius
155. Cavalerius
156. Galileo
157. Cardan
158. Krafft
159. Olbers
160. Vasco de Gama
161. Hercynian Mts.
162. Seleucus
163. Briggs
164. Ulugh Beigh
165. Lavoisier
166. Gerard
167. Repsold
168. Anaxagoras
169. Epigenes
170. Timæus
171. Fontenelle
172. Philolaus
173. Anaximenes
174. Anaximander
175. Horrebow
176. Pythagoras
177. Œnopides
178. Xenophanes
179. Cleostratus
180. Tycho
181. Pictet
182. Street
183. Sasserides
184. Hell
185. Gauricus
186. Pitatus
187. Hesiod
188. Wurzelbauer
189. Cichus
190. Heinsius
191. Wilhelm I.
192. Longomontanus
193. Clavius
194. Deluc
195. Maginus
196. Saussure
197. Orontius
198. Nasir-ed-din
199. Lexell
200. Walter
201. Regiomontanus
202. Purbach
203. Thebit
204. Arzachel
205. Alpetragius
206. Promontorium Æ-
207. Alphonsus [narium
208. Ptolemy
209. Davy
210. Lalande
211. Mösting
212. Herschel
213. Bullialdus
214. Kies
215. Guericke
216. Lubienitzky
217. Parry
218. Bonpland
219. Fra Mauro
220. Riphæan Mts.
221. Euclid
222. Lansberg
223. Flamsteed
224. Letronne
225. Hippalus
226. Campanus
227. Mercator
228. Ramsden
229. Vitello
230. Doppelmayer
231. Mersenne
232. Gassendi
233. Agatharchides
234. Schiller
235. Bayer
236. Rost
237. Hainzel
238. Capuanus
239. Schickard
240. Drebbel
241. Lehmann
242. Phocylides
243. Wargentin
244. Inghirami
245. Bailly
246. Dörfel Mts.
247. Hausen
248. Segner
249. Weigel
250. Zuchius
251. Bettinus
252. Kircher
253. Wilson
254. Casatus
255. Klaproth
256. Newton
257. Cabeus
258. Malapert
259. Leibnitz Mts.
260. Blancanus
261. Scheiner
262. Moretus
263. Short
264. Cysatus
265. Gruemberger
266. Billy
267. Hansteen
268. Zupus
269. Fontana
270. Sirsalis
271. Damoiseau
272. Grimaldi
273. Riccioli
274. Cordilleras
275. D'Alembert Mts.
276. Rook Mts.
277. Rocca
278. Crüger
279. Byrgius
280. Eichstädt
281. Lagrange
282. Piazzi
283. Bouvard
284. Vieta
285. Fourier
286. Cavendish
287. Réaumur
288. Hipparchus
289. Albategnius
290. Parrot
291. Airy
292. La Caille
293. Playfair
294. Apianus
295. Werner
296. Aliacensis
297. Theon sen.
298. Theon jun.
299. Taylor
300. Alfraganus
301. Delambre
302. Kant
303. Dollond
304. Des Cartes
305. Abulfeda
306. Almamon
307. Tacitus
308. Geber
309. Azophi
310. Abenezra
311. Pontanus
312. Sacrobosco
313. Pons
314. Fermat
315. Altai Mts.
316. Polybius
317. Hypatia
318. Torricelli
319. Theophilus
320. Cyrillus
321. Catherine
322. Beaumont
323. Isidore
324. Capella
325. Censorinus
326. Taruntius
327. Messier
328. Goclenius
329. Biot
330. Guttemberg
331. Pyrenees
332. Bohnenberger
333. Colombo
334. Magelhaen
335. Cook
336. Santbech
337. Borda
338. Langrenus
339. Vendelinus
340. Petavius
341. Palitzsch
342. Hase
343. Snellius
344. Stevinus
345. Furnerius
346. Maclaurin
347. Kästner
348. La Pérouse
349. Ansgarius
350. Behaim
351. Hecatæus
352. W. Humboldt
353. Legendre
354. Stöfler
355. Licetus
356. Cuvier
357. Clairaut
358. Maurolycus
359. Barocius
360. Bacon
361. Buch
362. Büsching
363. Gemma Frisius
364. Poisson
365. Nonius
366. Fernelius
367. Riccius
368. Rabbi Levi
369. Zagut
370. Lindenau
371. Piccolomini
372. Fracastorius
373. Neander
374. Stiborius
375. Reichenbach
376. Rheita
377. Fraunhofer
378. Vega
379. Marinus
380. Oken
381. Pontécoulant
382. Hanno
383. Fabricius
384. Metius
385. Steinheil
386. Pitiscus
387. Hommel
388. Vlacq
389. Rosenberger
390. Nearchus
391. Hagecius
392. Biela
393. Nicolai
394. Lilly
395. Jacobi
396. Zach
397. Schomberger
398. Boguslawsky
399. Boussingault
400. Mutus

(1169.) Fig. 351 represents the lunar crater Copernicus, as drawn by Nasmyth. It aptly illustrates the appearance of large craters when seen with powerful telescopes. This crater, one of the finest on the moon, and crossing the summit of an upraised region about 300 miles across, is 56 miles in diameter. Its central mountain is 2,400

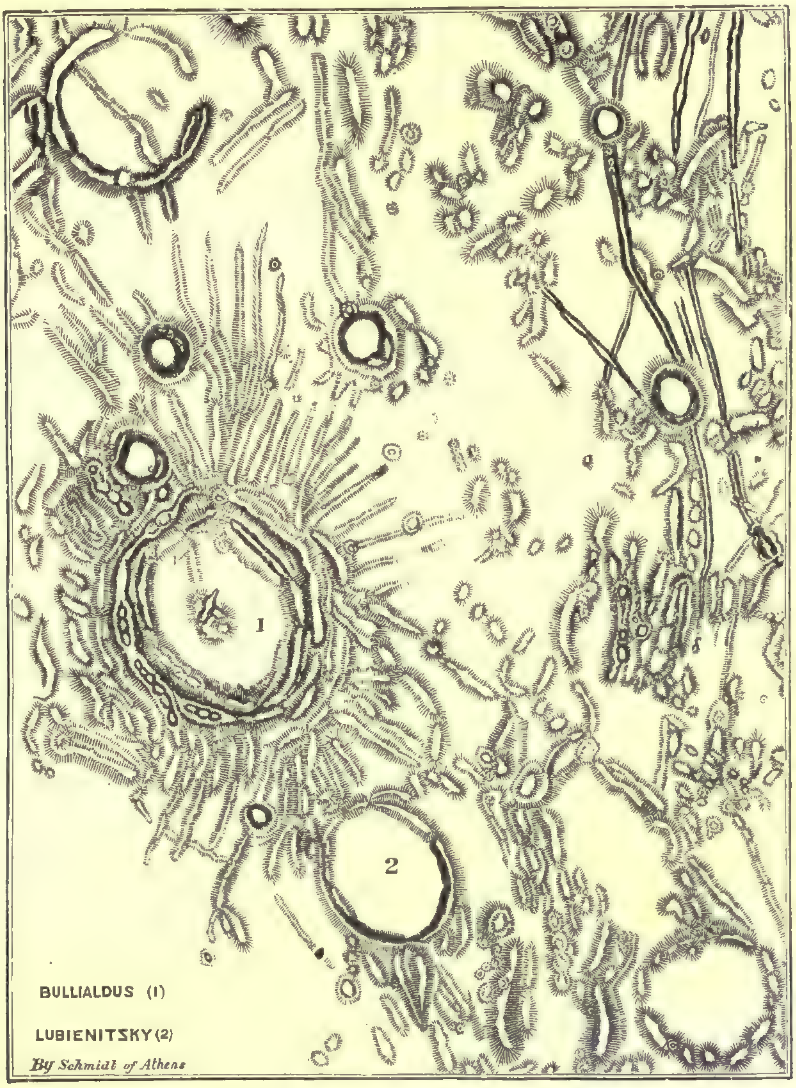


FIG. 352.—Portion of a Lunar Map by Schmidt of Athens, showing the craters Bullialdus and Lubienitzky.

feet in height; it is surrounded by several concentric rings, composed of distinct heights separated by ravines; the highest part of the ring is on the west, where it

bottomsof lunar craters are sometimes flat, sometimes concave, sometimes convex. Often we can recognise the signs of later disturbances in ridges, hills, minute craters, or in central hills seldom attaining the height of the wall. The ring is usually steepest within, as in terrestrial craters, and sometimes ridged into terraces. Small corrugations often run athwart the outer slopes of the ring; the wall is often broken by great masses like towers along bulwarks, or by openings like gateways.

runs, according to Schmidt, to a general height of nearly 12,800 feet, with a peak of 13,500 feet. The inclination in some places amounts to 60°. Copernicus begins to be lit up by sunlight a day or two after first quarter.

(1170.) Schmidt's map of Bullialdus and the neighbourhood (fig. 352) also well illustrates the nature of the lunar crateriform mountains of various dimensions. But yet further insight into the characteristics of the more disturbed and uneven portions of the moon's surface will be obtained from the study of Plate XXII., which represents a very rough and volcanic portion of the moon's surface, as modelled from telescopic observations by Mr. Nasmyth.

We can recognise here as elsewhere on the moon the action of a succession of eruptions, the largest being the older craters. Probably the record of the region illustrated in Plate XXII. represents the work of hundreds of thousands of years.

(1171.) The mountain-chains on the moon are seen under circumstances which enable us to recognise none but the boldest features of these formations. It is as unsafe to theorise as to their geological or selenological conformation as it would be to speculate on the structure of a mountain-range on earth which had only been seen from a distance of two or three hundred miles. Among them we find most of the forms recognised in terrestrial mountains, ranges of great height and extent, others flattened into plateaux intersected by ravines, others broken by detached peaks or by crowded hillocks. Most of the mountain-chains show like those on the Earth a greater steepness along one side. Detached peaks and mountain masses are found on the plains, some of them of great height and steepness. We find all classes of hills, cliffs, and ridges; among the last may be specially mentioned long narrow banks (see fig. 352), extending along level surfaces for great distances—and probably indicating the formation of fissures, and the extension of molten matter through them in long past ages.

(1172.) The next feature which attracts attention is the existence of radiating streaks from certain craters. The most remarkable system of rays is that which has the great crater Tycho (Plate XXIII.) as its centre. This system can be recognised over a very large proportion of the visible hemisphere, and doubtless extends on the south (the uppermost part of the maps and photographs) far upon the unseen hemisphere; one of them is visible through a range of 1,850 miles. The photographs of the moon exhibit the radiating bright streaks from Tycho, Copernicus, Kepler, and Aristarchus; other less striking systems can be recognised, and the telescope shows others. The streaks sometimes extend from a circular dark region round a crater, sometimes from a tract within it and near its centre. All the lunar mountains, valleys, rings, and craters are traversed—without any change of shape or contour by the rays—showing that those irregularities must all be of later formation. The rays are best seen when the moon is full, though faintly visible at other times. Since it is obvious that the original surface of these radiating bands was flat before new disturbances broke it up by throwing craters, ridges, mountains, &c., across it, we may probably infer that that surface was formed of matter still molten, poured out through rifts in the lunar crust and spreading out after extension, solidifying in broad stripes of flat surface—flat, but not necessarily smooth; indeed, more probably, crystalline in structure.

The lunar valleys include formations as remarkable as the long banks described above—namely, the *clefts* or *rills*, furrows extending with perfect

straightness for long distances, and changing in direction (if at all) suddenly, thereafter continuing their course in a straight line. These were first noticed by Schröter, and a few were discovered by Gruithuisen and Lohrman; but Beer and Mädler added greatly to the known number, which was raised by their labours to 150. Schmidt has discovered nearly 300 more. These furrows pass athwart plains extending chiefly between craters. The way in which they intersect crater-walls and rings proves that, unlike the 'rays,' which are older than the craters, the 'rills' are of later formation. In some cases we find them apparently passing under the walls of craters or beneath mountain masses as if by tunnelling. But beyond all reasonable doubt they are natural formations. They range from eighteen to ninety-two miles in length, and from half a mile to a mile in width. They may be regarded as cracks formed in the moon's crust in the later stages of its shrinking.

(1173.) The moon is now so utterly unlike the Earth that it is difficult to imagine that there was ever even such general resemblance as is implied in regarding her as a planet. She is not only arid and airless, but even were she clothed with sea and air she would yet differ greatly from the Earth because of her long day of more than four weeks. The moon's characteristic peculiarities, however, telling us as they do of her immense age, enable us more readily to understand her condition. How long must the moon have existed as a planet to have grown old, decrepit, and dead, as we see that she has?

(1174.) There is no difficulty in understanding that, even if formed as long ago, or later, the moon would have been much older than the Earth. With 81 times as much mass and only $13\frac{1}{2}$ times as large a surface, our Earth would have cooled through the various stages of her life much more slowly,—in fact, each stage would have lasted just as much longer as 81 exceeds $13\frac{1}{2}$, or be six times as long. Suppose the Earth and moon both white-hot 60,000,000 years ago, then the moon would have reached the Earth's present stage 50,000,000 years ago, and would correspond now to 300,000,000 years of Earth-life,—so that the moon would tell us of the Earth's condition 240,000,000 years hence. And though this result is based on assumptions, it yet presents truly the general inference we may safely form that the Earth will not be in the same stage of planetary life as the moon until many millions of years have passed. (If each stage of the Earth's life is six times as long as the corresponding stage of the moon's, then —on any assumption whatever—the Earth will only reach the moon's condition after a period five times as long as the interval which has elapsed since they were both simultaneously in the same stage of planetary life.)

(1175.) But even with this knowledge it remains difficult to understand why the moon should be so unlike the Earth. The waters of the Earth may soak their way beneath the crust (as our underground caves, and even our hot wells and volcanic outbursts, show they are doing) till they all disappear. Our air can hardly, however, become thinned to the condition of the lunar air. And even if it did, and every trace

of water had vanished, the Earth would not be as the moon is. There are no great craters on the Earth as on the moon: there are scarcely any great mountain ranges on the moon as on the Earth. In these chiefly, but in other important respects also, the moon and Earth are so unlike that the uniformitarian theory here appears to fail us.

The more the moon is studied, the clearer seems to be the evidence that she gives respecting the life-history of a planet. She tells us more, perhaps, of the future of our Earth than of the past; but she tells us of the past too. That the moon is waterless and practically airless too, now, is certain, and, therefore, there is probably no life now on her surface, though for those who like such fancies the belief is always open that there may be creatures on the moon utterly unlike any with which we are acquainted on Earth. Yet the moon's face tells us of a remote youth—a time of fiery activity, when volcanic action even more effective (though not probably more energetic) than any which has ever taken place on this globe upheaved the moon's crust.

So soon as we consider carefully the features of her surface we see that there must have been three well-marked eras of vulcanian activity. The systems of radiating streaks tell us of that stage of the moon's history when her still hot and plastic crust parted with its heat more rapidly than the nucleus of the planet, and so, contracting more quickly, was rent by the resistance of the internal matter, which, still hot and molten, flowed into the rents, and spreading formed the long broad streaks of brighter surface. The next stage of the moon's history (after many thousands, perhaps millions, of years had passed) was one in which the cooled crust, still plastic, contracted little, while the still hot nucleus contracted steadily, so shrinking from the crust, which under the action of gravity closed in upon the nucleus in such sort as to form a wrinkled or corrugated surface. The third era of lunar vulcanian disturbance was that of mighty volcanic eruptions, during which the great craters were formed which are so numerous on the lighter tinted higher regions of the moon's surface.

(1176.) We can never be absolutely certain, indeed, that the moon or any other planet is made of exactly the same materials as the Earth. Still less safely can we assume that those materials are similarly proportioned. Indeed, we have every reason to believe that there is considerable difference between the constitution of the moon and that of our Earth in regard to the proportions in which the various elements are distributed. Still we shall probably not be very far wrong in assuming that the surface of the moon, as we see it, consists of rocks such as form the surface of the Earth. We may probably infer also that the quantity of air and water above each square mile of the moon's surface bore the same proportion ($\frac{1}{6}$th) to the quantity above each square mile of the Earth's that we have already obtained for the durations of corresponding stages of the lives of the two planets. For the moon would probably (at any the same part of her life) have had $\frac{1}{81}$st part of the actual quantity of air and of water spread over a surface $\frac{2}{27}$ths (that is, six times $\frac{1}{81}$st) of the Earth's, and this would give her but $\frac{1}{6}$th part of the quantity of air and water possessed by the Earth per square mile. The air being drawn downwards with only $\frac{1}{6}$th the force of terrestrial gravity, and being only $\frac{1}{6}$th as much in quantity, would have but $\frac{1}{36}$th of the density of our air at the sea-level.

(1177.) Consider, now, the result of a comparison between those stages of the Earth's life whose records remain more or less completely in her strata, and the corresponding stages of the life of the moon. The problem is exceedingly complex, and is affected by a number of considerations, the data relating to which are but little

known to us. Still it is not an unfair assumption to say that since terrestrial exceeds lunar gravity six times, therefore subaerial denudation on the Earth, in so far as it depends on the energy of gravity, must be about six times as great as on the moon. And again, it is fair to assume that the sixfold supply of water per square mile on the Earth must have resulted in something like six times as much denudation being accomplished on the Earth (per square mile) as on the moon during corresponding parts of the life-history of each planet. So with the inferior density of lunar air. While, lastly, the relatively shorter duration of corresponding stages of the moon's life must necessarily have caused the work of denudation to have been less (per square mile, always), in corresponding degree.

We shall not exceed the truth, and shall probably be far within it, if we assume that at least one thousand times as much denuding work has been accomplished on each square mile of the Earth's surface as on each square mile of the moon's.

(1178.) Such a conclusion as this, while it enables us to understand much of the moon's history, as recorded on her face, which had before seemed inexplicable, enables us also to read in her record much that once existed in the record of the Earth, but has long since been removed. A much larger proportion of the terrestrial record has been destroyed than of the moon's. Whatever in the moon's record relates to the general life-history of a planet, as distinguished from those special details in which one planet may be supposed to differ from another, we may read in the undestroyed pages of the lunar record almost as readily as we might have read them in the corresponding pages of the Earth's record had these pages remained. If we inquire in what respects the moon differs most markedly from the Earth, we shall probably find evidence as to the remote past of the Earth's history.

(1179.) The moon's surface differs chiefly from the Earth's, as its surface would be if all the waters were removed, in three respects:

1. In the great system of rays.
2. In the much greater relative importance of craters as compared with craters on the Earth, and
3. In the relative insignificance of the lunar mountain ranges.

(1180.) Rays (explaining them as in Art. 1172) tell us of a stage in the vulcanian history of our Earth which has not only long since passed, but of which nearly every trace has long since been swept away by the action of the waves of the ocean and by all the multitudinous processes of subaerial denudation.[1] The moon, in showing these radiating streaks, and in affording at the same time evidence that they are much more ancient than all other features of the moon's surface, gives us an instructive lesson respecting a portion of the past geological history of the Earth of which scarcely any evidence remains in her own crust. In the cores of some of our great mountain ranges, indeed, the geologist can still recognise the wrecked remnants of the masses formerly extruded through the immense fissures which resulted from the resistance of the nuclear mass as the crust shrank. Strangely enough, it is through processes of denudation which in the main have destroyed these masses that those which remain have been first preserved and afterward disclosed. Covered over by deep layers of deposited matter, such portions as still remain have been at once con-

[1] This corresponds with the theory of Mallet and other vulcanologists, that the very earliest process of Earth contraction resulted in the breaking of the crust along fissures hundreds of miles in length, and the extrusion of molten matter through the openings thus formed.

cealed and preserved during hundreds of thousands of years, but later denuding processes have cut their way through the overlying matter, disclosing the ancient core belonging to the very first stage of mountain formation.

(1181.) The great lunar craters which are existing features of the Earth speak still more clearly about the Earth's past history. From their enormous span, their circular form, and their comparatively small but absolutely great height, we learn that they belong to an early era in the moon's vulcanian history, though not to an era quite so remote as that of the formation of the radiating streaks. Only the existence of great heat and vast expansive force could explain the extension of volcanic action from and around centres in such sort as to produce circular crater-rings forty or fifty, sometimes eighty or ninety, miles in diameter. But, as a matter of fact, if the lunar craters throw light on the Earth's past history, the study of the Earth's crust in turn enables us to understand better than we otherwise should the chief features of the moon's contour. We may recognise in some of the flat, circular floors of the great lunar craters the solidified surface of what were once immense lakes of molten lava, akin to the great lake of fire, Kilauea, in the Sandwich Isles. But in other cases we have probably crater-rings formed by the ejection of volcanic matter, cinders, lapilli, dust, and so forth, in the days when the moon's volcanic energies were still potent.

(1182.) Seeing that the great craters belong to the comparative youth of a planet's vulcanian life, we can easily understand why such craters remain on the moon and have disappeared from off the surface of the Earth. They have been destroyed by the long-continued action of subaerial denudation on the Earth, but on the moon there has not been time, nor have the forces at work been energetic enough to destroy these grand manifestations of her former subterranean energies. It would be a mistake to suppose that no such craters were ever formed on the Earth. The subterranean forces have indeed to work against the sixfold greater energy with which terrestrial gravity, as compared with lunar gravity, holds down the materials of the crust; but the self-same excess of gravitating energy gives to the Earth's subterranean forces correspondingly greater strength. For all the vulcanian energies of a planet are directly or indirectly the product of the energy of gravity possessed by the planet's mass. Craters as large as the moon were not wanting on the Earth in long past times. But hundreds of thousands, nay millions, of years of denuding action have worn them down, till now scarcely any traces of their past existence can be recognised except by the geologist. He, however, can see, in what to the ordinary eye appeared merely as a few scattered hills, the basal wrecks of immense volcanic craters. He had already interpreted in this way the hill ranges of Mull and Skye, for example, in the Hebrides. Here are the wrecks of immense volcanic mountains, formerly some twelve thousand feet high and thirty miles wide in Mull, still higher and a mile or two wider in Skye. On the moon's surface remain, still extant, still retaining their ring form, such craters as have not existed on the Earth during millions of past years.

(1183.) The interest of our lunar lessons about the Earth's past culminates when we consider the significance of those few ranges of mountains which exist on the moon. We see at once that we cannot explain the lunar mountain ranges as due to the disappearance of elevations formerly existing. If the moon had ever had mountain ranges corresponding with the Himalayas and the Andes on the Earth, such ranges would still exist upon her airless and waterless surface. Ranges as important as either of these may have existed on the Earth in past ages and have since disappeared.

Indeed we know that the very size of the Himalayas and Andes attests their comparative youth. The Dofrefjelds in Norway are much less elevated, because they are much older and have experienced much more wear and tear, and still older mountain ranges which may formerly have towered to even greater heights have doubtless long since yielded to the denuding forces which reside in air and water worked by the mighty hand of terrestrial gravity. But we know that on the moon the denuding forces were so much weaker and acted during so much shorter a time that assuredly it would not be because of their action that fewer and less lofty mountain ranges would exist on the moon than on the Earth.

(1184.) The answer to what at first seems enigmatical is seen to be altogether simple so soon as we consider what mountain ranges are. So long as geologists regarded them as mere products of upheaval, one could recognise no reason why the mountain ranges on the moon should not be relatively as numerous and as important as those on the Earth. But geologists no longer regard them thus. They know that the masses out of which our mountain ranges have been carved and chiselled by denuding forces were themselves products of denudation. Instead of being upheaved, our mountain ranges were deposited; instead of indicating steady uprising, they speak of the steady down-sinking of the crust where their crests now appear. In the case of the Alps, the Himalayas, the Rocky Mountains, and other such ranges of comparative youth (so that enough of their structure still remains to speak of their past history), we find that layer after layer of matter, the product, be it remembered, of denudation, was deposited beneath the surface of the sea, along gradually sinking trough-like depressions, until a thickness of more than ten miles was in many places attained. Then, indeed, a process of uprising began. Yet even that resulted from a much more important process of down-sinking or contraction. Immense areas contracting all around the long trough full of deposited matter, this matter was gradually forced up—shouldered up, as it were, like an immense seam in the crust of the Earth. We still find evidence of the tremendous energy of the forces which then acted on the deep layers of deposited matter; for these, despite their vast mass, despite their ten-mile thickness, were twisted an tortured and heaved so irresistibly (though slowly) that the heat developed in the process metamorphosed the material of the rocks until in many regions all trace of the sedimentary character they originally had disappeared. In many cases layers of immense thickness once horizontal were turned up vertically; in some cases such layers were absolutely inverted. Then came the work of yet other myriads of years (I use the words in their literal sense, meaning tens of thousands of years) of denuding processes, quarrying the mountain peaks which have risen rounded and dome-shaped above the sea-level out of the surrounding masses of hard yet more yielding materials. This later denuding work, however, unlike the former, reduced the height of the mountain range. Every thousand years in the earlier stages of denudation had added to the mass of material out of which that range was to be formed.

(1185.) Can we wonder if on the moon, where denuding forces have been so much weaker, and have had so much narrower a range of action, mountain ranges are few and small? The very size of the great lunar craters show why there are no lunar Himalayas. For in the craters we have the materials out of which great lunar mountains could alone have been formed. Denudation, though it has not, as some imagine, left the lunar craters alone, has taken comparatively little from their imposing dimensions. In other words, little material has been borrowed from the craters to make mountain ranges. Consequently the mountain ranges of the moon are few, and they

cannot be compared either in length or height with the chief mountain ranges on the Earth.

(1186.) On the whole, subaerial denudation on the moon must in all probability have been so exceedingly slow in its action, and the time during which it acted so exceedingly short, that the wonder is how any denudation at all can have taken place on the moon's surface. We may probably ascribe such denudation as is indicated by the condition of the crater-covered regions, and by the aspect of the mountain ranges —indeed by the very existence of any mountain ranges at all—to the time when the lunar atmosphere, like the Earth's air in past ages, was laden with carbonic acid (carbon dioxide), sulphuric acid, sulphuretted hydrogen, boracic acid, and so forth. The Earth's air when so constituted was immensely more dense than it is at present, and all the processes of denudation went on far more rapidly than they have done since. The lunar atmosphere at that stage of the moon's history was probably about as dense as our own atmosphere is now; but even if less dense, its constitution and its high temperature, in alliance with the high temperature of the crust, would lead to changes at least as rapid and effective (while they lasted) as those which have taken place on the Earth since the earliest ages of which geological records remain.

(1187.) Thus we should not expect to find the great craters belonging to the early stages of the moon's vulcanian history converted into such wrecks as alone attest on the Earth the former existence of similar terrestrial crater-mountains. But we may yet fairly look for evidence of considerable denudation during the time, short-lasting though it may have been, when the moon's atmosphere and oceans were capable of doing effective denuding work. Accordingly we find that while the immense craters remain still the most striking features of the moon's surface, they attest the action of subaerial denudation during a period which, though it may have been short compared with the corresponding period of our Earth's history, must still be measured by hundreds of thousands of years. But we see in the evidence of such denuding action the last important traces of subaerial denudation in the moon. Not there as on this Earth have lands and seas interchanged after the manner described by Tennyson when he says—

There rolls the deep where grew the tree;
  O Earth, what changes hast thou seen!
  There, where the loud street roars, hath been
The stillness of the central sea.
The hills are shadows, and they flow
  From form to form, and nothing stands,
  Like mists they melt, the solid lands,
Like clouds they shape themselves and go.

Probably once and once only have land and sea interchanged over any wide tract on the surface of the moon.

(1188.) Here a point of considerable interest arises. Since there are mountain ranges on the moon, these must presumably have been formed as the mountain ranges on the Earth were formed long since. Now all our mountain ranges were formed under water, as we have seen. May we not, then, infer, with at least a considerable degree of *à priori* probability, that the mass out of which, for example, the lunar Apennines were formed, was once beneath the waters of a widely extending lunar ocean. On either side of the Apennines extend broad dark tracts such as I have maintained, for different reasons, to be floors of ancient lunar seas. But these

seas, which when the lunar Apennines were being raised were separated by the rising range, must before that process have formed a single ocean. Hundreds of thousands of years must have passed while the mass of matter was being deposited in a deep trough-like depression, out of which the lunar Apennines were afterward to be carved and chiselled by processes of denudation. During all this time layer after layer of matter was being deposited over the floor of the whole ocean, though of course more was collected in the depression than on either side of it. Irregularities before existing on the submerged surface would be obliterated by these deposited layers. The surface eventually disclosed when the waters had retreated would be level compared with the crater-covered regions which during those hundreds of thousands of years had remained above the ocean surface. Yet we might expect to find traces of the buried continents over which these deep layers of ocean mud had been spread. Multiplied experiences show how persistently such irregularities attest their presence, even when lying beneath depths of deposited matter such as we might expect to hide them altogether.

(1189.) When the so-called lunar 'seas' are examined we find features which seem only explicable as such traces of long buried lunar lands we might precisely expect to find. They have been called by some the ghosts of craters. They have the forms and dimensions of such craters as we find in other parts of the moon, but unlike them they are rather markings in a level surface than contour features. They cast no shadows, their interior is never cast into shadow. They differ from actual craters much as fairy rings differ from ringed walls having a similar span. So interpreting these characteristic features we find that the so-called lunar seas tell not only of the past existence of actual seas on the moon, but of buried lunar lands. They tell us also something of the history of our own buried continents, and enable us to read more clearly than we otherwise might, the meaning of those traces of the former presence of water where now there is land which no doubt gave rise to the tradition in nearly every land and among all nations of a former flood.

(1190.) Astronomers have long since given up all hope of tracing either the signs of actual life upon the moon or traces of the past existence of living creatures there. But by sedulous and careful scrutiny processes of material change may be recognised in that seemingly inert mass. In reality, perhaps, the wonder rather is that signs of change should not be often recognised, than that from time to time a new crater should appear or the walls of old craters fall in. The moon's surface is exposed to variations of temperature compared with which those affecting the surface of our Earth are slight. There is, indeed, no summer or winter in the moon.[1] It is the change from day to night which chiefly affects the moon's surface. We have seen that in the lunar year of seasons there are only $11\frac{3}{4}$ lunar days, each lasting $29\frac{3}{4}$ of our days. Thus day lasts more than a fortnight, and is followed by a night of equal length. Nor is this all. There is neither air nor moisture to produce such effects as are produced by our air and the moisture it contains in mitigating the heat of day and the cold of night. Under the sun's rays the moon's surface becomes hotter and hotter as the long

[1] Sir W. Herschel has spoken of the lunar seasons as though they resembled our own, but in reality they are very different. The sun's midday height at any lunar station is only about three degrees greater in summer than in winter; whereas our summer sun is forty-seven degrees higher in the sky at noon than our winter sun. In fact, a midsummer's day on the moon does not differ more from a midwinter's day, as far as the sun's actual path on the sky is concerned, than with us the 17th of March differs from the 25th, or the 19th of September from the 27th.

lunar day proceeds, until at last its heat exceeds that of boiling water; but so soon as the sun has set, the heat thus received is rapidly radiated away into space (no screen of moisture-laden air checking its escape), and long before lunar midnight a cold exists compared with which the bitterest weather ever experienced by Arctic voyagers would be oppressively hot.

(1191.) These are not merely theoretical conclusions, though even as such they could be thoroughly relied upon. The moon's heat has been measured by Lord Rosse (using a fine three-feet reflector). He separated the heat which the moon simply reflects to us from that which her heated surface itself gives out (or, technically, he separated the reflected from the radiated heat) by using a glass screen, which allowed the former heat to pass, while it intercepted the latter. He thus found that about $\frac{6}{7}$ths of the heat we receive from the moon is due to the heating of her own substance. From the entire series of observations it appeared that the change of temperature during the entire lunar day—that is, from near midnight to near midday on the moon—amounts to fully 500° Fahrenheit.[1] In whatever way we distribute this range of temperature, no such life as we are familiar with could possibly exist on the moon; the moon's crust must yet possess a life of its own, so to speak, expanding and contracting unceasingly. It has been suggested that whatever changes might thus be caused in the moon's surface must long since have been completed. But such a surface as the moon at present possesses cannot undergo these continual expansions and contractions without slow disintegration. It seems also extremely probable that from time to time the overthrow of great masses, the breaking up of arched crater-floors, and other sudden changes discernible from the Earth, may be expected to occur.[2]

(1192.) Among cases of change which appear to confirm this view may be cited the alterations which the crater Linné on the Sea of Serenity appears to have undergone in 1866. Schröter asserted, by the way, that a dark spot hid the place of this crater in 1788. But before that time and afterwards until 1866 the crater had been distinctly visible and has been described as about $4\frac{1}{4}$ miles wide and very deep. In October, 1866, Schmidt observed that the crater Linné had disappeared. When the spot was close to the terminator no shadow could be seen, as usual, either within or without the crater. In November he again looked in vain for Linné. Many observers who carefully examined Linné agreed in confirming the results of Schmidt's observation. One of the most satisfactory observations of Linné was effected by Father Secchi at Rome. On the evening of February 10, 1867, he watched Linne as it

[1] Prof. Langley's measurements of the moon's heat have been so obviously affected by some unknown cause of error, since they assign to the full moon a temperature as low as the freezing temperature of water, that we can hardly take them into account as at all invalidating the observations of Lord Rosse.

[2] We must not overlook the enormous volumetric expansion as distinguished from mere lateral extension, resulting from the heating of the moon's crust to considerable depths. On a very moderate computation the surface of the central region of the moon at the time when the moon is full must rise above its mean position to such a degree that many cubic miles of the moon's volume lie above the mean position of the surface there. At new moon—that is, at lunar midnight for the same region—the same enormous quantity of matter is correspondingly depressed. And though the actual range in vertical height at any given point may be small, we cannot doubt that the total effect produced by these constant oscillations is considerable. Years or centuries may pass without any great or sudden change; but from time to time such catastrophes must surely occur. I believe that all cases of supposed change in the moon, if all are regarded as proved, can be thus fully accounted for without any occasion to assume the action of volcanic forces properly so called.

entered into the sun's light, and on the 11th he renewed his observations. In place of the large crater shown in the maps, he could just detect, with the powerful instrumental means at his command, a very small crater, smaller even than those craters which have received no names. 'There is no doubt,' he said, 'that a change has occurred.' Schmidt, it may be mentioned, independently detected the small crater described by Secchi.

The most reasonable interpretation of the change seems to be that the constant processes of alternate contraction and expansion considered in Art. 1191 had shaken down the walls surrounding the old crater, reducing the crater at once in height (and therefore in interior depth) and in span.

Save for such changes as these the moon may be regarded as dead.[1] To what she has taught us of the vast periods of time belonging to her past life, of the nature of the material changes belonging to a planet's life-history, and of the varieties which must exist between the life-histories of planets unequal in mass and volume, she adds the lesson that the planet, like the plant or the animal, has a finite life, and after attaining its prime must pass through stages of old age, decay, and decrepitude, to the final stage of planetary death.

[1] Changes have been suspected in the markings seen on the broad plains or 'seas,' and in the floor of some of the great craters—especially on the floor of Plato. But all such changes in the appearance of level or nearly level tracts can be fully and naturally explained as due to changes in the angle at which such tracts are illuminated, and to changes, however slight, in the direction of the line of sight. Changes in the apparent tints of certain regions under different solar illuminations are undoubtedly subjective only.

CHAPTER XI.

THE PLANET MARS.

(1193.) MARS is the outermost of the family of terrestrial planets, and fourth among the primary planets in order of distance from the sun—regarding the Earth and moon, since they travel together at the same mean distance, as one. In size Mars comes third among the five bodies which form the family of terrestrial planets ; but among the primary planets one only, Mercury, is less than Mars. Mars travels at a mean distance of 141,368,000 miles from the sun. His orbit is more eccentric than that of any primary planet in the solar system except Mercury, though orbits much more eccentric are found among the asteroids. The eccentricity of Mars's orbit is 0·09326, corresponding to a distance of 13,184,000 miles between the sun's centre and the centre of Mars's orbit. Thus his greatest distance from the sun amounts to 154,552,000 miles, while his least is only 128,184,000 miles. The position of the orbit of Mars in the solar system is shown in Plates IX. and X. ; but the relations between the orbits of Mars and the Earth can best be recognised from fig. 119, p. 178 (Art. 400). At his mean distance Mars receives less light and heat than the Earth (per square mile of perpendicularly illuminated surface) in the ratio of about 43 to 100 (or 100 to 232) ; that is, the Earth is more than twice as well warmed and illuminated as Mars. At his greatest and least distances the ratio 43 to 100 becomes about 36 to 100 and 52 to 100 respectively. The disc of the sun as seen from Mars bears to the disc of the sun as seen from the Earth (each at mean distance) the same proportion which the area of the orbit of the Earth bears to the area of the orbit of the Earth in Plate IX., or in fig. 119, p. 178.

(1194.) Mars completes a revolution round the sun in 686·98 days, or rather more than 1 year 10½ months ; so that if Mars and the Earth have started from any conjunction-line, Mars returns to the same part of his orbit when the Earth, having already made one circuit, is still nearly a month and a half's journey (more exactly 43½ days) from the same part of her orbit. Moving nearly twice as fast as Mars (in angular motion round the sun), the Earth at the end of these 43½ days—that is, two years from the former conjunction—is little more than half her former distance (in longitude)

behind the planet, and about $49\frac{1}{2}$ days more bring the planets again into conjunction. That is to say, oppositions of Mars succeed each other at average intervals of about two common years and fifty days, or one common year, one bissextile year, and forty-nine days. But owing to the eccentricity of the orbit of Mars and the consequent variations in the rate of his orbital motion, the actual intervals between successive oppositions of the planet are very unequal, as the positions of the successive loops in Plate V. serve to show, for the dot in each of these nearest to E indicates the position of Mars at each the corresponding opposition as dated in the plate. The following dates[1] of oppositions of Mars during over twenty years illustrate the same point:—

	Interval d.	Interval h.
1871, March 20, 4 A.M.	—	
1873, April 27, 3 P.M.	769	11
1875, June 20, 8 A.M.	783	13
1877, September 5, 12 Mid.	808	16
1879, November 12, 8 P.M.	797	20
1881, December 27, 5 A.M.	775	9
1884, February 1, 11 A.M.	766	6
1886, March 6, Noon	765	1
1888, April 11, 6 A.M.	767	6
1890, May 27, 7 P.M.	776	13
1892, August 3, 10 P.M.	799	3

(1195.) The globe of Mars is 4,247 miles in diameter; its surface 56,665,000 sq. miles (rather more than $\frac{2}{7}$ths of the surface of our Earth, and about 5,000,000 miles larger than its land's surface). The Earth's volume exceeds that of Mars almost exactly six and a half times, while her mass exceeds his nearly nine times; whence it follows that his mean density is rather less than $\frac{3}{4}$ths of the Earth's, more exactly ·728 of the Earth's mean density, and 3·94 times the density of water. Gravity at the surface of Mars is less than gravity at the Earth's surface in the proportion of 39 to 100, so that a mass which would weigh 1 lb. on the Earth would only weigh $6\frac{1}{4}$ oz. (more exactly 6·242 oz.) on Mars. The globe of Mars shows no compression which can be satisfactorily measured, though doubtless the polar axis is in some slight degree shorter than the equatorial. Sir W. Herschel thought he had recognised a polar compression of $\frac{1}{16}$; Kaiser, of Leyden, made the compression $\frac{1}{114}$; Main, of the Radcliffe Observatory, deduced from his observations in 1862 a compression of $\frac{1}{39}$; Mr. Dawes found from one set of observations no compression, from another polar elongation.[2]

[1] The way to determine the mean interval between the successive conjunctions of the Earth and Mars—that is, between successive oppositions of Mars (to the sun)—is as follows. The average daily gain of the Earth on Mars in heliocentric longitude is

$$\frac{360°}{365{\cdot}26} - \frac{360°}{686{\cdot}98}$$

and therefore the Earth will gain one complete circuit from conjunction—that is, will overtake Mars afresh, on the average, in

$$1 \div \left(\frac{1}{365{\cdot}26} - \frac{1}{686{\cdot}98}\right) \text{ days}$$
$$= \frac{686{\cdot}98 \times 365{\cdot}26}{321{\cdot}72} \text{ days} = 779{\cdot}9 \text{ days.}$$

With the rough values of the periods of Mars and the Earth used in this computation, only the first decimal figure in the resulting value of the synodical period can be trusted.

[2] Prof. Adams, in a paper read before the Astronomical Society in November 1879, showed that the ellipticity of the planet might be determined theoretically within not very wide limits from the motion of the nodes of the satellites produced by the action of the equatorial protuberance of the planet. Mr. Marth had shown in the *Astronomische Nachrichten*, No. 2280, that, if there were no such equatorial protuberance and no force depending on the internal structure of Mars to counteract the sun's action, the sun would cause the nodes of the orbits to be in opposition to each other about a thousand years hence, when the mutual inclination of the satellites' orbits would amount to about 49°. At present there is a very near approach to coincidence between the planet's equator and the planes of the orbits of the satellites, such as would exist if the ellipticity of the planet produced a much more rapid motion of the nodes than the sun's action. Prof. Adams guessed that this co-

(1196.) In dealing with the motions of Mars relatively to the Earth, I will adopt a different plan from that which I followed in the case of Mercury and Venus, and again from that which I shall follow in dealing with Jupiter and Saturn, deeming it well to make different planets illustrate different points and principles :—

I consider the movements of the Earth and Mars at the time of a particular opposition of Mars—selecting that of 1884. For convenience of illustration I only deal with the movements of the two bodies *after* Mars was in opposition, noting that the approach to opposition corresponds precisely with the departure from opposition. Mars was in opposition on February 1, or the Earth and Mars were then in conjunction as supposed to be viewed from the sun—*i.e.* in heliocentric conjunction—as at E_1 and M_1 respectively.[1] Successive ten days' intervals brought Mars to M_2, M_3, M_4, &c., the Earth to E_2, E_3, E_4, &c. The lines E_1 M_1, E_2 M_2, &c., joining these successive pairs of positions indicate the gradual increase of distance between Mars and the Earth, and also how Mars, viewed from the Earth, seemed to retrograde until about March 13 (when the direction of Mars from the Earth is indicated by the line E_5 M_5), afterwards advancing again until on or about May 11 Mars was in the same direction from the Earth (but at a much greater distance) as on February 1. After this, of course, he continued to advance, the Earth's motion and his conspiring to increase his apparent rate of advancing motion.[2]

(1197.) In order to determine the telescopic aspect of Mars in the different

incidence could not be merely fortuitous, and he showed that, owing to the small distances of the satellites from the planet's centre, the effect of a small ellipticity of Mars on the motions of the nodes of these satellites would be considerable.

From measures of the planet's diameter and the greatest elongations of the satellites, combined with the time of rotation of Mars and the periodic times of the satellites, Prof. Adams found that the ratio of the centrifugal force to gravity at the equator of Mars is about $\frac{1}{220}$, whence it follows that, if the planet were homogeneous, its ellipticity would be about $\frac{1}{176}$. But if, instead of being homogeneous, its internal density varies according to the same law as that of the Earth, so that the ellipticity would bear the same ratio to the above-mentioned ratio of centrifugal force to gravity at the equator, as in the case of the Earth, then the ellipticity would be about $\frac{1}{228}$. Prof. Adams also showed that such small ellipticities would cause the orbits of the satellites to preserve constant inclinations to planes inclined at very small angles respectively to the plane of the equator of Mars. Such ellipticities would also cause a rapid motion of the apses of the orbits of the satellites, particularly in that of the first. By this means the ellipticity of the planet will probably be determined in the future with considerable accuracy.

There are serious difficulties in determining by actual measurement the small polar compression of Mars. The brilliant polar 'snow-caps' add by irradiation to the observed polar diameter, and there is usually a notable difference of illumination at the two equatorial limbs which can hardly fail to exercise an influence on micrometric measures. Such measures can best be made when the planet reaches opposition and its node together. This was so nearly the case on November 12, 1879, that an observer on Mars would have witnessed a transit of the Earth across the sun's disc. Prof. C. A. Young utilised this opportunity for making an extensive series of measurements of the planet's diameters, which are published in the *American Journal of Science* for March 1880. From a total of 1140 micrometer readings he deduced an ellipticity of $\frac{1}{234}$, and, as the pole of Mars was not on the limb but at a distance of 14° S. from it, he found the actual polar compression of the planet to be $\frac{1}{219}$, and that the chances were more than two to one that the ellipticity lies between $\frac{1}{139}$ and $\frac{1}{515}$. A. C. R.

[1] Where the little arrow-headed line is shown Mars attains his greatest distance north of the ecliptic; at *m* he is in aphelion, at *m′* occurs the midsummer of his northern hemisphere.

[2] The student should turn to fig. 111, where the actual motion of Mars during the time dealt with in fig. 353 is shown. He may also study with advantage the 1886 loop of the relative path of Mars in Plate V.

positions which he thus assumes, it is necessary to consider his axial pose and rotation. The axial position of his globe is shown in Plate IX., which also indicates the position of the point on the orbit of Mars where the planet passes the spring equinox that is spring for his northern hemisphere, marked ♂'s ♎.[1]

The projections of Mars and the Earth in fig. 353 indicate the relative axial position of the two planets. They are on a much larger scale than the orbits—in fact, *exactly five thousand times* larger.[2]

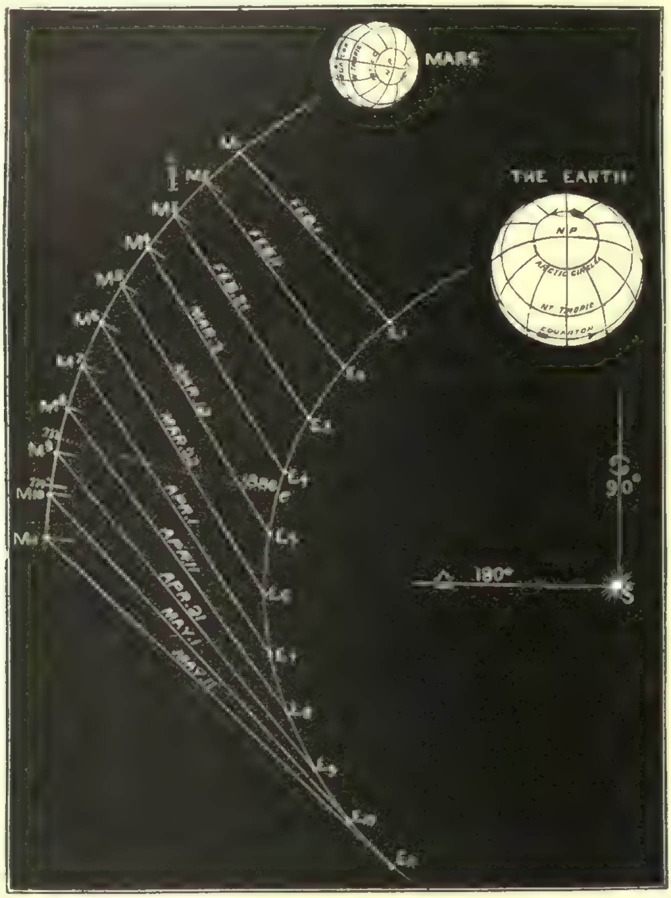


FIG. 353.—Illustrating the apparent motion of Mars at an opposition.

The general reader, but still more the student of Mars with the telescope, may be interested by the following simple method for ascertaining the exact position of the polar axis and equator of Mars (as observed from the Earth) when the right ascension

[1] It is a mistake to mark a planet's spring by the sign ♈, corresponding to the 'first point of Aries' (nominal), the sign which the sun enters as spring begins. The *Earth* enters Libra, not Aries, as spring begins, and a planet's spring is therefore properly named after the sign Libra, which, of course, is used only conventionally, just as in the Earth's case. This mistake [illegible]

[2] The orbits are on a scale of fifty millions of miles to the inch; but the projections of the globes of Mars and the Earth are on a scale of only 10,000 miles to the inch. On the same scale the sun would be nearly five yards in diameter; but it would be inconvenient to show him on that scale in these pages.

and declination are known (these are given at convenient intervals in Whitaker's Almanac).[1]

[1] First make the construction in fig. 354—which is good for every case during the next half

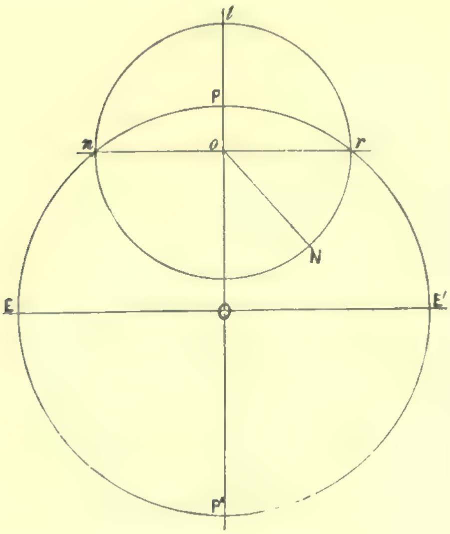


Fig. 354.—Constant part of construction for determining the axial pose of Mars.

century or so. It explains itself:—P r, P n, each = 39·6°; r N = 47·9°.

Suppose the axial aspect of Mars for February 1, 1884, is required.

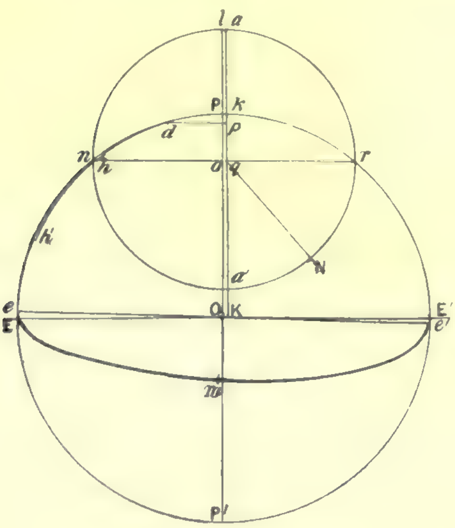


Fig. 355.

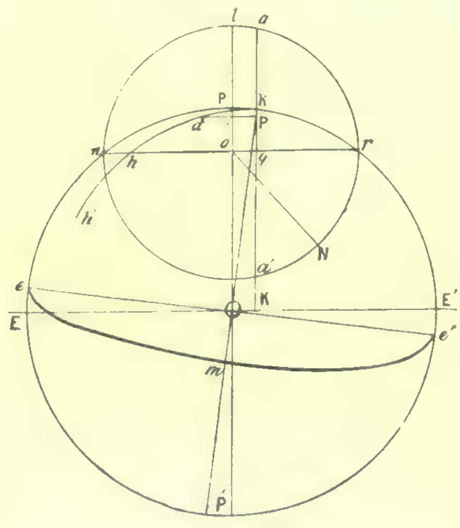


Fig. 356.

Construction for two cases.—Mars on February 1 and March 13, 1884.

Take (with a protractor) arc N a (fig. 355) = Mars's right ascension 136° (9 h. 4 m.), and draw a q K, sq. to n q r, E O E′; describe arc k h about centre K to meet n r in h. Take with a protractor arc h d = Mars's north declination 21° 32′; and draw d p, perpendicular to k K. Then p is the position of the north pole of Mars on his face P E P′ E′ (where P O P′ represents the meridian), and d p = O m, the apparent distance of the middle point of Mars's equator from the centre of his disc.

In fig. 357 the position of Mars as shown in an inverting telescope on February 1, 1884, is presented. Art. 1078 explains fully how to make such a construction after the position of the visible pole and the opening out of the equator have been determined.

I may remark that the above construction, including the constant part shown in fig. 354, applies at all times; but it is to be noted that if the point a falls on n N r, the north pole lies on the invisible half of the planet. For instance, if a came as at a' in figs. 355 and 356, the north pole would lie somewhere on k q, but on the concavity instead of the convexity of the sphere, P E P′ E′. Thus if the construction gave the point p, then p' such that O p' = O p would be the position of the visible pole. Declinations must be measured from h towards k in every case, whether northerly or southerly.

Fig. 356 gives the corresponding construction for March 13, 1884, when Mars reached his stationary point. From fig. 355 it will be inferred that the equator was at this time a little less open. About the end of April the planet's pose was nearly the same as on February 1. A few

(1198.) It is evident from fig. 353 that Mars does not turn his illuminated hemisphere towards the Earth except when he is in opposition as at M_1, the Earth at E_1. At other times he appears more or less gibbous. The illuminated part of his disc differs most from a circle when he is situated as at M_{11}, after opposition, the Earth being at E_{11}, or in the corresponding position before opposition. The angle by which the radius of Mars to the centre of his illuminated hemisphere is turned from the Earth at any time is approximately given by the angles indicated at M_2, M_3, &c., to M_{11}, fig. 353, these angles being formed by the line (fully drawn in the figure) joining Mars and the line (only a short part of which is obvious in the figure) from Mars towards the sun. This method suffices for all practical purposes; in preparing drawings of the presentation of Mars, as in figs. 357 and 359, we have only to note that the 'terminator' is on the side of Mars remote from the sun at the moment. A chart of the motions of the planet from day to day on the star sphere indicates the position of the

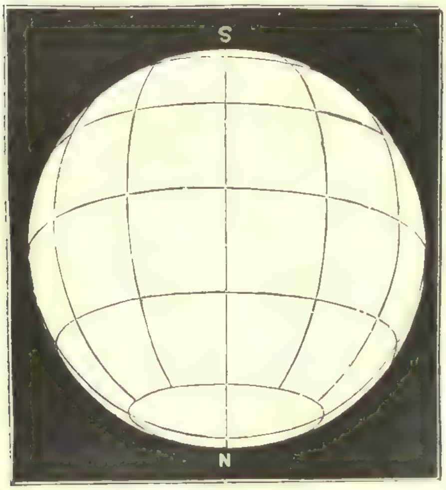


FIG. 357.—Aspect of Mars, February 1, 1884 (inverting telescope).

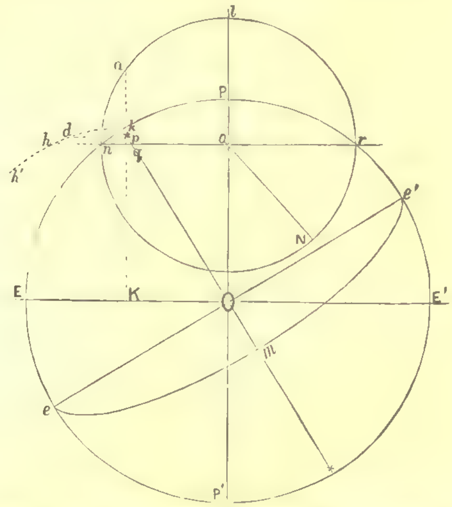


FIG. 358.—Construction for determining Mars's aspect, May 1, 1888.

planet with respect to the sun, or we can simply draw a line through the planet parallel to the ecliptic in the chart, giving approximately the angle which the direction of the meridian through this planet makes with an arc joining the planet and the sun in the celestial sphere.[1]

days later, or more exactly on May 10, the planet's polar axis coincided with the meridian, and after that the visible or north pole passed to the left or east of the meridian.

As a further example take the construction for determining the axial pose of Mars on May 1, 1888, about three weeks after opposition. The construction is precisely the same as that given above, substituting only the proper right ascension and declination—viz. R.A. 194° 9′ and S. Dec. 4° 31¾′ respectively.

The formulæ on which these constructions depend are discussed in an article of mine in Vol. XXXVIII. of the Monthly Notices of the Astronomical Society. I do not present them here, because those who would take interest in them could find no difficulty in deducing them by the formulæ of spherical trigonometry, from the known position of the polar axis of Mars.

[1] If the cusps of the terminator are set at right angles to the latter arc, and the gibbosity is made to correspond with the known position of the planet, the resulting projection of the planet will be sufficiently accurate. For exact accuracy, we must of course take into account the displacement of Mars north or south of the ecliptic—that is, the planet's latitude—giving a slightly different angle for the amount of gibbosity

The following example of the simpler method will suffice not only to show how all such cases can be dealt with, but also to indicate the actual amount of gibbosity attained by Mars at an average quadrature. [He becomes slightly more gibbous at some quadratures (when he is near perihelion) and slightly less at others (when he is near aphelion), but such differences are of no great importance.] On July 22, 1888, Mars attained his quadrature at about his mean distance from the sun, which is also not far from the position Mars attains when passing the autumnal equinox of his northern hemisphere. The face turned earthwards was that shown in fig. 360—the same very nearly as that

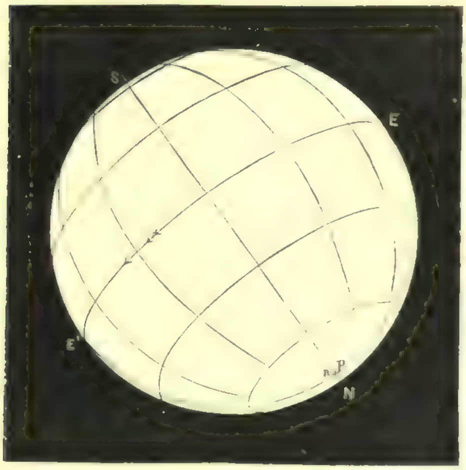


Fig. 359.—The aspect of Mars (in an inverting telescope) May 1, 1888.

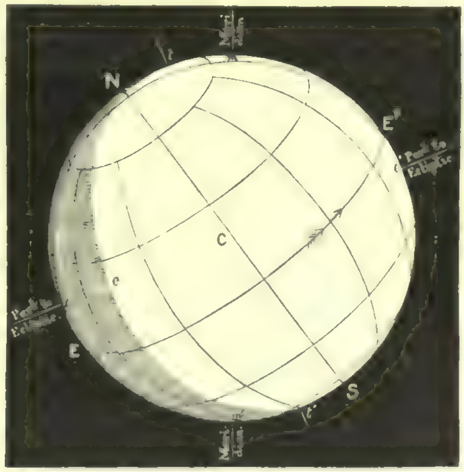


Fig. 360.—Illustrating the gibbosity of Mars (at quadrature July 1888.)

shown in fig. 359, only in fig. 360 Mars is not reversed, as we are not just now considering his telescopic aspect. The meridian through Mars had the position *m* C *m'*; and *e* C *e'* was the direction parallel to the ecliptic (Mars was then not far from the star Spica, where the ecliptic is inclined about 23° to the equator). The angle between the direction-line to Mars and the line connecting Mars and the sun (determined as in fig. 353) was about 44°, so that of the diameter *e* C *e'*, only the part *c* C *e'* remained illuminated—*c* C being equal to *e* C cos 44° roughly, or *c e'* equal to about $\frac{86}{100}$ths of *e e'*. Thus the semi-ellipse *t c t'* marks the position of the terminator.[1]

(1199.) The globe of Mars, axially posed as stated in Art. 1197, and illustrated in figs. 357 and 359, rotates on its axis in 24 hrs. 37 mins. 22·7 secs. Thus it will be understood that this is the sidereal day of Mars, not the solar day; so that it is with our sidereal day of 23 hrs. 56 mins. 4·1 secs. it is to be compared, not with the mean solar day of 24 hrs. Thus the rotation-period of Mars exceeds the rotation-period of the Earth by 41 mins. 18·65 secs.

and also a slightly different position for the line joining the cusps of the terminator. This is a matter in which there is no difficulty whatever; but for all practical purposes to be considered in actual observation, whatever time is taken either in the constructions or in the calculation by the formulæ of spherical trigonometry for obtaining the true gibbosity of Mars in this way is time thrown away; and space given to the discussion of the necessary constructions and formulæ here would also be thrown away.

[1] It will be noticed that *t c t'* nearly coincides with a meridian on Mars; this, of course, corresponds with the proximity of Mars to the equinox of the Martian year, for at the equinoxes of any planet the boundary between the illuminated and dark hemispheres coincides with a meridian.

(1200.) The determination of a planet's period of rotation, to any great degree of exactness, may appear a waste of labour. Sir W. Herschel considered such problems worthy of attention, however, as supplying us with a time-measurer *external to the Earth*, and therefore possibly enabling us to detect slow changes in the Earth's rotation. The planet Mars, undisturbed by the attractions of a satellite (for the tiny bodies called his moons must be quite ineffectual in that way), and marked by easily recognisable features, is clearly the one to be selected for such a purpose; and although modern astronomy would hardly look to Mars for information respecting such changes, yet the bare possibility that at some future date the rotation of Mars may serve this purpose will make the investigation of the subject interesting to astronomers.

It is clear that for the exact determination of a planet's rotation-period the number of rotations dealt with should be great, for whatever error is made in the determination of the planet's rotation-phase in the views at the beginning and end of the interval dealt with will be distributed equally among all the rotations, and affect the deduced rotation-period with a correspondingly small error. Noting this, the student will find no difficulty in following my own discussion of the planet's rotation-period.

Comparing pictures taken by Mr. Dawes in 1852, 1856, 1860, 1862, and 1864, I deduced the period 24 h. 37 m. 23 s., but with such short intervals I could get no nearer to exactness. I next compared a picture taken by Mr. Dawes on October 3, 1862, with drawings by Mädler in 1830; the period 24 h. 37 m. 23 s., or 88,643 seconds, was slightly too great. Kaiser's value, 88,642·6, seemed, on the other hand, slightly too small. I proceeded to yet longer intervals with the provisional period 88,642·8, which was evidently very nearly correct.

I took Herschel's observation of September 30, 1783, for comparison with Dawes' observation of October 3, 1862. First, the planet was nearly at the same part of its path on both occasions (42 revolutions of Mars differ by little more than two days from 79 revolutions of the Earth) and presented nearly the same face; also the dates of observation nearly coincide. The interval separating the two observations (Herschel's made at 10 h. 30 m., Dawes' at 12 h. 15 m.) is 2,493,251,100 seconds. (It will be remembered that 1800 was not a leap-year.) Making due correction for phase, &c., amounting to about 4,000 seconds, I obtained the period 2,493,255,100 seconds, which, divided by 88,642·8, gives a quotient 28,127 *very nearly*. This is the number of revolutions which had elapsed between the two observations. Dividing 2,493,255,100 seconds by 28,127, we get the rotation-period 88,642·76 seconds. I then discussed a much longer interval. In the 'Phil. Transactions,' Vol. I. 1666, there are two pictures taken by Hooke in 1666. They are dated March 3, 1665 (0 h. 20 m. and 0 h. 30 m. morning), which being translated into our present style becomes March 13, 1666 (12 m. 20 h. and 12 m. 30 h.). These pictures, the only fairly recognisable ones in the set of pictures by Hooke and Cassini (shown in the same plate), represent that long sea running north and south with which astronomers are familiar. Figs. 362 and 363 are copied from Hooke's drawings, the equator, however, divided by hour-marks having been added. Comparing these with drawings by Messrs. Dawes, Browning, and others, including some observations made specially for this purpose with a fine 13-inch reflector, lent me by Lord Lindsay, and subsequently with a Browning-With reflector of my own during the oppositions of 1875, 1877, and 1879, I was able to deal with periods ranging from 67,682 to 76,131 rotations of Mars. The error of the first and last observations for such periods being thus divided up is necessarily small. A whole hour divided into 72,000 parts gives for each but the twentieth of a second. The rotation-period finally deduced by me was 24 h. 27 m. 32·7 s.

In the 'Annalen der Sternwarte in Leiden' for 1872, Professor Kaiser endeavoured to show that his estimate of the rotation-period of Mars—viz. 24 h. 37 m. 22·62 s.—was preferable to mine (at that time 24 h. 37 m. 22·73 s.). When I first read this paper it appeared to me that, whether Kaiser were mistaken or not in his conclusions, a great difficulty was introduced; because he stated as the result of his calculations that the drawing of Mars by Huyghens, in 1672, fig. 361, cannot be reconciled with the two drawings by Hooke, in 1666, on which I placed reliance, unless a mistake of two hours in the time be accounted for. Even then—that is, after my first cursory reading of his paper—it seemed to me that the readiest solution of the difficulty was not, as Kaiser suggested, to reject the work of one or other observer, but to divide the error between them. Yet it seemed clear to me that this would involve the supposition of very careless drawing *both* by Huyghens and by Hooke. I found, however, when leisure at last permitted me thoroughly to examine Kaiser's work, that the greater part of this discrepancy arose from two clerical errors in his reckoning. In estimating intervals between the observations in the seventeenth and nineteenth centuries he forgot that the years 1700 and 1800 were no leap-years, and so made the intervals systematically two days too great. As he thus had two Martian rotations too many,

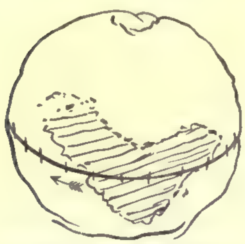


FIG. 361.—Drawing of Mars by Huyghens, August 13, 1672 (N.S.), 10.30 P.M., Paris mean time.

and each exceeds a terrestrial day by 37 m. 22·7 s., the error amounts approximately to the loss of 1 h 14 m. 45·4 s. This reduced the discrepancy by more than one-half. Oddly enough, a second error in calendar matters prevented Kaiser from detecting the error, as he might have been expected to do when he compared the observations of Hooke and Huyghens. For Hooke's observation, made on March 3, Old Style, he translates into March 14, New Style, whereas the real reading is March 13. Thus an error of 37½ minutes is introduced as between Hooke's observations and Huyghens', and this raises the discrepancy between them so nearly to that which *should* exist if Kaiser's rotation periods were correct as to satisfy the requirements of the case.

A certain discrepancy between Kaiser's result as thus corrected still remained to be dealt with. It is very slight, the original difference being only $\frac{11}{100}$ of a second, and when Kaiser's clerical error with regard to the years 1700 and 1800 was corrected, the discrepancy was reduced to $\frac{4}{100}$. And here I may remark that I fully agree with Kaiser's opinion that Linsser and others exaggerated the accuracy with which the rotation-period can be determined. I note that, although I give the seconds to the third decimal place, I only do this to indicate the most probable value. I mention that the value appears to me to lie between 22·73 s. and 22·74 s. Kaiser sets the value at 22·622 s. with about equal confidence in the two last decimal figures. It seems to me to follow from the evidence cited in his latest paper that he and I have ourselves erred slightly in the point we have both dwelt on as involving a mistake (though a larger one) on the part of others, and that at present we must be content to regard the probable error as somewhat larger than the value at which we had agreed in estimating it.

The question turns on the ancient observations employed; for, as Kaiser very justly remarks, when an old observation is compared with modern observations taken within a comparatively short interval of time, the resulting rotation-periods will be almost identical, even though the two modern observations be affected with a considerable error. I must confess, however, I cannot understand why this remark is expressed as a correction on my own statements respecting the two observations in 1864 and 1867, which I compared with Hooke's observations in 1666, seeing that I prefaced those remarks with a statement to precisely the same effect as Kaiser's ostensible correction.

The corrected difference between Kaiser's result and mine arose from our selection of different observations in the seventeenth century to start from. Kaiser depended on the drawing by Huyghens, reproduced in fig. 361, bearing date August 13, 1672 (New Style), at half-past ten, Paris time. I employed the two drawings by Hooke, shown in figs. 362 and 363, taken on March 12, 1666, at 12 h. 20 m. and 12 h. 50 m., Greenwich time (New Style, and astronomical hour).

Fig. 362.

Fig. 363.

Drawings of Mars by Hooke, March 12, 1666 (N.S.), 12 h. 20 m. and 12 h. 50 m., Greenwich mean time.

Kaiser deduced from Huyghens' observations, combined with a great number of modern observations, the value 24 h. 37 m. 22.622 s., but 24 h. 37 m. 22·69 s. must be taken as the true value resulting from the data diligently collected by Kaiser, and from his discussion of those data so far as that discussion was correct. The clerical errors, which he would have been the first to acknowledge, must not be allowed to vitiate results of work intrinsically valuable and exact.

When Kaiser's attention was drawn by my discussion of the problem to Hooke's observations, he strove in vain to reconcile them with Huyghens'. His second clerical error relating to the correction for 'Style' made the discrepancy between Hooke's drawing and Huyghens' appear so great that he pronounced them irreconcilable, and expressed his belief that the north and south sea shown in figs. 362 and 363 cannot be the Hour-glass Sea.[1] On the other hand, he was convinced—and no doubt justly—that the feature depicted by Huyghens can be no other than the Hour-glass Sea. It does not seem to have occurred to Kaiser to construct a picture of Mars, at the time of the observation in 1666, *using his own rotation-period*. If he had done this he would have seen that the large continent on the east (left in map) of the Hour-glass Sea would fall over the space where Hooke shows a well-marked north and south sea. Absolutely no feature on Mars, except the Hour-glass Sea, *could* look like the north and south sea depicted by Hooke; but the region which would fall

[1] 'Hieraus würde folgen,' he says, after stating the facts, 'dass die Abbildungen von Hooke und Huyghens nicht denselben Fleck darstellen können. Es scheint mir nicht schwer zwischen beiden eine Wahl zu treffen. Huyghens's Darstellung ist eben so bestimmt und sicher als die von Hooke unbestimmt und unsicher ist. Hat Huyghens den Flecken *f.* (Hour-glass Sea) wirklich gezeichnet und sich in der Zeitengabe nicht gänzlich geirrt, so muss Hooke's Abbildungen für eine Bestimmung der Rotationszeit unbrauchbar sein.'

where this sea is shown, if Kaiser's period is correct, is the most unlike conceivable. In fact, I think Kaiser would not have remarked on the uncertainty of Hooke's drawing if he had seen views of this planet at the same rotation phase, and when in the part of its orbit where Hooke observed it. Hooke's aspect is quite as characteristic for the summer season of the Martial northern hemisphere as Huyghens' for the winter season of the same hemisphere. As to the accuracy of the two drawings there is not much to choose perhaps. One cannot be enthusiastic over the artistic qualities of Huyghens' drawing, and the fact mentioned by Kaiser, that of twelve drawings by Huyghens this is the only one in which any known feature of Mars can be recognised, does not suggest either excellence of telescopic performance or surpassing skill in delineation. Hooke's drawings are in some respects much better; but it is easy to perceive from the aspect of other drawings by Hooke representing Mars as his distance increased that the telescope employed was barely equal to show even the Hour-glass Sea when Mars was close to opposition.

It seems to me that unless we absolutely reject the work either of Hooke or Huyghens we must use both. If I had been able to use Huyghens' observations, I should certainly not have trusted implicitly to Hooke's; and, on the other hand, I cannot agree with Kaiser in trusting implicitly to Huyghens'.

Fortunately, when Kaiser's calendar mistakes have been taken into account, we have no occasion to attribute very large errors either to Huyghens or Hooke. The discrepancy in time amounts to 1 h. 6 m.[1]

This discrepancy is easily disposed of. Note first, that when the discrepancy is thus reduced, the supposition that Hooke depicted some other feature than the Kaiser Sea must at once be dismissed. Then, an error corresponding to 40 minutes' rotation may be readily assigned to Huyghens' drawing, and one corresponding to rather more than 26 minutes' rotation to Hooke's. This seems a fair apportionment of the discrepancy, and a very brief inspection of the figures will show that the difference in the drawings would be insignificant.

Now if we assign 26½ minutes of the error to Hooke's drawing, setting the Kaiser Sea towards the right by the rotation in that interval, we get for the rotation-period 24 h. 37 m. 22·713 s., instead of my former estimate, 24 h. 37 m. 22·735 s. And this seems the result according best with the evidence, or one may say that the rotation-period is fairly represented by the value 24 h. 37 m. 22·71 s.,[2] which may be perhaps so much as $\frac{1}{50}$th of a second in error. I should regard 24 h. 37 m. 22·70 s., however, as more correctly representing the rotation-period than any value claiming greater accuracy. The second decimal figure of seconds cannot be regarded as determined.

I cannot attach any independent weight to estimates of the rotation-period of Mars which have been published since Kaiser and I worked out the results indicated in the text—results which are in practically perfect agreement when once Kaiser's clerical errors are corrected. Later estimates have either been found by simply extending our results, taking in later observations of Mars (a process legitimate enough, but not capable of giving more correct results until at least half a century has passed), or by the entirely illegitimate method of taking a mean between different estimates, including Kaiser's, *uncorrected for clerical errors !*

[1] [The comparison is as follows :—

		h.	m.
Huyghens . . .	1672, August 13,	12	10·6
Hooke . . .	1666, March 13,	2	56·1
Difference . .	2345 days	9	14·5

In the interval Mars made an exact number of rotations; but, taking the period as 24 h. 37 m. 22·735 s. (which was the value I had deduced), we find

2286 rotations = 2345 days 8 h. 8 m. 12 s.,

a difference of 1 h. 6 m. 18 s.]

[2] It may be noted that if we ascribe the whole error to Hooke, we get the period 24 h. 37 m. 22·681 s., whereas if we ascribe the whole error to Huyghens, we get the period 24 h. 37 m. 22·735 s., and the period may be regarded as certainly lying between these extreme values (only differing by 0·054 s.). I conceive that had Kaiser detected the calendar errors in his computation, he would not have contended for the rejection of Hooke's observations. By applying to the ancient observations his method of taking the probable mean of modern observations, we deduce the rotation-period 24 h. 37 m. 22·71 s., or perhaps, giving due weight to the fact that Hooke made two drawings, we may infer a value lying even nearer to my former estimate. When we note the roughness of Huyghens' drawing, and the unsuitability of the aspect he has pictured for deducing a time-estimate, we may not unfairly consider that a somewhat larger proportion of the error should be awarded to Huyghens. Hence we should deduce some such period as 24 h. 37 m. 22·72 s. But the difference between this result and the other is not worth special discussion. The truth probably lies between the values—

	h.	m.	s.
	24	37	22·70
and	24	37	22·72.

(1201.) It does not appear that Galileo, when he applied to Mars the same telescope which had revealed to him the satellites of Jupiter, was able to detect any features of interest in the nearer planet. More than half a century, indeed, appears to have passed after the invention of the telescope before anything was detected which led to the suspicion that Mars has permanent markings upon his surface. In the beginning of March 1666 Cassini, with a telescope 16 feet in length, but very far inferior in power to many modern telescopes not one quarter as long, noticed features sufficiently remarkable to enable him to determine roughly the rotation-period of the planet. Not many days later Hooke (who had detected spots on Mars in 1665) made the two drawings of Mars, figs. 362 and 363. These drawings were taken by means of a telescope twelve yards long. At the end of the same month observers at Rome, using Campani's glasses, constructed drawings of Mars, which, according to Cassini, were not correct. Certain it is that Cassini deduced from his observations a nearly correct rotation-period, while the Roman observers gave a period only one half the true one, having apparently been deceived by a certain resemblance which exists between two opposite hemispheres of the planet.

(1202.) In 1704–1719 Maraldi made a series of observations of Mars, and two of his drawings are easily recognisable. In one there is seen a triangular or funnel-shaped spot, running nearly north and south, which is doubtless the feature called the 'Hour-glass Sea' by modern astronomers. In the other there is an elbow-shaped spot which powerful modern instruments have broken up into two important 'seas.'

(1203.) Sir W. Herschel, however, was the first who attempted a systematic examination of Martian features. His primary object was the determination of the planet's rotation-period, in order to ascertain whether the length of our day is constant (Art. 1200). His pictures have been described as 'caricatures' of Mars. His large reflectors, though admirably adapted for the observation of objects requiring a great degree of light-gathering power, were wanting in accuracy of definition. Yet the features of the planet as now known can be very readily recognised in Herschel's drawings, a set of eighteen of which I have redrawn on stone, intending to use them in the present work had not more important illustrations crowded these out.

Schröter made many drawings of Mars, some of them well worth studying; but on the whole the 'Areographische Fragmente' of this laborious observer is a conglomerate in which the useless far outweighs the useful portion.

(1204.) The next series of observations which deserves special comment

is that taken by Mädler, in the years 1830–1837. He used an instrument about four inches in aperture, and rather more than five feet in focal length. With this small instrument he constructed an admirable series of views, which he subsequently combined in a 'chart of Mars.'

(1205.) Passing over telescopic studies of greater or less value by Webb, De la Rue, Lassell, Phillips, Terby, Nasmyth, Secchi, and others, we find in the exquisite drawings constructed by Dawes in 1852–1864 a decided advance on our knowledge of the planet's features. He employed first an exquisite $6\frac{1}{2}$-inch refractor from the celebrated Munich works, an instrument which he once described to me as 'absolutely perfect.' Afterwards he employed a fine refractor, $8\frac{1}{4}$ inches in aperture, by Alvan Clarke.

(1206.) From thirty-seven drawings by Mr. Dawes, lent to me for the purpose in 1867, I constructed the chart of Mars shown in fig. 364. The

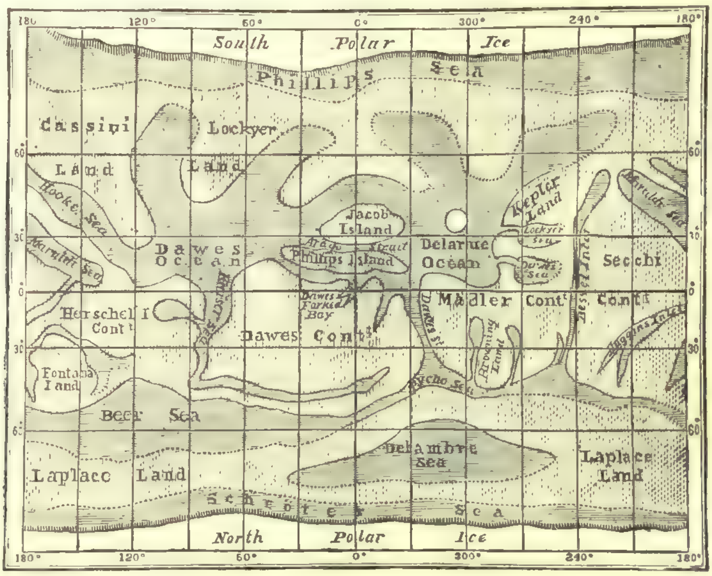


FIG. 364.—Chart of Mars made from thirty-seven drawings by Mr. Dawes.

names were only provisional, being simply appended to various features on the planet for convenience of reference at the time. In the much fuller chart of Mars which is given farther on I have followed a suggestion made by Mr. Green that many of the names should be altered

(1207.) Among later observers may be mentioned Messrs. Green, Knobel, Burton, Schiaparelli, and Perrotin. In examining views of Mars by various observers the peculiarity which first strikes the attention is the great variety of appearances presented in them. One is led to doubt whether all that is pictured is to be taken as representing what the observer actually saw. For while there are large and well-marked features corresponding in many

drawings, there are a multitude of smaller features, streaks, patches, faint shadings, and the like, which one might well suppose to represent merely the general effect presented to the observer by parts of the planet not rendered quite so distinctly visible as the rest. Then, again, on a rough comparison of several views, whether taken on succeeding days or belonging to different years, one does not find the sort of resemblance which one would be led to expect. Yet in dealing with the work of the best observers all the peculiarities presented must be taken into account and specially considered in connection with the Martian season and the Martian time of day at each part of the planet's disc under telescopic scrutiny. In many cases (of course not in all) it is found that details which had seemed unmeaning or perplexing are full of interest and significance when thus dealt with.

(1208.) The planet Mars presents a disc divided into tracts of a ruddy yellow tint and of a colour which to some appears neutral tint, while to others it presents a decidedly greenish hue. The border of the planet appears nearly always whitish, and occasionally broad whitish tracts are seen on other parts of the disc. One might be disposed indeed to call these parts of the planet absolutely white were not this impression corrected by the much more marked whiteness of two spots occupying the planet's polar regions, which shine with lustre so far surpassing that of the planet that I have seen them shining like two rather dull-looking stars when all the rest of the planet has been obliterated from view by a neutral-tinted darkening wedge.[1]

Mars does not at all seasons present identical features. Besides periodic changes in the dimensions of those two white spots near the polar regions (which have so long been recognised as

The snowy poles of moonless Mars)

the details of other portions of his surface are variable. Spots and patches clearly made out on one occasion appear blurred and indistinct on another—though the same telescope may be used, and our own atmosphere (as tested by the performance of the telescope on double stars) may be in a state as well suited for definition. The colour of the planet is also variable; the redness (compared to a faint tinge of Indian red by some observers, and to a coppery tint by others) and the greenish-grey tint of the darker parts of the disc being much more marked on some occasions than on others. The paleness of the disc round the edges is also variable.

(1209.) The variations in the appearance of Mars may be explained on the hypothesis of an atmospheric envelope, such as that surrounding our own Earth, bearing

[1] So called, but in reality, to prevent optical difficulties which would obviously arise from the use of a single wedge-shaped glass, two wedges are combined so that the complete darkening glass has parallel plane-faces.

clouds and mist over the surface of the planet. Judging by the analogy of our own Earth, we may consider that the planet's cloud-covering would vary in density not only from place to place upon the surface, but, considered as a whole, from season to season, and from year to year. It gives a high idea of the difficulty of the problem attacked by astronomers in examining Mars to note that, for favourable research, we must have a fine night upon our Earth and a clear day on Mars (or a clear surface, for, as will be seen presently, it is not at all certain that the whitish spots are due to Martian clouds), combined with favourable circumstances of distance, altitude, and presentation ; that we cannot watch the planet through any single Martian year, but must be content to piece in the observations of different seasons of different years ; and, finally, that Mars, when in opposition at the solstice of his northern hemisphere is nearly twice as far from us as when he is in opposition at the solstice of his southern hemisphere.

(1210.) We may safely assume that the dark spots on Mars are really seas, and the light ochrish-coloured spots continents. Among reasons for this conclusion the following may be cited :—

First, the colour of the spots. It was formerly supposed that the greenish tint of the dark spots might be merely the effect of contrast with the brighter spots which give to Mars its ruddy tint, and earned for it the title of ὁ πυρόεις (the fiery) among the Greeks. But this opinion has been found to be erroneous, and all modern observers agree that the green tint really belongs to the dark spots. In fact, more doubt rests on the reality of the orange tint than on that of the green. Until lately the orange was attributed to the absorptive qualities of the Martian atmosphere.

(1211.) Then we have the evidence drawn from the white spots which cap the Martian poles. If these are really masses of ice, resembling those which surround the poles of our own Earth, the question must of course be answered in the affirmative ; for whence could such enormous masses of snow and ice be formed, save from large seas ? These white spots vary in extent in a way corresponding precisely with the progress of the Martial seasons—and this not for one or two Martian years, but ever since Sir W. Herschel first called attention to the periodicity of the variation. There is something singularly striking in the contrast between the small sharply-defined ellipse of white light round the pole of that hemisphere which is enjoying the Martian summer, and the irregular and wide-spreading tracts of snowy light round the cold pole. In the Martian winter these tracts extend as far from the pole as latitude 45°, a circumstance which indicates an extent of snow-fall corresponding very closely to that which in winter covers the northern tracts of Asia and America. In summer, on the other hand, the icy circle is reduced within a range of about 8° or 10° from the pole. When we see features corresponding so closely with those presented by our own Earth, we are led to the conclusion that these white patches are in reality snowy masses, and therefore that there must exist large seas and oceans whence the vapours are raised from which these snows have been condensed.

(1212.) At a first view we seem to have decided evidence of the existence of a cloud-bearing atmosphere around Mars. The features of the planet are often blurred and indistinct when every circumstance is favourable for observation. The wintry hemisphere is always much less distinct than the hemisphere which is enjoying the Martian summer, as if the wintry skies of Mars were commonly clouded. In the outer parts of the disc the red and green tracts of Mars are always somewhat dimmed or lost under a whitish light, and as those parts of the disc contain the Martian regions which have lately

come into sunshine, or are about to pass out of it, the phenomenon may be interpreted as signifying that the morning and evening skies of Mars are more clouded than the sky of midday.[1]

(1213.) The most decisive evidence in favour of a general resemblance between the physical relations of Mars and those of our own Earth is that which is afforded by spectroscopic analysis :—

During the opposition of Mars in 1867, Mr. Huggins was able to make observations of the spectrum of solar light reflected from Mars. During these observations he saw groups of lines in the blue and indigo parts of the spectrum. But the faintness of this part of the spectrum did not permit him to determine whether these lines are the same as those which occur in the same part of the solar spectrum, or whether any of them are new lines due to absorption undergone by the light at reflection from the planet. He also detected (as in former observations) several strong lines in the red part of the spectrum, these lines being known to be due to the absorptive action of aqueous vapour. He saw Fraunhofer's C very distinctly, and another line about one-fourth of the way from C towards B. As the latter line has no counterpart in the solar spectrum, it was clearly due to an absorptive effect produced by the planet's atmosphere. On February 14 Mr. Huggins was able to detect faint lines on both sides of Fraunhofer's D. These lines occupied positions in the spectrum apparently coincident with groups of lines which make their appearance in the solar spectrum when the sun is low down, so that its light has to traverse the denser strata of the atmosphere. Mr. Huggins was able to show that these lines were produced by the atmosphere of Mars, and not by that of our own Earth. The moon was, at the hour of observation, somewhat lower down than Mars, so that if the lines were due to the absorptive effects of our atmosphere, they should have been more distinctly marked in the spectrum of the lunar light than in that of the light from Mars. But when the

[1] We are not, however, required to make such an assumption as this. For if clouds were pretty uniformly distributed over the whole surface of Mars there would still result a greater brilliancy of the limb. Consider fig. 365, for example. Here a fourth part of the circumference of Mars is supposed to be illuminated by the sun on the left, and clouds are represented which are arranged with perfect uniformity all round this quadrant. When the light falls between the clouds, it is supposed to be returned after a considerable absorption, corresponding to the shaded spaces. When it falls on a cloud, it is supposed to be returned after much less absorption—that is, to remain much more brilliant after reflection—corresponding to the unshaded spaces. And it is at once seen that near the limb all the light would be (in this imaginary case) derived from reflection at the clouds, whereas, near the centre of the disc, the larger proportion is derived from reflection at the real surface of the planet.

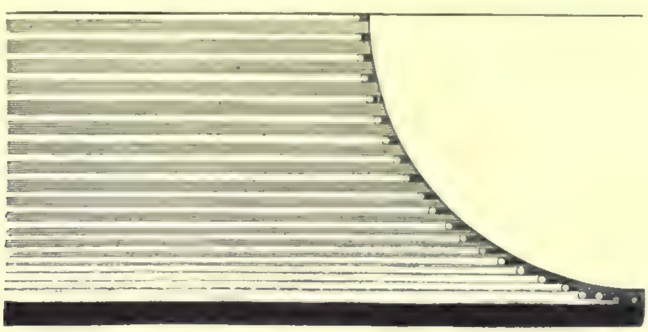


FIG. 365.—Explaining the brightness of the edge of the disc of Mars.

spectroscope was directed to the moon these lines were not visible—an observation regarded by Mr. Huggins as proving that the lines were caused by the absorptive action of the Martian atmosphere.[1]

(1214.) Were it not for the spectroscopic evidence of the presence of aqueous vapour in the atmosphere of Mars, which would seem to indicate an atmosphere of considerable density towards the Martian sea-level, I should be disposed to consider it on the whole more probable that the air of Mars, though of considerable extent (owing to the small attractive power of Martian gravity), is very rare compared with our own. The antecedent probability of this is indeed so great that, despite my recognition of the exceeding care and caution with which Mr. Huggins's spectroscopic work (and indeed all his scientific work) has been effected, I can hardly help doubting whether the evidence by which he considered he had shown that our own atmosphere would not account for the dark lines in the spectrum of Mars was altogether sufficient. May not these lines have failed to show in the spectrum of the moon because of the greater quantity of light dealt with, while in the comparatively faint and delicate spectrum of Mars they showed more plainly? The explanation would be akin to that given in Art. 897 note, of the supposed recognition by Secchi of the same absorption lines of aqueous vapour in the spectra of certain sun-spots.

(1215.) The reasons we have for regarding Mars as probably unlike the Earth in having much less water and much rarer air, and in being probably much more advanced in planetary life (much more aged as it were), may be presented as follows:—

Comparing Mars and the Earth at any similar stage (possibly separated by many millions of years) of their respective careers, it seems reasonable to assume that the total mass of water on the one hand and of free oxygen and nitrogen forming each planet's atmosphere was proportional to the planet's mass. If so, or if there were a rough approach to this relation, the Earth would have nine times as much water and nine times as much air as Mars. And since the surface of the Earth exceeds that of Mars (roughly) only as 7 exceeds 2, the total quantity of water and of air over each square mile of the Earth's surface would exceed the total quantity over each square mile of the surface of Mars in the proportion of about $9 \div 3\frac{1}{2}$ to 1, or about as 18 to 7. So far as the density of the air at the Martian sea-level is concerned, however, we have further to take into account the value of Martian gravity, than which terrestrial gravity is stronger in precisely the same degree,[2] or as 18 to 7. Thus, while there would only be $\frac{7}{18}$ as much water and air per square mile on Mars as on the Earth, the Martian would be rarer than the Earth's (at corresponding stages

[1] The spectroscopic analysis of the darker portions of the disc of Mars seems also to render it probable that these parts are neutral or nearly so in colour. But the observation is a very delicate one. The spectroscopic observations show unmistakably that the ruddy colour of Mars is not due to the effects of the planet's atmosphere. This is also proved by the whiteness of the polar spots, though, being situated upon the edge of the disc, they should exhibit the effects of the atmosphere's absorptive powers more strongly than the central parts of the disc, where the light has passed through a much smaller range of atmosphere. Rejecting as altogether fanciful the suggestion that the red colour of parts of Mars is due to the colour of Martian foliage, we may more safely attribute it to the nature of the planet's soil.

[2] The identity of these propositions is not a mere coincidence, but exists always:—Thus, suppose R r the radii of two planets, P and p, whose masses are respectively M and m, then the quantity of atmosphere per square mile (assuming the mass of the atmosphere of each planet proportioned to the planet's mass) will be proportional to the mass directly and to the surface inversely, so that the ratio for the two planets will be that of $M \div R^2$ to $m \div r^2$, but this is also the ratio of the gravity at the surface of P to gravity at the surface

of the history of each) in the same degree that the square of 7 is less than the square of 18, or that 49 is less than 324, or, roughly, as 5 is less than 33.[1]

If we suppose that the atmosphere of Mars has the degree of tenuity thus indicated at the sea-level, while the quantity of water per square mile on Mars is but about $\frac{7}{18}$ the quantity on the Earth, and the sun's action on each square mile of Mars is less than half the sun's action on each corresponding (that is, similarly presented) square mile of the Earth, we can hardly imagine that there can ever be enough aqueous vapour in the atmosphere of Mars to afford clear spectroscopic evidence of its presence. Even if we double our estimate of the quantity of water and air, we only diminish the difficulty. It must be remembered that our Earth, observed as a whole as we observe Mars, would not usually give very marked spectroscopic evidence of the presence of aqueous vapour in her atmosphere, since the greater part of her light would come either from regions over which the air was not heavily laden with moisture or where, owing to the presence of much moisture, clouds had formed, the light from which would pass to the distant observer only through the upper layers, the moisture-laden parts of the arc below being entirely concealed by clouds. So much less aqueous vapour (relatively as well as absolutely) must, it should seem, be passed through by the light received from Mars, on any reasonable assumption as to the condition of his atmosphere, that on the whole the inference suggested at the close of Art. 1214 seems more probable than the usually accepted interpretation of Mr. Huggins's spectroscopic observations.

(1216.) More speculative, perhaps, yet deserving consideration, is the argument based on the probable age of the planet Mars. Whether his career as an independent orb began before or after that of our own Earth, he must in all probability be far more advanced in planetary life than the Earth is. For with his much smaller mass the processes of cooling which constitute planetary life must in his case have been passed through much more quickly than in the case of our Earth. The ratio 18 to 7, which we have already had to deal with in considering the probable quantity of water and of air on Mars, comes in also in dealing with the relative duration of the various stages of the planet's cooling. Comparing Mars and our Earth when each is at some given part of its life as a planet, our Earth with her greater mass would have nine times as much heat to part with as Mars in passing to some other given part of the planet life of either; but with her surface nearly three and a half times as great the Earth would part with nearly three and a half times as much heat in equal periods of time at any the same stage of cooling. With nine times as much heat to part with, and parting with three and a half times as much heat moment by moment, the Earth's supply of heat would last

of p. It follows that if water and air be proportioned as the masses

Water or air per square mile on P : water or air per square mile on p : : M r^2 : m R^2;

Density of air at sea-level on P : density of air at sea-level on p : : M^2r^4 : m^2R^4;

and though it would be unsafe to assume in comparing any given planet with the Earth that this relation holds, it is always the most probable arrangement antecedently, and probably is in general not far from the truth.

[1] Thus, taking the stage of the planetary life of Mars corresponding with the stage of the Earth's life now in progress, the Martian barometer (mercurial) would stand at an average height of about 4½ inches, and such creatures as man or any of the higher warm-blooded animals of the Earth could not exist on Mars, seeing that experiment has shown (I refer here particularly to the balloon ascent of Messrs. Coxwell and Glaisher, when a height of 6½ miles was attained, at which height Mr. Glaisher fainted, and Mr. Coxwell was within a little of doing likewise) that life is not possible for man even during a few minutes at a height of 7 miles above the Earth's surface, where nevertheless the mercurial barometer still shows a height of 4½ inches.

longer than that of Mars in the same degree that 9 exceeds $3\frac{1}{2}$, or that 18 exceeds 7. Thus, if we imagine only 18,000,000 years to have passed since Mars and the Earth were at the same stage of planetary life, Mars would have reached our Earth's actual condition in 7,000,000 years, or 11,000,000 years ago, corresponding to more than 28,000,000 years of the life of our Earth, so that on this assumption 28,000,000 years would have to pass before our Earth would be cooled to the same degree as Mars, and on any reasonable assumption immense periods of time would have to pass, or (which is the same thing) we must on any reasonable assumption regard Mars as probably much more advanced in planetary life than our Earth. For though the law of relative cooling used in the above reasoning is but a rough approach to the real law, a number of considerations having to be taken into account, respecting which we have no information, or very little, yet almost every circumstance affecting the result tends to increase our estimate of the relatively advanced age of Mars.[1]

(1217.) Although the atmosphere of Mars is probably much rarer than our Earth's, it would extend farther, being compressed by a force so much less than that of terrestrial gravity. On the Earth a height of $2\frac{1}{2}$ miles suffices to halve the atmospheric pressure; on Mars $6\frac{1}{2}$ miles of vertical ascent are required to diminish the atmospheric pressure by one-half. At a height of 13 miles above the sea-level our atmospheric pressure is reduced to less than $\frac{1}{32}$nd of its value at the sea-level; the pressure of the Martian atmosphere at that height is reduced only to $\frac{1}{4}$th its sea-level value; and supposing this to be only $\frac{1}{7}$th of the atmospheric pressure at our sea-level, the Martian air would be measurably denser at a height of 18 miles than our air is at that elevation. At greater heights still the Martian air would be denser still as compared with our Earth's atmosphere at similar heights.

(1218.) It is not altogether easy to determine what is probably taking place in Mars when we seem to see signs as of clouds forming and dissolving, of morning and evening mists, and other phenomena not compatible with the idea of extreme cold. Even the presence of ice and snow implies the action of heat. 'Cold alone,' says Tyndall, 'will not produce glaciers. You may have the bitterest north-east winds here in London throughout the winter without a single flake of snow. Cold must have the fitting object to operate upon, and this object—the aqueous vapour of the air—is the direct product of heat.' The sun exerts enough heat on Mars to raise some vapour of water into the planet's atmosphere, and this vapour must be conveyed in some way to the Martian arctic regions, there to be precipitated in the form of snow. Further, the whole surface of Mars would seem to be above what would be the snow-line for a planet otherwise like our Earth; for any region on our Earth where the cold was so great as it must be on Mars, and where the atmosphere is so tenuous,

[1] Amidst much that is necessarily speculative it is certain that Mars and every other planet must, like our Earth, pass through various stages of development, through loss of heat, through processes of volcanic action, through sub-aerial denudation, and through various physical and chemical changes affecting the condition of the materials forming its substance, and especially its outer regions. It is almost equally certain that every one of these processes proceeds in such a manner on a smaller planet that the stages of its planetary life must be shorter than the corresponding stages of the life of a larger planet, and now that we know how enormous are the periods belonging to the past life of our own Earth, we are compelled to recognise the periods of the lives of all the planets as correspondingly vast, and therefore the absolute differences between the corresponding periods for planets of different dimensions as also enormous, even when relatively they seem small. If, for instance, each stage in the life of another planet were but one-tenth less than the corresponding stage in the life of our Earth, yet one-tenth of many tens of millions would be many millions of years.

would be far above the snow-line even at the equator. How is it then that the snow ever melts, as it manifestly does, since we can see the ruddy surface of the planet?

(1219.) Possibly this difficulty may be removed in a way first suggested, I believe, in Mr. Mattieu Williams's 'The Fuel of the Sun.' The snow actually falling on Mars must be small in quantity, simply because the sun's heat is not competent to raise up any great quantity of water vapour. There would not, then, be anything like the accumulation of snow which gathers in regions above our snow-line; but instead of this there may exist over the surface of Mars, except near the poles, a thin coating of snow, or rather there would ordinarily be but a coating of hoar-frost. Now the sun of Mars, though powerless to raise great quantities of vapour into the planet's tenuous atmosphere, is perfectly competent to melt and vaporise this thin coating of snow or hoar-frost. The direct heat of the sun, shining through so thin an atmosphere, must be considerable wherever the sun is at a sufficient elevation; and the very tenuity of the air renders vaporisation so much the easier, for the boiling-point (and consequently all temperatures of evaporation at given rates) would be correspondingly lowered.[1] Accordingly, during the greater part of the Martial day the hoar-frost and whatever light snow might have fallen on the preceding evening would be completely dissolved, and the ruddy earth or the greenish ice-masses of the so-called oceans revealed to the terrestrial observer. The whitish light round the disc of Mars may probably be better explained thus than as in Art. 1212, note.

(1220.) If we adopt this view of the planet's condition, the principal feature of Martian meteorology is the melting of the coating of hoar-frost (or of light snow, perhaps) from the ruddy soil of the planet and from the frozen surface of his oceans in the forenoon, and the precipitation of fresh snow or hoar-frost when evening is approaching. Throughout the day the air remains tolerably clear, so far as can be judged from the telescopic aspect of the planet, though there is nothing to prevent the occasional accumulation of light cirrus or snow clouds, especially in the forenoon. In fact, the phenomena which have commonly been regarded as due to the precipitation of rain from true nimbus clouds over Martian oceans and continents may more probably be ascribed to the dissipation of cirrus clouds by solar heat. The polar regions would be permanently coated with snow, the limits of the polar snowcap varying, however, with the seasons, on the assumptions here adopted, as on those before considered. But the white polar spots would represent much smaller snow masses—glaciers much less deep—than they would if regarded as formed like the polar snow masses on the Earth.[1]

(1221.) It may be interesting to consider how the considerations we have been here dealing with bear on the results of the latest observations of Mars:—

During the opposition of Mars in 1877 and 1879 Signor Schiaparelli found the narrow streaks (inlets) on Mars, shown in fig. 364, very clearly visible, and observed several others not shown in that map. He found that in 1877 these markings were best seen after the planet had passed opposition, at a time corresponding to the end

[1] Mr. M. Williams suggests the reverse; but with the much reduced quantities alike of air and water which we have assumed in the case of Mars, and the smaller force of gravity making the Martian winds and atmospheric currents much less active, much smaller quantities of water must be carried to the polar regions of Mars than are carried to the polar regions of the Earth. Doubtless the quantity removed by the action of the sun's heat would also be very much less relatively as well as absolutely, but there could be no such accumulation as Mr. Williams suggests, even if the relative amount of air and water were much greater than we have assumed.

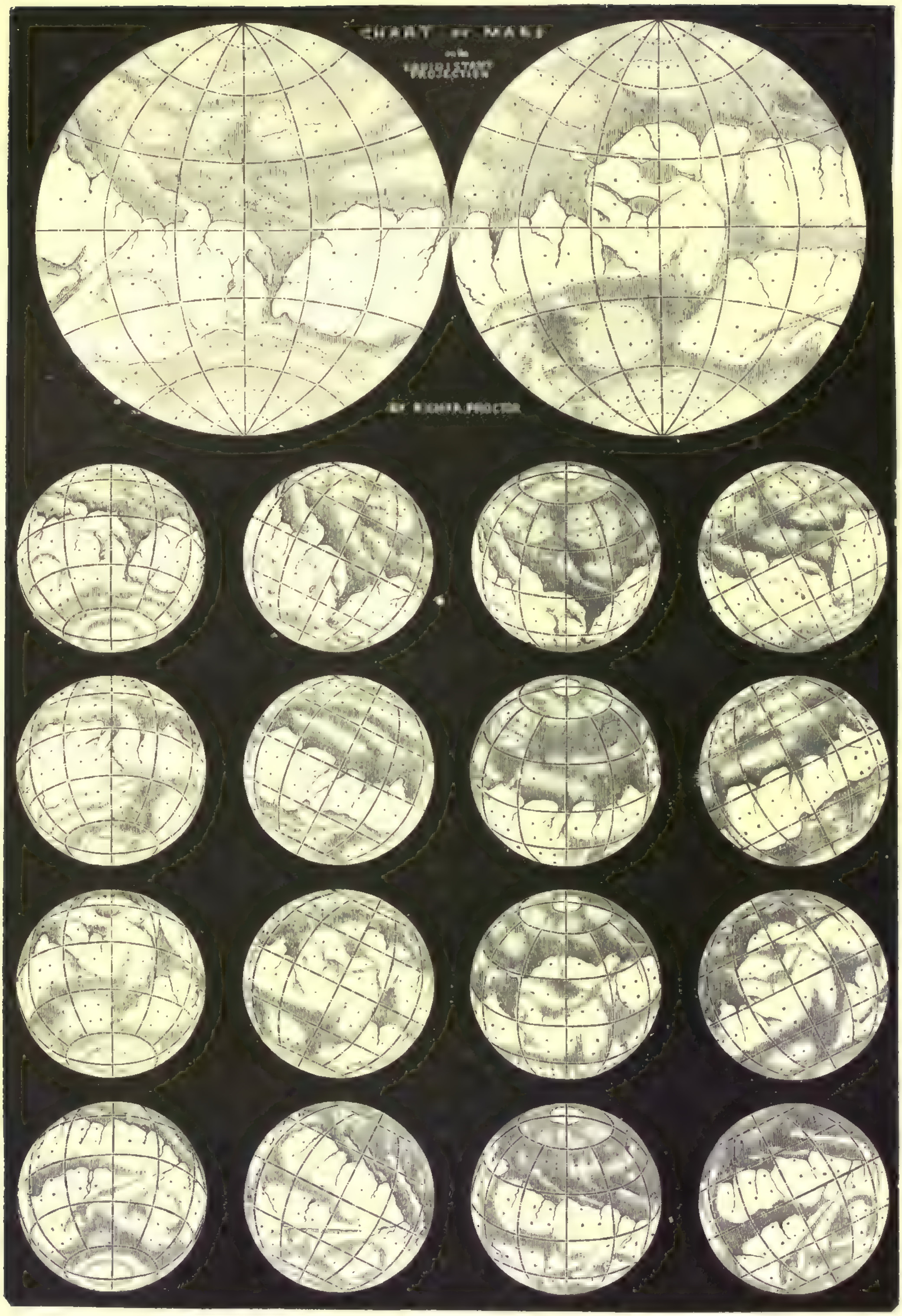
of the winter season of the planet's northern hemisphere, on which most of these streaks are situated. In the opposition of 1879 the markings were seen during the same part of the Martian year, corresponding to nearly the time of opposition.

So far there was nothing specially remarkable in Signor Schiaparelli's observations. It was natural that the markings in question, whether they be regarded as inlets of the Martian seas or as rivers, should be more clearly seen after midwinter was past on the part of the planet to which they belonged, since either cloud or haze or snowfalls might be expected to hide them from our view while the Martian winter was in progress. But in the opposition of 1881–82 (actual opposition occurred on December 27, 1881) Signor Schiaparelli made a series of observations which were justly regarded as surprising. He found that many of the dusky streaks, which he (somewhat rashly) called 'canals,' were doubled, two streaks being seen side by side where one alone had before been visible. No fewer than thirty duplications took place between December 19, 1881, and February 22, 1882; in nineteen cases a well-traced and parallel line was formed at a distance of from 4° to 10° from a previously existing streak, while in the remaining eleven cases the second line seemed doubtful, or did not seem to be quite certainly parallel to the former. A single case of the kind had been noticed in 1879, Nasmyth inlet being doubled shortly before the vernal equinox of the northern hemisphere; but during the whole time of observation in 1877, extending from several weeks before to several weeks after the midwinter of the northern hemisphere, no instance of duplication was observed. At the opposition of 1884 Signor Schiaparelli observed similar duplications in January, February, and March, the Martian spring quarter for the northern hemisphere ranging that year from October 26, 1883, to May 13, 1884, so that the time when many or most of the streaks were seen double corresponded to the middle of the Martian spring quarter (as our month of May, for example) for the region where the double streaks were seen. During the opposition of 1886 (actual opposition March 6) nearly all the duplicate streaks had disappeared. The midsummer of the northern hemisphere of Mars occurred that year on March 30. In 1886 the double streaks were seen, not only by Signor Schiaparelli and his assistants, but by MM. Perrotin and Thollon at Nice. At the time of writing the observations for the opposition of 1888 have not been fully reported, but the double streaks have again been recognised on this occasion before opposition, or again during the spring of the northern hemisphere. Enough has been learned to show that the duplication of the streaks is a phenomenon associated with the Martian seasons. It may be said, indeed, that, so far as observation has extended, the streaks themselves, single or double, are only to be seen, or are at least only conspicuous, after the midwinter of the hemisphere to which they chiefly belong has passed.

(1222.) Fig. 366 is a map of Mars on a modification of Mercator's projection, as drawn by Signor Schiaparelli after he had completed his observations of the duplicated streaks. No one who has ever seen Mars through a powerful telescope can regard this chart as correctly delineating the planet's aspect,[1] even though the visibility of

[1] My own observations of the planet with the telescope have necessarily been few and short lasting, as I have been obliged to make it a point to limit my telescopic studies to just such an amount as should suffice to familiarise me with the characteristics of telescopic work. But with Lord Lindsay's fine 13-inch Browning-With reflector, and later with my own $9\frac{1}{4}$-inch reflector of the same construction, I have observed Mars on favourable occasions with results entirely confirming those of observers who, while admitting that Signor Schiaparelli's duplicated streaks may

PLATE XXIV.



Map and sixteen projections of Mars made by combining drawings of the planet by Dawes, Grēen, Schiaparelli, and others.

the double markings in the clear atmosphere of Milan or Nice be accepted as an observed fact.

(1223.) It seems to me clear that if we accept—and I think we must accept—Signor Schiaparelli's double streaks as actually seen, we cannot regard them as objective realities. Were the single streaks 'canals,' and these 'canals' duplicated, to say nothing of the subsequent disappearance and reappearance of the second canal (in each case of thirty or more), we should have to account for the duplication of canals, some of which must be nearly 2,000 miles long and fifteen or twenty miles broad, the companion canal lying at a distance of about 250 or 300 miles from the first. This is manifestly incredible. But one has only to look at fig. 366 to see the

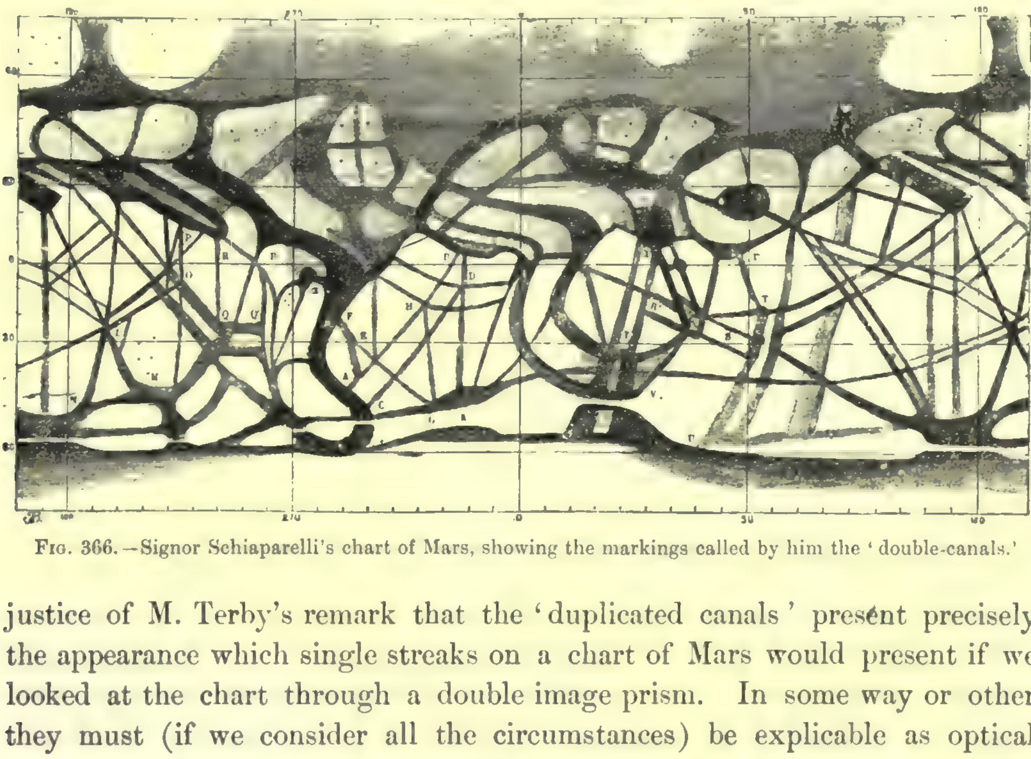


Fig. 366.—Signor Schiaparelli's chart of Mars, showing the markings called by him the 'double-canals.'

justice of M. Terby's remark that the 'duplicated canals' present precisely the appearance which single streaks on a chart of Mars would present if we looked at the chart through a double image prism. In some way or other they must (if we consider all the circumstances) be explicable as optical products. By this I do not mean that they are optical illusions. The diffraction disc of a star and the diffraction rings surrounding the star are not optical illusions, for they are really pictured on the retina of the eye. They are optical products, explicable by the known laws of optics. And I suspect that the double streaks on Mars will be explained by known optical laws, if we only look in the right direction.

(1224.) If on Mars there are rivers akin to terrestrial rivers, but

exist, feel absolutely certain that neither they nor the single streaks have any such hardness or definiteness of shape and outline as he has depicted in his views of the planet and reproduced in his charts.

smaller because water is less in quantity on Mars and the continents are smaller, such rivers would be mere lines on the planet, and as absolutely invisible in their real dimensions as the actual discs of stars. For even a mile's breadth at the distance of Mars would subtend less than the 200th part of a second. We could only see their optical images. What these images would be may be readily determined.

If circumstances were such that a river, seen from near at hand, would appear as a dark marking on a relatively light ground, its optical image as seen in a telescope from a great distance would be a dark streak, having a false apparent breadth much greater than its real breadth. On either side of this dark streak and parallel to it there would be much fainter dark streaks, but unless the real marking were absolutely black on a white ground, these would not be discernible, and we are dealing with rivers supposed to be only somewhat darker than the background on which they are seen, even as the greenish seas on Mars are somewhat darker than the ruddy land. But if circumstances were such that a river on Mars appeared lighter than the background of land, and especially if it were absolutely white (as it would appear if snows lay on its frozen surface or if clouds hung over its length), we know that in the telescope this line of light would appear as a whitish band having an apparent false breadth much greater than the real breadth; on either side of this bright streak and parallel to it there would be fainter bright streaks. And my suggestion is that in the case of an object on the limit of vision, an observer would be less apt to recognise the optical image thus presented as a median bright streak with two fainter bright streaks on either side than as two dark streaks with a relatively bright tract between them. I take it that where Schiaparelli, Perrotin, and Thollon have noticed two parallel dark bands the bright space between these bands has been the diffraction image of a river on Mars, showing white on a relatively dark background because either of clouds along its course, or of snow on its frozen surface left unmelted while the snow all all round had disappeared.

(1225.) Signor Schiaparelli's observations are far more interesting as thus interpreted than as associated with wild fancies about canals and their duplications. They accord well too with what we might antecedently have inferred, whether we regard Mars as a miniature of our Earth and like her in condition, or as a much more aged world, with relatively rare air, exposed to much greater cold, and approaching the stage of planetary decrepitude if not death.

(1226.) On the first supposition it seems reasonable to suppose that (1) during the winter months of the northern hemisphere the rivers on Mars

would not be visible, or would at least not be conspicuous; (2) after the vernal equinox the clouds and mists hiding the continents and rivers in great part from view would melt away, but would linger longest over the river beds; and (3) as summer approached the mists would melt away during the midday hours over the rivers also. The observations of Schiaparelli correspond with this sequence; for (1) in the winter of north Mars no dark streaks are seen, or but few, and those indistinctly; (2) in the spring are seen the parallel dark streaks; and (3) towards summer the duplicated streaks become single. Obviously, too, on the other, and perhaps more probable, supposition considered in Arts. 1223–24 and interpreted as I have interpreted them, Signor Schiaparelli's observations agree well with the thermal changes of the planet's condition and aspect.

(1227.) The requirements of space do not permit me to give in this work any considerable number of drawings, of which I possess several hundreds—either given to me as sketched by Messrs. Dawes, Green, Browning, &c., or contained in essays, pamphlets, and books by Maraldi, W. Herschel, Schröter, Mädler, Secchi, Kaiser, Trouvelot, Dawes, Webb, Green, Browning, Niesten, Knobel, Denning, and a score more. (M. Terby's inquiries and original investigations into the geography of Mars—Areography, as it is somewhat affectedly called—have been of great value to me in the researches by which I have endeavoured to determine the configuration of the Martian continents and seas from the year 1867 until now.) The four figures 367, 368, 369, and 370 serve sufficiently to show how the planet has been depicted by skilful draftsmen. (Mr. Green is not only an able astronomical observer but an artist of great skill.)[1] It

The Planet Mars in 1877.

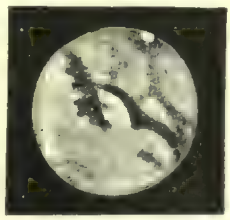


Fig. 367.—Sept. 16, 9h. 40m. G.M.T. (*Trouvelot*).

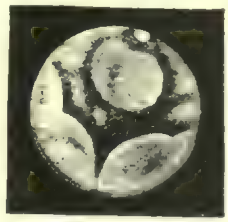


Fig. 368.—Sept. 22, 10h. 0m. G.M.T. (*Trouvelot*).

Fig. 369.—Sept. 10, 11h. 45m. G.M.T. (*Green*).

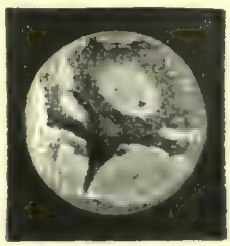


Fig. 370.—Sept. 18, 11h. 20m. G.M.T. (*Green*).

[1] By a mistake of the artist, who by a photographic process reproduced figs. 367–370 from transparencies—a mistake which my distinct injunctions ought to have prevented—the views are not simply inverted as seen in an ordinary telescope, a change which can always be corrected, when required, by holding or setting a telescopic view upside down, but introverted; in fact, they are inverted as respects top and bottom, but not as respects right and left, so that if held upside down they are set right as respects top and bottom but are inverted right and left. This is a

will be noticed how different the markings shown in these views are from the strange-looking, in reality inartistically drawn, markings in Schiaparelli's chart.

(1228.) The map and sixteen projections of Mars in Plate XXIV. and the chart, fig. 371, have been constructed in accordance with the best observations, including Signor Schiaparelli's, of the planet Mars.[1] Such a world as is here depicted and mapped I believe Mars to be—an aged world

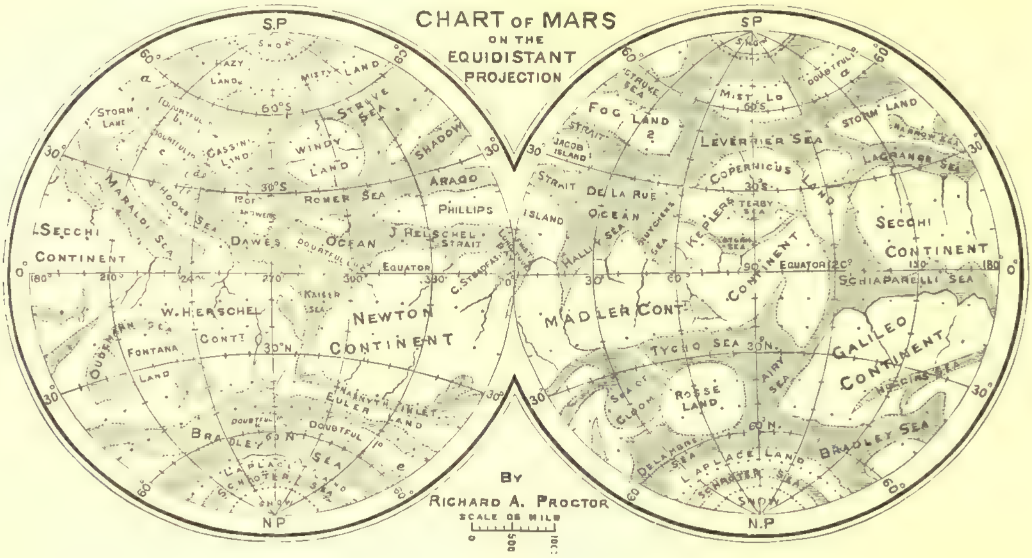


Fig. 371.

indeed, probably long past the fulness of its planetary life, a world unlike our Earth in many respects, unlike her as she is now and not closely like her even as she will be when she has reached a similar age, but a planet whose life history resembles hers in those general respects in which all planets probably are alike—viz. that beginning with a fiery youth they pass through a middle age of moderate temperature, and thence through stages of old age, decrepitude, and finally of death.

point of no importance, and it was therefore not thought necessary to have the pictures re-engraved. In fact, views of Mars with a front view or Herschelian reflector present this same peculiarity. But if the point were not explained the student might be confused in comparing the four figs. 367–370 with the chart, fig. 371, and the map and sixteen projections of Plate XXIV.

[1] Mr. Proctor's reasons for altering the names of the Martian features from those used in his first map of Mars on p. 536 are given in an article of his published in 'Knowledge' for September 1, 1888. It will be noticed that many of the continents named in the first map are entirely wanting in the second; and that with the exception of the Kaiser Sea or Hour-glass marking very few details shown in the Dawes and Green drawings can be identified with certainty with similar markings in Schiaparelli's chart, or with markings in the chart of Mars by Burton and Dreyer, published in Webb's 'Celestial Objects for Common Telescopes.'—A. C. R.

(1229.) Until the year 1877 telescopists searched in vain for a satellite attending on Mars. It had early been suggested by Kepler[1] that since the Earth has one and Jupiter four, Mars probably has two satellites, Saturn six or eight. But there was no scientific value in the suggestion, which was based on analogies now known to have no existence. Swift (at the suggestion probably of Dr. Arbuthnot) ridiculed the idea in his account of the Laputans, in 'Gulliver's Travels,' who, among other imaginary scientific dreams had discovered (that is, invented) two moons attending on Mars and obeying Kepler's laws. Voltaire, with similarly concealed sarcasm, ridiculed Kepler's fancy in his 'Micromegas.' And some later writers, who appear to understand the purpose which all the details of the universe were intended to subserve, maintained that Kepler's suggestion must be sound, since the Creator would not have left a planet like Mars farther than the Earth from the sun without a supply of satellites to make up for the diminished sunlight which his inhabitants enjoy. But the most powerful telescopes employed in the search failed to show the satellites Kepler imagined; and the planet had come to be known as moonless from the time when Tennyson so described it, speaking of 'the snowy poles of moonless Mars' in his 'Palace of Art' (first edition).

Yet all the time Mars has been attended, as Kepler guessed, by two satellites, and not only so, but though minute bodies compared with all other moons in the solar system—so minute, in fact, that they must be relegated to an inferior order of bodies—the moons of Mars have been in reality well within the range of telescopes which have repeatedly been directed to the search for them in vain.[2]

(1230.) In August 1877 Professor Hall, of Washington, began a search for a Martian satellite with the 26-inch refractor (by Alvan Clarke) of the Washington Observatory. Mars was very favourably situated as regards distance during the opposition of 1877, but rather low as observed from northern stations. Professor Hall searched first at some distance from Mars, but on August 10 began to look close to the planet. On August 11 he detected a faint object near Mars which he could only observe for a few minutes, as fog came on. This was really the outer satellite, but not proved to be so till the 16th, when it was found to be travelling with the planet. On the 17th, while looking again for the satellite, Professor Hall found another, whose behaviour then and for several days perplexed him greatly. It would be seen on different sides of the planet within a few hours, so that Professor Hall began to think there were two or three inner moons. But observations made on August 20 and 21 proved beyond a doubt that there was only one inner moon, completing a circuit round the planet in 7⅝ hours—less than a third of the time in which Mars completes a rotation on his axis.

(1231.) Fig. 372 presents the configuration of the orbits of the satellites around Mars at the time of their discovery. The outermost moon is about 14,650 miles from the centre, or 12,500 miles from the surface of Mars; the innermost about 5,830 miles

[1] Writing to his friend Wachenfels in 1610 about Galileo's discovery of the four moons of Jupiter, Kepler says: 'I am so far from disbelieving the existence of the four circum-Jovian planets, that I long for a telescope to anticipate you, if possible, in discovering two round Mars, as the proportion seems to require, six or eight round Saturn, and perhaps one each round Mercury and Venus.' There can be very little doubt that both Swift (through Arbuthnot) and Voltaire were familiar with this suggestion, or that the references to the two moons of Mars in 'Gulliver's Travels,' the 'Micromegas,' and the 'Histoire Philosophique,' were intended humorously.

[2] Herschel seems to have searched specially for a Martian moon in 1783.

from Mars's centre, and about 3,700 miles from his surface. The outermost completes the circuit of its orbit in 30h. 18m., the innermost in 7h. 39m., both moving directly and nearly in the plane of Mars's equator. Some degree of eccentricity has been suspected in the orbit of the inner; but the observation of this body is so difficult that the point must as yet be regarded as doubtful. The dimensions of the satellites cannot be determined by measurement; but the amount of light received from each shows that the diameter of the inner probably lies between six and twelve miles, that of the outer between twelve and twenty-five miles. It is impossible to estimate the mass of either.

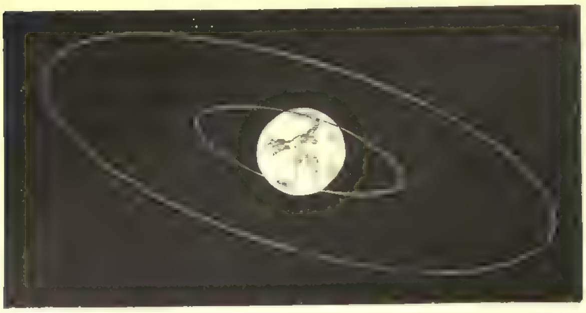


FIG. 372.—Orbits of the Martian moons, Deimos and Phobos, round the planet, as situated in August 1877.

(1232.) The phenomena of the Martian satellites, as observed from the planet's surface, are full of interest, whether regarded from their mathematical or physical aspect. A volume fully as large as the present might be devoted to their discussion. But we must bear in mind that, curious though the subject may be, and considerable the credit which we must give to Professor Hall, and still more to Mr. Alvan Clarke, the moons of Mars are very small bodies, and, individually, not much better worth considering than a pair of rather large meteorites. The following are the principal relations which seem worth considering, and possibly the astronomical reader may regard even these as trifling.

Assigning to the two moons diameters of ten and twenty miles respectively—values so near the maximum admissible values as to imply very low light-reflecting powers (or *albedoes*) to the two bodies—the nearest would have about $\frac{1}{10}$th the apparent size of our moon, and about $\frac{1}{20}$th her apparent light as seen in the zenith, or at her nearest, while the other would have about $\frac{1}{18}$th of our moon's size, and $\frac{1}{36}$th her apparent lustre, as similarly seen. The two together give (on these assumptions) little more than $\frac{1}{13}$th of the light we receive from the full moon. But, as a matter of fact, the two moons can seldom be seen either at their nearest or absolutely full. From large Arctic and Antarctic areas the inner moon is not visible at all, and the outer is invisible from Arctic and Antarctic areas of considerable extent. In the planet's temperate latitudes, north and south, the satellites are never seen at any considerable altitude, and whenever seen they are low down and farther off, and, therefore, look much smaller than when near the Martian zenith. Even at the equator itself their course above the horizon is much less always than half their entire circuit; while, during the greater part of the Martian year the innermost, and in a considerable part of that year the outermost, is lost from view in the planet's shadow at the very time when being in opposition to the sun it would be full but for such total eclipse.

(1233.) It is an interesting result of the rates of circuit of the two satellites that the innermost moves from west to east across the skies, the outermost moving like our own moon from east to west; while also the eastwardly motion of the inner from horizon to horizon is more rapid than the westerly motions of the outer. The former is completed for places at the Martian equator in about $4\frac{1}{4}$ hours of our time, the average interval between successive risings being about $11\frac{1}{2}$ hours. The motion of the

outer from the eastern to the western horizon occupies about 60 hours—more than fourteen times as long, and nearly five times the average interval during which our own moon is above the horizon—the average interval between successive risings being about 132 hours.[1]

But it would be idle to dwell longer on such details. Suffice it to notice further only that with the assumed diameters the inner satellite has less than one four-millionth, and the outer less than one-millionth of our moon's volume. The mass of each is probably less than our moon's in much greater degree. They can certainly raise no perceptible tides in the Martian seas.

[1] The average interval between successive risings or settings is obtained for the two satellites as follows:

The innermost travels eastwards $\frac{1}{7\frac{2}{3}}$ of a circuit per hour, while, owing to the planet's rotation, the heavens are carried westwards $\frac{1}{24\frac{2}{3}}$ of a circuit per hour; wherefore the motion, with respect to the rising or setting place of the satellite,

$$= \left\{\frac{1}{7\frac{2}{3}} - \frac{1}{24\frac{2}{3}}\right\} \text{ of a circuit } \textit{eastwards}$$

$$3\left\{\frac{1}{23} - \frac{1}{74}\right\} = \frac{3(51)}{23 \times 74} \textit{ eastwards}$$

$$= \frac{1}{11{\cdot}124} \textit{ eastwards.}$$

Showing that the average interval between successive risings or settings of the inner satellite is 11·124 hours, or 11h. 7m. 26s., the risings taking place in the west, the settings in the east.

The outermost travels eastwards $\frac{1}{30\frac{1}{3}}$ of a circuit per hour, while, owing to the planet's rotation, the heavens are carried westwards $\frac{1}{24\frac{2}{3}}$ of a circuit per hour; wherefore the motion, with respect to the rising or setting place of the satellite,

$$= \left\{\frac{1}{24\frac{2}{3}} - \frac{1}{30\frac{1}{3}}\right\} \textit{ westwards}$$

$$= 3\left\{\frac{1}{74} - \frac{1}{91}\right\} = \frac{3(17)}{74 \times 91} \textit{ westwards}$$

$$= \frac{1}{132{\cdot}039} \textit{ westwards.}$$

Showing that the average interval between successive risings or settings of the outer satellite is 132·04 hours, or 132h. 2m. 24s., the risings taking place in the east, the settings in the west.

The length of time that either satellite is above the horizon depends on the latitude. It is greatest at the equator, where the arc traversed above the horizon is equal to

$$2 \cos^{-1} \frac{\text{Radius of Mars}}{\text{Distance of Satellite}}. \text{ Therefore,}$$

For the inner satellite this arc

$$= 2\cos^{-1} \frac{2120}{5830} = 2(68\tfrac{2}{3}^\circ) = 137\tfrac{1}{3} \text{ degrees;}$$

and the time the satellite is above the horizon

$$= \frac{137\frac{1}{3}}{360} (11{\cdot}124) \text{ hours} = 4{\cdot}242\text{h.} = 4\text{h. } 14\text{m. } 31\text{s.}$$

For the outer satellite the arc is

$$= 2\cos^{-1} \frac{2120}{14650} = 2(82\tfrac{1}{2}^\circ) = 165 \text{ degrees;}$$

and the time the satellite is above the horizon

$$= \frac{165}{360} (132{\cdot}039) \text{ hours} = 60{\cdot}52\text{h.} = 60\text{h. } 31\text{m. } 12\text{s.}$$

It may be noticed (from the angles $68\frac{2}{3}^\circ$ and $82\frac{1}{2}^\circ$ which come out in this computation) that the centre of the inner moon is not visible on the horizon apart from atmospheric refraction in latitudes higher than $68\frac{2}{3}^\circ$, or, therefore, within $21\frac{1}{3}^\circ$ from either pole; the centre of the outermost not in latitudes higher than $82\frac{1}{2}^\circ$, or, therefore, within $7\frac{1}{2}^\circ$ from either pole.

In the above computation I have not thought it necessary to take into account the circumstance that the periods used for rotation of the planet and the revolutions of the satellites are sidereal, not synodical.

CHAPTER XII.

THE ZONE OF ASTEROIDS.[1]

(1234.) It was noticed by Copernicus, and later still more particularly by Kepler, that there is a sort of gap in the solar system between the orbits of Mars and Jupiter. It is true that the distance separating these orbits (3·679, Earth's as 1) is less than the distance separating the orbits of Jupiter and Saturn (4·336); but there is evidence of a law of increase in the distances separating each orbit from its next outer neighbour, and this law is obviously interrupted if Jupiter is regarded as coming next to Mars.[2]

(1235.) In Kepler's day it was thought by many a sufficient solution of the difficulty to conclude that a planet formerly travelling along this seemingly vacant track had been destroyed on account of the wickedness of its inhabitants. And we are told that there were not wanting preachers who used the destruction of this hypothetical planet as a warning to evil-doers, who were told that if they continued in their sins they might not only bring destruction on themselves but on the world, which might burst, as had that other world, and reduce the sun's family by yet another planet.

[1] This name, *asteroids*, is far better than 'minor planets' for these small bodies. One might as reasonably call meteoric masses minor planets as the asteroids. It would have been convenient, but for this misuse of the term, to call the four outer planets the major, and the four inner the minor planets.

[2] This is shown by the following table of differences :—

Planets	Distance from Sun (Earth's 1000)	First Differences	Second Differences
Mercury	387	336	− 59
Venus	723	277	+ 247
Earth	1000	524	+ 3155
Mars	1524	3679	+ 657
Jupiter	5203	4336	
Saturn	9539		

The irregularity in the case of Mercury, indicated by the change of sign in the column of second differences, has been already considered, and may be removed either by supposing some body or bodies to travel inside the orbit of Mercury or by supposing that the distance of Mercury and all the planets should be measured from the surface of the sun when his globe was much larger than it is now. But there is no way of correcting the irregularity resulting from the great distance between the orbits of Mars and Jupiter by supposing any change in the series of distances at or beyond Saturn. It is evident that the second difference, 3,155, is a great deal too large for symmetry, while symmetry is obviously suggested by the orderly sequence of distances everywhere else in the series —a slight correction being made in the starting-point on the sun's side.

(1236.) It was not until the discovery of Uranus by Sir W. Herschel in 1781 that the speculations of Kepler attracted scientific attention. The three laws of Kepler had been interpreted physically by Newton; and when the empirical law of distances, for which, as it appeared, no reason in nature could be assigned, was found to be fulfilled by the new planet, astronomers could not but regard the circumstance as somewhat more than a mere coincidence.[1]

(1237.) A society was therefore formed—chiefly through the active exertions of De Zach of Gotha—to search for the missing planet. It consisted of twenty-four astronomers under the presidency of Schroeter. The zodiac was divided into twenty-four zones, one of which was assigned to each member of this Society for the Detection of a Missing World. The twenty-four commenced their labours with great zeal. When we consider that over the region of the heavens which they were to examine at least a hundred planets, well within the range of their telescopes, and probably many hundreds of smaller bodies were travelling, we may fairly wonder that they discovered nothing. Such, however, was the result of their labours. After they had been at work a considerable time, accident revealed to an astronomer outside their society a body which was regarded for a long time as the missing planet.

(1238.) Professor Piazzi, while observing stars for his catalogue, was led to examine very carefully a part of the constellation Taurus, where Wollaston had marked in a star which Piazzi could not find. On January 1, 1801, the first day of the present century, he observed in this part of the heavens a small star, which he suspected of variability, seeing that it appeared where before no star of equal brightness had been mapped. On January 3 he found that the star had disappeared from that place, but another, much like it, lay at a short distance to the west of the place which it had occupied. The actual distance between the two positions was nearly a third of the moon's apparent diameter. On January 24 (our observer was not too impatient) he transmitted to Oriani and Bode, members of the Missing World Detection Society, an account of the movements of this star, which had travelled towards the west till January 11 or 12, and had then begun to advance. He continued his labours till

[1] It is strange to consider that had Neptune instead of Uranus been discovered by Sir W. Herschel, the very reverse would have been inferred. Mercury's orbit by Bode's law should be 96, but is really 91; that of Saturn's distance from Uranus should be 192, but is really 188, so that Bode's law is satisfactorily fulfilled by Uranus; but Neptune's distance from Mercury's orbit should be 384 and is really but 296, which cannot in any way be reconciled with the law. Supposing Uranus unknown when Neptune was discovered, the distance of Neptune would have seemed too great by 104 for Saturn's next neighbour (being 296 instead of 192), and too little by 88 for Saturn's next neighbour but one, according to Bode's law of distances. Thus astronomers would have inferred that Bode's law is erroneous (as indeed it is), and would not have thought of looking for a planet between Mars and Jupiter. As, however, by good fortune Uranus was found first, they inferred (mistakenly) that Bode's law represents a real relation existing, no one could say why, among the planetary orbits, and thence concluded (rightly) that the space between Mars and Jupiter is not vacant.

February 11, when he was seized with serious illness. Unfortunately, his letters to Oriani and Bode did not reach those astronomers until nearly the end of March, by which time the planet (for such it was) had become invisible, owing to the approach of the sun to the part of the heavens along which the planet was travelling.

But the planet was not lost. In September the region occupied by the planet was again visible at night. In the meantime Gauss had calculated from Piazzi's observations the real path of the planet. Throughout September, October, November, and December search was made for the missing star. At length, on the last day of the year 1801, De Zach detected the planet, Olbers independently effecting the rediscovery on January 1, 1802. Thus the first night of the present century was distinguished by the discovery of a new planet, and before the first year of the century had passed the planet was fairly secured.

Piazzi assigned to the newly discovered planet the name of the titular goddess of Sicily, where the discovery was made—Ceres.

(1239.) Ceres was found to be travelling in an orbit corresponding in the most satisfactory manner with Bode's law. According to that law the missing planet's distance from the orbit of Mercury should have been 24; calling Mercury's distance from the sun 4, the actual distance of Ceres is $23\frac{1}{3}$.

(1240.) Yet astronomers were not satisfied with the new planet. It travelled at the right mean distance from the sun; but passing over its inferiority to its neighbours, Mars and Jupiter, in size and splendour, its path was inclined to the ecliptic at an angle of more than ten degrees—a thing as yet unheard of among planets. As to its size, Sir W. Herschel, from measurements made with his powerful telescopes, estimated the new planet's diameter at about 160 miles, so that, supposing it of the same density as our Earth, its mass is less than one 125,000th part of hers. Thus it would take more than 1,560 such planets to make a globe as massive as our moon. And even this probably falls far short of the truth, for our Earth owes no small part of her density to the compression produced by the attractive energy of her own substance. Small Ceres would be very much less compressed, and, if made of the same substances, as we may well believe, would probably have a density not very much exceeding the average density of the bodies of which the Earth's crust is composed, that is, about two and a half times the density of water. Thus, it would probably take some quarter-million worlds like Ceres to make such a globe as our Earth, while from our moon three thousand such worlds as Ceres might be made. It was natural that astronomers should regard with some suspicion a planet falling so far short of every known planet, and even of a mere moon, in size and mass.

(1241.) But presently a discovery was made which still more markedly separated Ceres from the rest of the planetary family. Olbers, during his search for Ceres, had had occasion to study very closely the arrangement of the groups of small stars scattered along the track which Ceres might be expected to follow. What reason he had for continuing his examination of these groups after Ceres was found does not appear. Possibly he may have had some hope of what actually occurred. Certain it is that in March 1802, or nearly three months after Ceres had been rediscovered, he was examining a part of the constellation Virgo, close by the spot where he had found Ceres on January 1 in the same year. While thus at work he noticed a small star forming with two others known by him an equilateral triangle. He felt sure this star had not been there three months before, and his first idea was that it was a variable star. At the end of two hours, however, he perceived that it had moved slightly towards the north-

west. On the next evening it had moved still farther towards the north-west. It was, in fact, a planet, and the study of this planet's motion showed that its mean distance from the sun differed very little from that of Ceres. This discovery, rightly understood, was the most surprising of any which had been made since the nature of Saturn's ring was discovered by Huyghens in 1656. We are so accustomed to hear of the discovery of planets travelling along the region of space between the paths of Mars and Jupiter, that we forget how strange the recognition of the second planet of that family must have appeared to astronomers at the beginning of the present century. It showed that the old views respecting the solar system were erroneous, and that in addition to the planets travelling singly around the sun the existence of bodies of a different order —possibly (as astronomers must then have suspected) of a ring of planets—must be admitted. The discovery of this second planet (to which the name Pallas was given) showed that Ceres was not travelling alone in the region which had so long been supposed untenanted. And as it seemed in some degree to explain the smallness of Ceres, suggesting the idea that possibly the combined mass of bodies travelling in this space might not be greatly inferior to the mass of a primary planet, the notion of a ring of worlds travelling between Mars and Jupiter was presently entertained as according fairly with the facts already discovered.

(1242.) Olbers himself was fully satisfied that other planets travel in the region between Mars and Jupiter. He was struck by the remarkable features of the orbit of the planet he had discovered. It was inclined more than three times as much as that of Ceres to the median plane near which lie the tracks of all the single planets. So greatly is the path of Pallas inclined to this tract that even as seen from the sun its range on either side gave to the planetary highway a width of sixty-nine degrees, or nearly four times the width of the zodiac (as determined by the range of Venus, viewed from the Earth, on either side of the medial track). The range of Pallas as seen from the Earth is still greater; so great, indeed, that this planet may actually be seen at times among the Polar constellations. Moreover, the path of Pallas is remarkably eccentric, insomuch that her greatest distance from the sun exceeds her least in the proportion of about 5 to 3.

(1243.) Olbers was led by these peculiarities to the belief that Ceres and Pallas are the fragments of a planet which formerly travelled between the paths of Mars and Jupiter, but had been shattered to pieces by a tremendous explosion. If our Earth, as she travels along her present path, could by some violent internal action be shattered into fragments, the greater number of these would no longer travel in the plane in which lies the Earth's present path. Those which chanced to be driven outwards in that plane would continue to travel in it, though on a changed path; for their original motion and their imparted motion both lying in that plane, so also of necessity would that motion which would result from the combination of these. But fragments which were driven away at an angle to that plane would not travel in it. Hence the great inclination of the path of Ceres and the monstrous inclination of the path of Pallas might be explained by supposing that the former was a fragment which had been driven away at a considerable angle to the ecliptic, while Pallas was a fragment driven away on a path nearly square to that plane.[1]

[1] To show more clearly how Olbers accounted for the peculiar motions of the new planets, suppose our Earth to explode on or about March 20 (see Plate XIX.), at noon, Greenwich time. Then the greater part of South America would be driven forwards; it would therefore travel on a course not far from the original track of the Earth, but more quickly; our Indian Empire would be driven

(1244.) Regarding the two planets hitherto discovered as fragments of one which had burst, Olbers perceived that there was a certain region of the heavens where he would have a better chance of discovering other fragments than anywhere else. Every fragment after the explosion would have a path passing through the place where the explosion occurred. For the place of explosion, being the spot from which each fragment started, would of necessity be a point along each fragment's future track. The fragments would not return simultaneously to that spot. Those which had been driven forwards (more or less) would have their period of circulation lengthened, those which had been driven backwards would have their period shortened; these last then would return to the scene of the outburst sooner than the former, and in point of fact no two would return simultaneously to that place unless, by some utterly improbable chance, they had been hastened or retarded in exactly the same degree. But all would pass through that spot for many centuries (and near that spot for many thousands of years) after the terrible catastrophe which had scattered them on their various paths. If the region of the heavens towards which that spot lay could be determined, then the careful observation of that region would probably soon be rewarded by the discovery of other fragments. Moreover, the region exactly opposite to it would be similarly suitable for the search after these small bodies; for though their paths would not all pass through a *point* exactly opposite the scene of the explosion, these paths would all pass through the prolongation of a line drawn through the sun from that place.

(1245.) Olbers then set himself the task of carefully observing two parts of the heavens, one being the place where the tracks of Ceres and Pallas cross as seen from the sun, the other being the place directly opposite to this.[1] He also persuaded

backwards; and though the advancing motion previously possessed by this part of the Earth, in common with the rest would still carry it forwards, this motion would be greatly reduced. The central parts of Africa and the Atlantic around Ascension Island and St. Helena would be driven sunwards—an impulse which, combined with the previous advancing motion of this region, would cause this part of their new track to cross their former nearly circular track at a sharp angle, passing athwart that track inwards. The part opposite to the last-named—that is, in the middle of the Pacific—would be driven directly from the sun, and this impulse, combined with advance, would cause this part of the new track of the scattered fragments from the Pacific to cross the origina track at a sharp angle, passing outwards. All these regions, and all lying on the zone passing through them, would continue to move in or near the former plane of the Earth's motion; some more quickly than before, some more slowly; some passing outwards at that portion of their course to return eventually inwards till they came to it again, and some passing inwards for awhile, to return, however, after a complete circuit, to the scene of the catastrophe. But England and other European countries would be impelled partly sunwards, partly upwards and northwards, from the plane of their former motion, and would therefore travel on a track largely inclined to their former course—that is, to the Earth's present track. The same would happen, so far as upward motion was concerned, to the United States and to all the northern parts of Asia. The fragments from all these regions would thenceforward travel on inclined paths crossing their original track ascendingly at the place where the explosion occurred. On the other hand, Australia and New Zealand, South Africa, and the southern parts of South America would be driven somewhat downwards or southwards, and the fragments of this zone of the Earth would accordingly travel on paths crossing the original track of the Earth descendingly at the place of the explosion. The North Polar regions, especially the parts north of the American continent, would be driven more directly upwards by the explosion; while the South Polar regions, especially the parts south of the Indian Ocean, would be driven as directly downwards; the fragments from these regions then would travel on paths most largely inclined to the original track of the Earth.

[1] One point is to be noticed as essential to Olbers's faith in the success of his method of search. In his day it was generally believed that many centuries had not passed since the planets had

Harding, of Lilienthal, to pay special attention to these two regions; one near the northern wing of the Virgin, the other in the constellation of the Whale.

(1246.) At length, on September 4, 1804, the search was rewarded with success; the planet called Juno being discovered by Harding in that part of the Whale which Olbers had indicated. Olbers did not cease from the search, however, but continued it for thirteen months after Harding's success, and five years after his own discovery of Pallas. At length, on March 28, 1807, the fifth anniversary of this discovery, Olbers detected Vesta, the only member of the family of asteroids which has ever (we believe) been seen with the naked eye.

(1247.) Astronomers seem to have been satisfied with this fourth fragment of Olbers's hypothetical planet. As ancient theologians, in the fulness of their wisdom, decided that there could not be more than four corners to the earth (whatever they understood by corners), so even in the present century the idea prevailed that an exploding world could not break up into more than four pieces. But, twenty-three years later (or in 1830), Hencke, an amateur astronomer of Driessen, in Germany, commenced a search destined to meet with no success until more than fifteen years had elapsed.

(1248.) On the evening of December 8, 1845, he observed a star of the ninth magnitude in the constellation Taurus, in a place where he felt sure, from his recollection of the region, that there had previously been no star of that degree of brightness. He communicated the observation to Encke, of Berlin; and on December 14 they rediscovered it in the place to which by that time it had removed. It was found to be an asteroid travelling at a distance almost midway between that of Vesta and that of Ceres. Hencke requested Encke to name the new planet, and that astromomer selected for it the name of Astræa.

(1249.) On July 1, 1847, Hencke discovered a sixth asteroid, which Gauss named at his request, calling it Hebe. In the same year, and only six weeks later, our English astronomer Hind discovered the asteroid Iris; and on October 18, 1847, he discovered another, to which Sir J. Herschel, at his request, assigned a name, selecting (somewhat unsuitably, perhaps, for an October discovery) the name Flora.

(1250.) Since the year 1847, inclusive, not a year has passed without adding at least one of these small planets to our knowledge, and in some years new planets have come in by the dozen or more,[1] as the following list sufficiently shows:—

been set moving on their respective paths. According to this view the catastrophe by which Ceres and Pallas and the fragments yet to be discovered had been sent on their new courses could not have occurred so long ago that the paths of the fragments had been materially displaced from their original position. If, on the other hand, millions of years might have elapsed since the catastrophe happened, there would have been little room for hoping that the actual paths of the fragments would have retained any trace of the peculiarity we have described. It was somewhat fortunate for science (an almost unique case of science gaining from the prevalence of false ideas) that Olbers had full faith in the doctrine that the date of the catastrophe could not be more than four or five thousand years before his time, and that therefore he observed the two regions of the heavens indicated by the explosion theory with unwearying assiduity for many months.

[1] In fact it may reasonably be asked whether the researches made of late in this direction are not to be regarded as involving a waste of time which might be better employed. Unless it can be shown that some new facts of interest may be hoped for from such researches, or unless the observers are led on by the unacknowledged hope that while they are looking for intra-Jovian asteroids they may light on an extra-Neptunian planet, it would be difficult to justify the use to which they are applying their time and the powerful telescopes which many of them are employing in this work. They are not now striving to master a problem as yet unsolved, nor are any of them now conducting systematic surveys of the zodiacal zone for star-charting purposes.

Year	Number of planets discovered	Year	Number of planets discovered	Year	Number of planets discovered	Year	Number of planets discovered
1801 } 1807 }	4	1855	4	1866	6	1877	10
		1856	5	1867	4	1878	12
1845	1	1857	8	1868	12	1879	20
1847	3	1858[1]	6	1869	2	1880	8
1848	1	1859	1	1870	3	1881	1
1849	1	1860	5	1871	5	1882	11
1850	3	1861	9	1872	11	1883	4
1851	2	1862	6	1873	6	1884	9
1852	8	1863	2	1874	6	1885	9
1853	4	1864	3	1875	17	1886	11
1854	6	1865	3	1876	12	1887	7
						1888	10
	33		52		84		112

Grand total of asteroids discovered to the end of 1888 281

(1251.) Thirty-five observers have discovered these 281 asteroids in the following order, ranging them according to the number of asteroids discovered by each.

Palisa	68	De Gasparis	9	Knorre	4	Cottenot, d'Arrest, De Ball, Graham, Harding, Laurent, Piazzi, Schiaparelli, Schulhof, Stephan, Searle, & Tietjen, Förster (with Lesser) and Safford (with Peters), 1 each	14
Peters	47	Pogson	8	Tempel	4		
Luther	23	Paul Henry	7	Ferguson	3		
Watson	22	Prosper Henry	7	Charlois	3		
Borelly	15	Chacornac	6	Olbers	2		
Goldschmidt	14	Perrotin	6	Hencke	2		
Hind	10	Coggia	5	Tuttle	2		
	199		48		20		

As not one in a thousand possible readers of these pages is likely to need for any purpose, either of observation or study, the details of the orbits of any of the asteroids, and as not one among those few who would so need them can by any possibility be without ample lists of all the data he could require (much more complete lists than any treatise on astronomy has ever yet given), I shall not follow the usual course of supplying a table of the elements of the 281 asteroids, here or at the close of this work. I content myself with a sketch of the general characteristics of the family. Such a sketch is, indeed, calculated to be much more instructive than the customary table is (though usually omitted).

(1252.) With regard to their mean distances from the sun, the asteroids discovered to the end of 1887, range in order from Medusa (No. 149) and Sila (No. 244), whose mean distances are 2·1327 (Earth's as 1) and 2·1765, to Ismene (No. 190) and Hilda (No. 153), whose mean distances are 3·9471 and 3·9523. Since the mean distances of Mars and Jupiter are 1·5237 and 5·2028, we see that the mean distance of the nearest asteroid yet discovered is but ·6090 beyond that of Mars, while that of the farthest is but 1·2505 within the mean distance of Jupiter, while the range of mean distances between the nearest and the farthest asteroid is no less than 1·8196, or nearly three times as great as the space separating (in mean distance) the orbit of Mars from the inside of the asteroidal zone, and nearly half as great again as the distance separating the outside of the asteroidal zone from the orbit of Jupiter. We may say that half

[1] In the planets for 1858, Melete, really discovered by Goldschmidt in September 9, 1857, is included because that observer mistook Melete for Daphne, a planet for which he was searching when he lighted on Melete, and it was not till late in 1858 that M. Schubert discovered that Melete was a different body, and therefore to be added to the list of asteroids.

the width of the broad space between the orbits of Mars and Jupiter is occupied by the asteroidal zone.

(1253.) These relations, and others more detailed, are illustrated in fig. 373, which shows the orbits of 225 asteroids set as circles at their proper mean distances, indicated by the numbers 2·0, 2·1, &c., along the bounding lines of the sectorial space, alone supposed to be shown. The student should imagine that he is looking at the rings of orbits through an opening bounded by the lines E F, F H, H G, and G E.

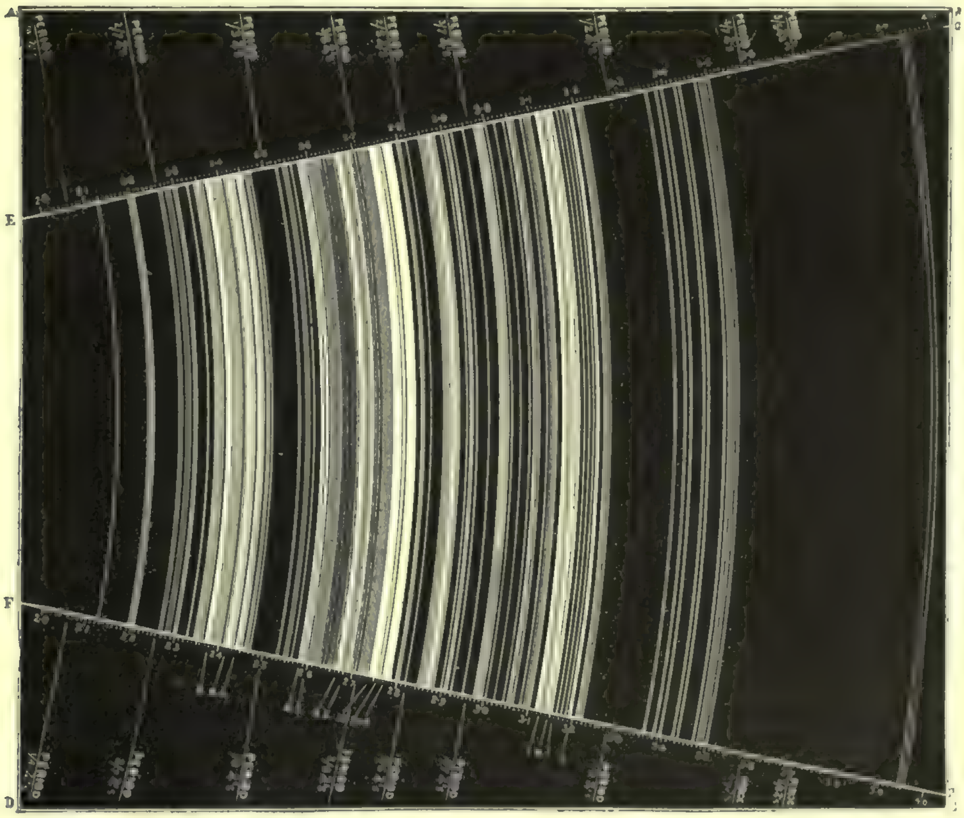


FIG. 373. PATHS OF THE ASTEROIDS.

Distributed according to their Mean Distances, showing the gap at those distances where the Orbital Revolution would synchronise with Jupiter's.

Where, instead of a fine arc, a white band is shown, it will be understood that several rings are supposed to be seen together, the number so seen together being indicated by the numbers (with index lines) 8, 6, 6, &c., below F H. Setting the orbit of Mars to the left of the innermost orbit in fig. 373 by about one-third of the span of the asteroidal zone as there shown, and Jupiter to the right of the outermost by about two-thirds of that span, and imagining the circuit of all the orbits completed, the student will be able to form a clear idea of the extent of the asteroidal zone.

(1254.) But the asteroidal paths are very far from being concentric, as shown in fig. 373, or from lying in one plane. Their orbits range both in eccentricity and in inclination far outside those of the eight primary planets. The orbit of Mercury has an eccentricity of only ·205, but the eccentricity of Æthra (No. 132) is no less

than ·38, little less than that (·41) of the second comet of the year 1867. The average eccentricity of the orbits of 268 asteroids whose elements have been calculated is ·1569.[1] The least eccentric orbit, that of Philomela (No. 196), is nearly circular, the eccentricity being but ·01. The inclination of Mercury to the ecliptic is only 7° 0′, and its inclination to the mean plane of the solar system is considerably less. But the inclination of Pallas (No. 2) to the ecliptic is no less than 34° 44′. Sixteen minor planets have inclinations exceeding 20°. The mean inclination of the whole family is about 8°, and 154 have inclinations between 3° and 11°. The least inclination is that of Massalia (No. 20), which is only 0° 41′. All but two of the sixteen asteroidal orbits having inclinations exceeding 20° are very eccentric, but among the more than fifty in number having eccentricities exceeding ·2 many have small inclination.

(1255.) As a consequence of the great eccentricities of asteroidal orbits—which eccentricities, it is to be remembered, though always averaging about what they do now, may be at different times very differently distributed—the asteroidal orbits range much more beyond the limits indicated by their mean distances. Probably many asteroids of small mean distance have passed far within the aphelion distance, or even the mean distance, of Mars, while others of great mean distance have probably passed far outside the mean distance of Jupiter.

(1256.) When as yet only about eighty asteroids were known, Professor Kirkwood recognised a feature in the asteroidal zone which suggested interesting considerations, though when closely investigated it is not quite so significant as it appeared at first to him and to others (myself included). He found that when the asteroidal orbits are arranged in the order of their mean distance certain gaps are found—no asteroids travelling in orbits corresponding to special distances, and these distances being invariably those at which asteroids would move in periods synchronising with the motions of Jupiter according to some simple law of commensurability. No asteroidal orbit corresponds to a period equal to $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{2}{5}$, $\frac{3}{5}$, $\frac{2}{7}$, and so forth, of Jupiter's period. This is shown in fig. 373, where the distances corresponding to such periods are indicated above the line E G, and we see that except an approach to coincidence at a distance corresponding to a period equal to $\frac{2}{5}$ths of Jupiter's, there are gaps or vacant zones opposite every distance which would give commensurable periods. Even opposite this particular distance there is a very obvious gap, despite the single orbit which falls in the midst of the gap.[2]

[1] I am indebted for many of the details here dealt with to the excellent little work, 'The Asteroids,' by Professor Kirkwood, of Bloomington Indiana.

[2] A period $\frac{2}{5}$ths of Jupiter's gives a relation corresponding to the relation of Jupiter's period to Saturn's—a relation resulting in the perturbation called the Great Inequality of Saturn and Jupiter. The recurrence of similar perturbations, resulting from commensurability of this class, affects three nearly equidistant parts of the orbit of the disturbed body, and is not so effective as a disturbance always affecting the same, or affecting two equidistant parts of the orbit. The difference between the numerator and the denominator of the fraction representing the proportion which the period of the inner bears to that of the outer planet indicates the number of the equidistant spaces into which each orbit is divided by the different conjunction-lines. This is easily shown: Thus, suppose a planet's period is $\frac{p}{q}$ths of another planet's period, p being less than q, and $\frac{p}{q}$ a fraction reduced to its lowest terms, then p revolutions of the outer are equal to q revolutions of the inner; then the angular gain of the inner on the outer, in a unit of time, if we so select our unit of time that the period of the inner planet is a unit of time, is represented by $2\pi\left(1-\frac{p}{q}\right)$, and the time in which a complete circuit or 2π will be gained will therefore be $\frac{pq}{q-p}$. This is the interval between suc-

(1257.) Professor Kirkwood finds in this evidence as to the action in remote times (tens or hundreds of millions of years ago) of the mass of Jupiter in disturbing the movements of those portions of the aggregating mass of the nebula, out of which (following Laplace) he supposes our solar system to have been formed, which chanced to be at such a distance from the sun's centre as caused their movements to synchronise with those of the giant planet. And although I have long since recognised in Laplace's nebular theory a hypothesis absolutely untenable (as originally presented) in the light of recent physical researches, and only waiting exact inquiry, to disappear from exact science, even as Laplace's ideas about the rings of Saturn did when exact inquiries were made, yet I was for a long time disposed to regard the gaps in the asteroidal zone as features of amazing antiquity, wrought out by the planet Jupiter's perturbing action while as yet the region of the solar system inside the orbit of Jupiter was occupied by multitudinous streams of meteoric matter.

(1258.) But as a matter of fact the feature discovered by Professor Kirkwood not only admits of far simpler explanation, but must be regarded as an inevitable consequence of the laws of planetary perturbation acting on a system such as the asteroidal zone, even though the members of that system were originally strewn with perfect uniformity, or if (which is actually the case) they are now strewn with uniformity, when their mean and not their momentary condition is considered.

(1259.) If we consider what must be the behaviour of an asteroid moving in an orbit having its mean period commensurable (according to some simple relation) with Jupiter's period, we shall see that the chances are greatly against the period in which that asteroid seems to be moving at any epoch taken at random appearing to be commensurable with Jupiter's. In a case of simple commensurability Jupiter's perturbing influence, which is greatest, of course, when he and the asteroid are in or near conjunction, is exerted in the same way on the asteroid again and again when at one, or two, or three, or some small number of parts of the asteroid's circuit round the sun. Similar effects being produced at each return to conjunction, because the conjunctions recur at the same part or parts of the asteroid's orbit, it follows that for long periods of time the perturbative effects are cumulative, and the asteroid's motion is largely affected—the more largely as the relation of commensurability is simpler and the number of parts of the asteroid's orbit opposite which conjunctions take place are few. But the more effective the disturbance thus produced, the more does the orbit of the disturbed asteroid depart from the relation of commensurability on which the effectiveness of the disturbance depends. In the course of time the lines on which conjunctions take place so far shift that the disturbances which had been effective gradually become less and less effective till they cease altogether, presently beginning to act in the opposite way, and with more and more efficiency, until the asteroid has resumed the mean distance and period which it had at the beginning. The asteroid is now again exposed to effective perturbation, but under altered conditions. If the original perturbations tended to increase the mean distance and lengthen the period, the perturbations now at work tend to continue that work of diminution, alike of period and distance, which has restored the orbit to its mean position. Thus the mean distance continues to diminish

cessive conjunctions of the two planets; but the interval between successive conjunctions on the same conjunction-line is pq. Hence there are $(q-p)$ conjunction-intervals between two conjunctions on the same line, and obviously these $(q-p)$ conjunctions must be equally distributed round the circuit of the two orbits.

rapidly till this process, like the former, is checked, because it constantly diminishes the condition on which it depends. Thus the diminution of the period goes on more and more slowly as it attains its maximum, and presently ceasing is altered into increase, which proceeds at first slowly, then more rapidly, until it attains its maximum rate of increase when the asteroid has again attained its true mean period.[1]

(1260.) Now it is obvious that if at any time, taken at random, we consider the motion of an asteroid which, though its real mean period is commensurable with Jupiter's, is thus perturbed out of such commensurability—always most effectively when moving most nearly at its mean distance, and always lingering longest in the stages most remote from its mean condition—the chances are very small that we shall find the asteroid moving at that which, nevertheless, is its true mean distance. For hundreds of years an asteroid whose real mean distance gives it a period commensurable with Jupiter's, moves at a mean distance (for each individual circuit) appreciably less than that, for hundreds of years it moves at a mean distance appreciably greater; only for tens of years does its mean distance for each circuit correspond with its mean distance for all its circuits. If then we find it moving in circuits not giving it a period commensurable with Jupiter's, we must not conclude that its real mean period is not so commensurable, until we have calculated what its motions have been during hundreds of years in the past, under Jupiter's attractions, and what they will be during hundreds of years in the future. If this be done for all the asteroids which Professor Kirkwood finds in the neighbourhood of the zones corresponding to periods commensurable with Jupiter's, it will be found that the real mean distances of not a few of them correspond to these gaps, and that the asteroidal orbits thus dealt with are distributed as uniformly as the laws of probability would lead us to expect.

(1261.) Olbers's theory that the four small planets, Ceres, Pallas, Juno, and Vesta, were the fragments of a single planet which had burst was not hopelessly untenable, so far as those four planets were concerned, for their mean distances are nearly equal, being 2·769, 2·771, 2·668, and 2·361 respectively, the Earth's being taken as unity. Indeed, when Olbers first suggested the theory (Art. 1243) only two of the small planets had been discovered, Ceres and Pallas, and these travel at almost the same mean distance from the sun. In the 'Connaissance des Temps' for 1814 Laplace showed that with a velocity of explosion not exceeding twenty times that of a cannon-ball—say twelve times that of a ball fired from one of our best modern cannon—the observed range in the mean distances of these bodies might have been obtained. It is, however, to be noticed that even for this the Pallas fragment was assumed to have been expelled directly forwards and the Vesta fragment directly backwards (with reference to the course of the planet at the moment of the cata-

[1] The reader who chances to possess the first edition of my treatise on 'Saturn and its System' will find the processes here considered illustrated by the Mean Inequality of Saturn and Jupiter, the table of which (Table X.) with first and second differences, as explained at the end of the book, indicates the progress of precisely such changes as are considered above, brought about in precisely the same way, only of course the change of period for an asteroid disturbed by Jupiter is much greater than the change for Saturn disturbed by Jupiter or for Jupiter disturbed by Saturn.

strophe), in order that the whole effect of the change of velocity might fall on the period, lengthening to the utmost that of Pallas and shortening to the utmost that of Vesta. Fig. 373 shows that the range in the known mean distances of the asteroids has enormously increased since then, and that even in this aspect alone the explosion theory is no longer so hopeful as when Laplace held it to be barely tenable.

(1262.) But another objection, first dealt with by Encke, has always been held more serious, and has now become to all intents overwhelming. Olbers and his contemporaries, as we have seen, held that whenever such an explosion occurred the new orbits of the scattered fragments must pass through the point where the explosion took place. They found no such point common to the orbits of the four first-discovered asteroids.[1]

But Encke showed that, judging from the present variations of the orbits of the asteroids, there was even less approach towards a region of common intersection in past ages than there is at present. And more careful investigations of certain of the asteroidal orbits has shown that, apart from the attraction of the small planets on each other, the orbits examined never *could* have intersected. Since the attractions of these planets on each other are exceedingly small, it is practically impossible that the divergence of the system from a state of things which *must* have existed if ever a planet burst in the mid-region between Jupiter and Saturn can be explained in this way.

(1263.) There is further an equally strong physical objection against Olbers's theory in the circumstance that, though in a great volcanic outburst the Earth's internal forces suffice to eject matter with velocities comparable with those required by the hypothesis, we have in these cases the energies of enormous subterranean regions exerted on relatively minute bodies. To conceive that the energies even of the whole Earth, if all brought into action at once, could suffice to propel the whole mass of the Earth—in fragments—from around the centre of explosion, with velocities enormously exceeding those ever observed in any fragment ever shot out from a volcanic crater, is a very different matter indeed. A certain charge of gunpowder will drive a cannon-ball to a distance of two or three miles, but a thousand times that charge would not scatter the fragments of the cannon (if the ball had been tightly fixed in) over a similar distance all around the place of explosion. Nothing known about our Earth's interior, nothing which we can infer about the interior of any other planet formed by processes such as we recognise in the development of the solar system as at present understood, suggests the

[1] Professor Newcomb seems surprised that they expected to find an actually existent region of intersection, which would have implied that the explosion had occurred within a few thousand years. It would have been strange if in Olbers's time men had expected anything else.

possibility that a millionth part of the force necessary to shatter a planet, as Olbers's theory requires, can ever be generated and accumulated within the planet's interior.

(1264.) Laplace in his celebrated nebular hypothesis advanced a different explanation of the origin of the zone of asteroids—an explanation sufficient if only the nebular theory be accepted to explain the existence of a zone of small planets instead of a single planet. This theory, associated with (and subordinated to) the theory of meteoric aggregation which recent discoveries have established, gives also an account of the position of the zone of asteroids in the solar system. These points I shall consider later, endeavouring to show that the zone of asteroids belongs to the region separating the sun's special family of small planets from the region where orbs which not so very long ago were minor suns travelled with their attendant families of worlds. Here it is only necessary to note that the zone of asteroids appears to represent a portion of the material of the solar system which under more favourable conditions would have developed into a single planet, but, disturbed by the attraction of the great planet Jupiter, remained scattered in the form of a widely strewn ring.

CHAPTER XIII.

THE PLANET JUPITER.

(1265.) Beyond the zone of asteroids we enter upon a region of the solar system differing alike in character from the domain of the terrestrial planets and the ring region of the planetoids. The distances between the several planetary orbits show a large increase, while the planets themselves in this outer region of the solar system are so much larger than the Earth and her fellow planets that we are led even at the outset to regard them as belonging to an entirely different class. This opinion is confirmed as we proceed, and may be regarded as now to all intents and purposes established by the evidence.

(1266.) Jupiter is the first in order of distance from the sun among these larger planets, and first also in point of size and mass. He travels round the sun in a period of 11 years 314·9 days. His distance from the sun is 482,716,000 miles; but, as the eccentricity of his orbit is ·048, his distance from the sun when he is in aphelion exceeds his mean distance as 1,048 exceeds 1,000, while his distance when he is in perihelion is less than his mean distance as 952 is less than 1,000. Thus his greatest mean and least distances are as 131, 125, and 119. Calling the Earth's mean distance 1, the greatest, mean, and least distances of Jupiter are respectively 5·45, 5·20, and 4·95. Plate X. shows the position of Jupiter's perihelion at J, and of his aphelion at J′; and it will be manifest from a comparison of that plate with Plate IX., in which dates are shown round the Earth's orbit, or from a study of the Earth's orbit itself (similarly situated) in Plate XVII., that we get the nearest view of Jupiter if the Earth is on the side of her orbit towards J, Plate X., when Jupiter is on the same side of the sun, or technically when Jupiter is in opposition in September and October; whereas if the Earth is towards J′ when the planet is in opposition, Jupiter is at his greatest opposition distance from the Earth. The distances of Jupiter from the Earth under these conditions range from about 3·95 at an opposition near J to about 4·45 at an opposition near J′, calling the Earth's mean distance from the sun 1. Plate X. also shows

Jupiter's range above and below—that is, north and south—of the plane of the Earth's orbit, which is the plane of the picture. The points marked ☊ and ☋ show where Jupiter crosses the plane of the Earth's orbit, while the very small arrowed I's midway between these show, correctly to scale, the distance reached by Jupiter above and below that plane.

(1267.) I deem it well, in order more clearly to indicate Jupiter's position in the solar system, already shown in Plate X., to present the path of our Earth and a part of Jupiter's in a single figure on a larger scale for comparison. This figure will also serve to illustrate usefully the various changes of distance and presentation which the planet Jupiter undergoes as he and the

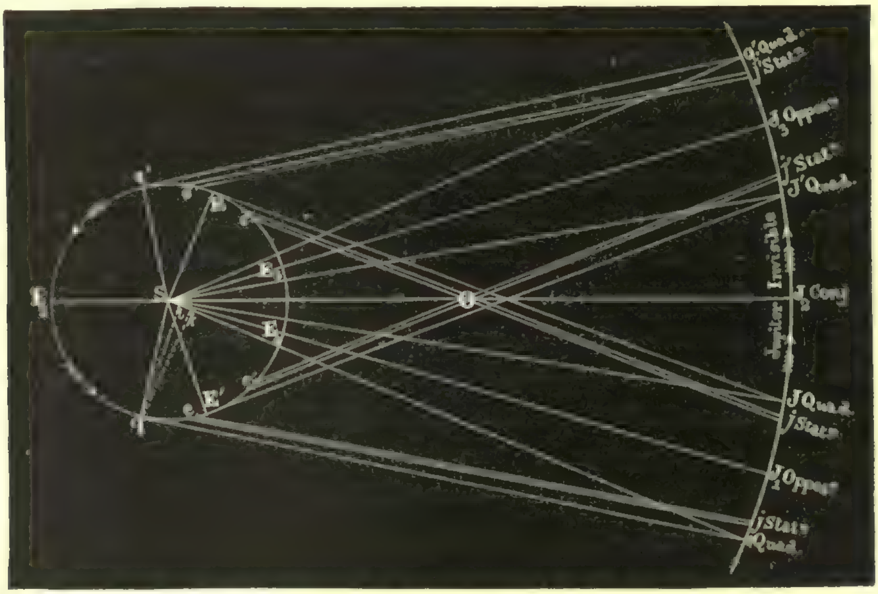


Fig. 374.—The Earth's orbit ($E_1E_2E_3$) and a part of Jupiter's orbit ($J_1J_2J_3$).

Earth severally complete their circuit around the sun. Fig. 374 shows, then, the Earth's orbit, $E_1E_2E_3$, about the sun, S, and a portion, $J_1J_2J_3$, of Jupiter's orbit traversed by the planet while the Earth is passing from E_1 to E_2, and past E_1 again to E_3—S E_1J_1, E_2S J_2, and S E_3J_3 being straight lines passing severally through simultaneous positions of the Earth and Jupiter.

(1268.) In order to complete in this one case (which will enable the student to understand all such cases) the illustration and explanation of the apparent motions of a planet advancing, retrograde, and standing still, I have added to the opposition and conjunction lines in fig. 374 lines indicating the varying directions of Jupiter as seen from the Earth at his quadratures and stations preceding and following oppositions. No explanation is needed, but the student should carefully consider the relations presented in fig. 374, noting

specially the following relative positions: The line q Q joins the Earth at q and Jupiter at Q when Jupiter is in quadrature preceding opposition and still advancing, though slowly, as the small angle between q Q and the parallel lines e j shows. These connect adjacent positions of the Earth at e with corresponding positions of Jupiter at j, and their parallelism shows that Jupiter is for the moment stationary. The Earth passes on to E_1 and Jupiter to J_1, the line S E_1J_1 showing, by passing through the sun, Earth, and Jupiter, that he is in opposition (to the sun), and by its position that he has retrograded. He continues to retrograde till he arrives at j, when the Earth is at *e*, and the parallelism of the lines e j shows that Jupiter is for the moment stationary. The line E J indicates the position of Jupiter at quadrature following opposition, and now slowly advancing. He continues to advance at a constantly increasing rate, his distance from the Earth increasing until he passes out of view into conjunction (with the sun at J_2), as shown by the line E_2S J_2. Still advancing, Jupiter passes again into view, coming to quadrature preceding opposition at J′, to a stationary position preceding opposition at *j*′, to opposition at J_3, to station following opposition at j′, and to quadrature following opposition at Q′.[1]

(1269.) It is obvious that we see Jupiter best when he is in opposition, as at J_1, J_3 (fig. 374), the Earth at E_1, E_3 respectively, for he is then nearest, and, being opposite the sun, comes to the meridian at midnight. When in or near conjunction, as at J_2, the Earth at E_2, he is invisible, being on the meridian at midday, close by the sun. When Jupiter is in quadrature, as at Q, J, J′, Q′, the Earth at q, E, E′, q′ respectively, the line of sight from the Earth to Jupiter is inclined at its greatest angle (q Q S, E J S, &c.) to the line from the sun to Jupiter, and Jupiter must be in some degree gibbous. But his gibbosity is too slight to be perceptible[2] to ordinary

[1] Obviously, while the constant advance of Jupiter round the sun is indicated (for the movements we have considered) by the lines S Q, S j, S J_1, &c., the retrogression of Jupiter from station to station is shown by the angle between the lines e j and *e j*, on either side of the opposition line S J_1, and again by the angle between the lines *e′ j′* and e′ j′ on either side of the opposition line S J_3. The angle *j* O *j′* measures the apparent advance of Jupiter between a station following opposition and the station preceding the next opposition.

[2] The radius of Jupiter on the side where he is most affected by this gibbosity is obviously reduced in the proportion of Q *l* to Q *k*, where *q l* is an arc of a circle round Q as centre, *q k* a perpendicular from *q* on S Q. For if Q *l* represent the radius of Jupiter's disc seen fully illuminated as in opposition, Q *q* will represent the radius bounding full illumination when the rays of the sun are inclined (as at Jupiter's quadratures) at an angle S Q *q* to Q *l*; so that to an observer looking squarely towards Q *l*, the apparent limit of illumination will be at *k*, where *q k* is square to Q *l*. The diameter of Jupiter most affected by gibbosity will be to the full diameter (in the same direction, for Jupiter has diameters differing in length) as Q *l* + Q *k* : 2Q *l*, which is very nearly one of equality. The mean value of the angle S Q *q* (whose sine is the Earth's mean distance ÷ Jupiter's) is 11° 5′, and therefore Q *k* : Q *l* :: *cos* 11° 5′ : 1. Hence Q *k* = ·981 if Q *l* = 1, or the diameter most affected by gibbosity is reduced in the ratio of 1,981 to 2,000, or by less than one-hundredth part.

telescopic vision. It is recognisable, however, on very careful measurement. Moreover, the satellites of Jupiter, when occulted on the side reduced by gibbosity, have been observed to disappear when distinctly not yet in contact with Jupiter's luminous edge.

(1270.) Jupiter presents a somewhat flattened form, such as is shown in fig. 375, which gives a rough view of the planet's globe, J, for comparison with E, the globe of the Earth. The compression is estimated at about $\frac{1}{17}$, but measurements differ somewhat, the feature being delicate. Calculation has suggested $\frac{1}{14}$ as the compression which the planet's globe should present, and for a long time observation agreed in a remarkable way with calculation. But it is almost certain that the compression is much less, and from the best observations $\frac{1}{17}$ appears to be a reasonable mean estimate. The equatorial diameter of Jupiter is about 86,520 miles, the polar diameter being about 81,430 miles. The volume of the planet is thus found to be about 1,228 times greater than the volume of the Earth. In mass or quantity of matter also Jupiter largely surpasses the Earth, but not so largely as in volume. He is $315\frac{2}{5}$ times more massive than the Earth. It follows that his mean density is only ·257 of the Earth's.

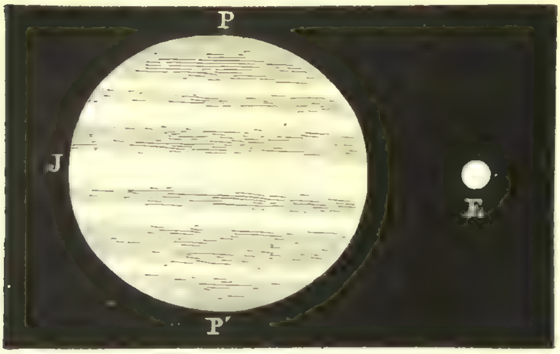


FIG. 375.—The globes of Jupiter, J, and the Earth, E, on the same scale. (From *Elementary Lessons in Astronomy*. See note, p. 319.)

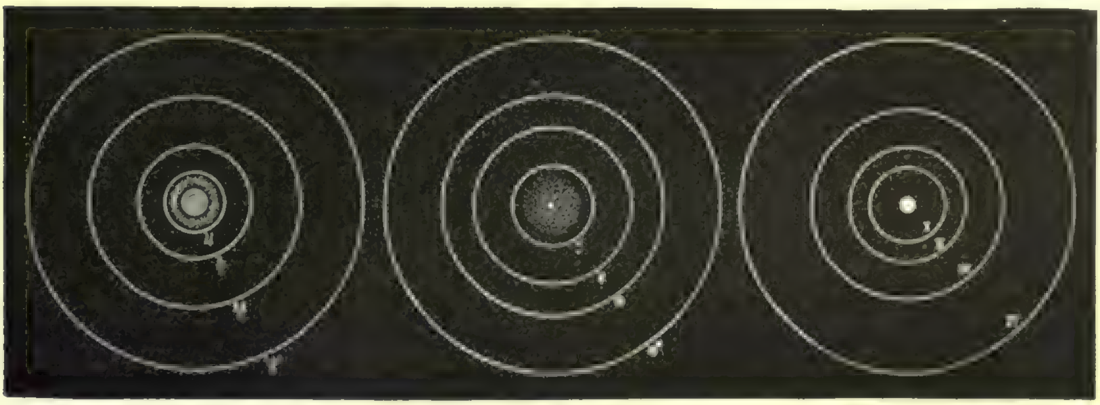
(1271.) The planet's axis is inclined about 3° 5′ from uprightness to the plane of the planet's orbit. Thus there is no such inclination as would produce seasons if the planet were like our Earth in other respects. The position of what would be the vernal equinox of the planet in that case is indicated in Plate X. It cannot be regarded as very exactly determined, either from the rotational aspect of Jupiter or from the motions of the satellites, but probably we shall not be far from the truth in saying that when Jupiter is in longitude 133° he is at the vernal equinox of his northern hemisphere.

(1272.) On January 7, 1610, Galileo first examined Jupiter with a telescope of sufficient power to show the satellites distinctly. On that night, indeed, when three satellites were visible at the hour of his observation, he supposed them to be but stars seen in the same field of view, two on the east and one on the west of Jupiter. But on the following night, 'moved,' as he says, 'I know not by what power' (*nescio quo fato ductus*), to examine the planet again, he saw the three supposed stars quite differently arranged, being now

all on the west of the planet, and closer together than before. It gives a strange idea of the imperfection of astronomical tables in those days that, finding the planet now on the east of all the stars, whereas according to the tables the planet should have been retrograding or moving towards the west, Galileo suspected that the tables were in error, and the planet might really be moving eastwards. Such a discrepancy would not only imply a gross error in the tables, but very imperfect observational means possessed by Galileo. The matter could have been settled by a few minutes' calculation based on the position of the planet with regard to the sun. However, Galileo waited till the next night, which unfortunately proved to be cloudy, and only on the night of the 10th made the decisive observations. He then saw two of the stars on the east of Jupiter, and concluded that one of the three he had seen was behind the planet. Continuing his observations, he presently satisfied himself that the supposed stars were bodies travelling around Jupiter as the moon travels around the Earth. On the 13th he discovered a fourth satellite, completing the system as now known.

(1273.) Galileo showed that these bodies travel on nearly circular paths round Jupiter, forming thus a miniature of the solar system. It is note-

Fig. 376. Fig. 377. Fig. 378.



I. II. III.

Comparing—I. (fig. 376) the system of giant planets; II. (fig. 377) the system of terrestrial planets; and III. (fig. 378) the system of satellites travelling around Jupiter. (The scale of fig. 376 is $\frac{1}{30}$ that of fig. 377, $\frac{1}{2285}$ that of fig. 378.)

worthy, however, that the system of Jupiter may be regarded as a much truer perfect miniature of the system of terrestrial planets than of the entire solar system, and as an almost equally true miniature of the system of giant planets. In figs. 376, 377, 378, for example, we have the three systems drawn on such a scale that the outer orbit in each case has the same diameter. The eccentricities of the orbits are not indicated, because, being variable, they do not need to be considered in such a comparison. It will be seen that the

systems differ chiefly in the relative size of the central body, but that, so far as the arrangement of the orbits is concerned, they closely resemble each other.

(1274.) The motions of the satellites within this system were found to accord (*inter se*) with the laws of Kepler.

(1275.) Naturally Galileo's discovery excited much interest among students of science, and much pain among those who, though calling themselves men of science, objected to see science pass beyond the limits with which they had been familiar. The weaker sort were offended for another reason, not being able to reconcile the discovery of a miniature of the solar system with the limited ideas they had formed about the universe. The aged mathematician Clavius expressed the opinion that the satellites of Jupiter were the children of Galileo's telescope; but the honest Jesuit frankly admitted his mistake when he had himself seen them. Deterred by this backsliding, those of feebler faith declined to look, lest they should be perverted by what they saw. Of one of these who died soon after, Galileo expressed the hope that the doubter might see the satellites on his way to heaven.

(1276.) When at length it was impossible to deny the existence of Jupiter's moons, it became the fashion to dispute the real character of their movements. It was argued that these objects do not revolve around the planet, but travel backwards and forwards behind its disc. Down to the middle of the seventeenth century many refused to believe that the satellites actually circulate around Jupiter.[1]

The discovery, by Cassini, in 1665, that the satellites can be traced when their orbital motions carry them between the planet and the Earth, placed the true character of these bodies beyond a doubt. By means of Campani's object-glasses of 100 and 136 feet focal length, Cassini was able to see the satellites projected as small bright spots on the disc of the planet. He found also that their motions when thus situated are precisely those due to an orbital motion around the planet, and therefore very different from those of bodies attached to the planet. This circumstance, and the fact that the bright spots remain unchanged in form as they pass over the disc, proved incontestably that he had not mistaken bright spots, such as are sometimes seen on the body of the planet itself, for the satellites whose ingress on the disc he had previously watched. But he was able to detect another evidence of the true

[1] For aught I know the motion of the satellites may be denied to the present day. In the preface to the last edition (1823) of the *Principia*, edited by the learned Jesuits Le Sueur and Jacquier, there occurs the following remarkable passage: 'In adopting the theory of the Earth's motion, to explain Newton's propositions, we assume another character than our own, for we profess obedience to the decrees of the popes against the motion of the Earth.' It is, therefore, not wholly impossible that decrees may have been promulgated against the circulation of Jupiter's satellites also.

nature of these bodies, since he discovered that the shadows which they cast upon the body of the planet are visible as small dark spots upon the disc.

(1277.) The satellites travel at distances of 261,550 miles, 416,460 miles, 664,940 miles, and 1,231,200 miles from Jupiter's centre, so that their distances from Jupiter's surface are 218,290 miles, 373,200 miles, 621,680 miles, and 1,187,940 miles respectively. (The mean distance of our moon from the Earth's surface is about 235,000 miles.) The entire span of the system of satellites is 2,462,400 miles, or rather more than five times the span of our moon's orbit, and about $2\frac{7}{10}$ times the diameter of the sun. Referring to Plate X., it will be seen by the scale of lengths that the whole system of Jupiter would not span the breadth of the fine circular line by which the path of Jupiter itself is represented.[1]

(1278.) The diameters of the satellites have not been satisfactorily determined, but within perhaps two or three hundred miles may be represented as follows :—

I., 2,500 miles ; II., 2,200 miles ; III., 3,700 miles ; and IV., 3,100 miles.

Fig. 379 shows how they compare in size with each other : as the second is almost exactly equal to our moon, fig. 379 shows also how they compare with the moon and with our Earth. Fig. 379 indicates, moreover, the relative dimensions of the shadows and penumbra of each of the four moons on the surface of Jupiter.

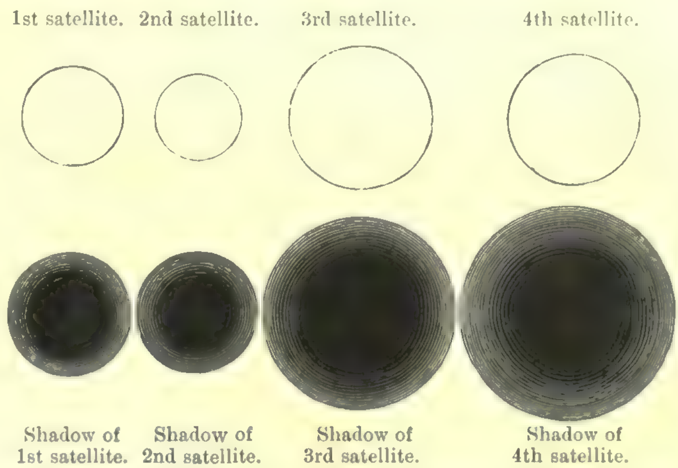


Fig. 379.—Relative dimensions of the four satellites of Jupiter and their shadows.

(1279.) The masses of the satellites are better known than their magnitudes. The densities, since they depend on the magnitudes, are not better known. The following may be accepted as a probable estimate of the densities of the four satellites :—

[1] The incorrectness will be recognised of those representations of the solar system in which, often without explanation, the system of Jupiter is shown with a span corresponding to a third or even half the distance between the orbits of Jupiter and Saturn.

	Density, Earth's as 1.	Density, Water's as 1.
I.	0·20	1·12
II.	0·37	2·07
III.	0·33	1·85
IV.	0·26	1·46
Our moon	0·61	3·42

But quite probably the effects of irradiation have so far increased the observers' estimates of the apparent diameters of these bodies, that (the true magnitudes being much less) the actual densities are considerably greater than those noted above. Possibly they are not much less than the density of our moon.

(1279*a*.) In the telescope Jupiter presents an appearance altogether unlike that of Mars. Instead of features which may be regarded as representing lands and seas, permanent in form, and only varying in appearance according as clouds or haze or snows form or melt away, Jupiter presents features constantly varying. The most marked among these are the belts, parallel bands, some darker, some lighter, and variously tinted. These may be regarded as indicating the rotation of the planet on its axis, independently of that oblateness of figure which has already indicated the same circumstance. The bands have also, in general, the position corresponding to the rotation of Jupiter, being parallel to the planet's equatorial axis as already determined.

(1280.) Fig. 380, from a drawing by Capt. W. Noble, affords a good general idea of the appearance of Jupiter in a telescope of small power. It may be remarked at the outset that the existence of variable belts always corresponding in a general sense with the planet's axial position affords positive evidence of difference in rotational velocity due to difference of distance from the axis of rotation. Thus in our Earth we have the zones of the trade and counter-trade winds caused by difference of rotational velocity in different latitudes. We can hardly attribute Jupiter's belt-zones to this cause, however, since they occupy varying positions, besides varying in number, colour, and other respects. We must therefore attribute them to differences of distance

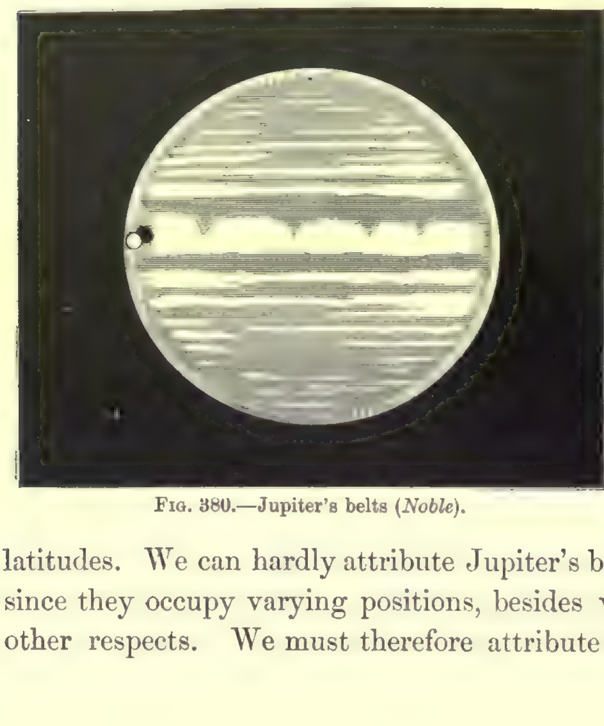


Fig. 380.—Jupiter's belts (*Noble*).

from the axis of rotation, such differences arising from vertical motions—upwards, or downwards—in Jupiter's atmosphere, which, thus viewed, would seem to be of great depth. We shall see presently that there is decisive evidence showing this to be the case.

(1281.) The student will find it instructive to compare such a picture of Jupiter as fig. 380, the work of a small telescope, with the picture given in fig. 381, the work of a powerful telescope 13 inches in diameter. But fig. 381 must not be regarded as presenting the most characteristic aspect of the planet. In fact, the aspect of Jupiter, illustrated (admirably) by Mr. De la Rue in this picture, is remarkable rather for the absence of striking features such as Jupiter usually presents. In fig. 380 we recognise indications of the formation of irregular markings along the Jovian belts. One belt only is marked in this way in fig. 380, viz. the great equatorial belt. Usually this belt is of a somewhat yellowish-white tint; but occasionally it assumes a well-marked tint of orange-yellow, or even ochreish red, and decidedly darker than the rest of the planet. In fig. 380 the markings on the equatorial belt are somewhat regularly disposed. Occasionally the regularity of arrangement is remarkable, the whole equatorial belt (and sometimes other belts) being occupied by egg-shaped forms resembling shaded clouds, ranging from five or six thousand to nine or ten thousand miles in (longer) diameter. This arrangement is very striking in fig. 382, showing Jupiter as seen with the great Parsonstown reflector; but often the regularity of the rounded clouds is even more marked than this, as in the drawings of Jupiter by Lohse, when the rounded clouds were farther apart. There is another peculiarity occasionally presented by the equatorial belt which occasionally exhibits features which have been called the 'pipe-bowl markings' (a word due to the same fanciful

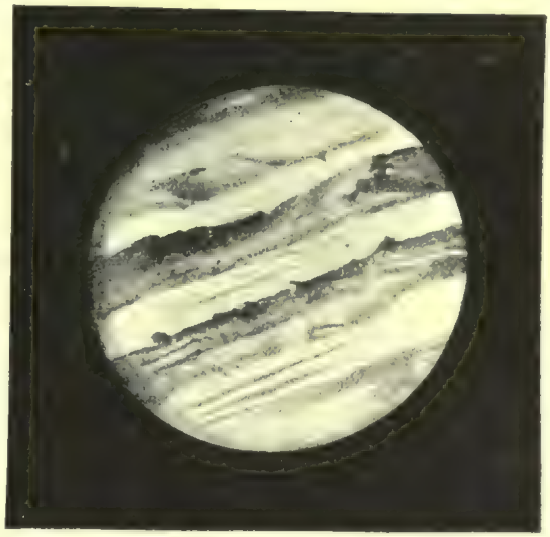


FIG. 381.—Jupiter in 1856 (*De la Rue*).

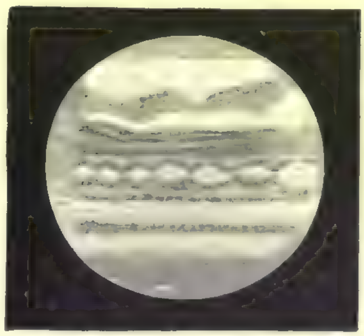


FIG. 382.—Jupiter on January 24, 1873. (Drawn with the great Rosse telescope).

imagination which named the regularly-arrayed rounded clouds the 'port-hole markings'). It is shown in fig. 384 (drawn with the great Rosse telescope).

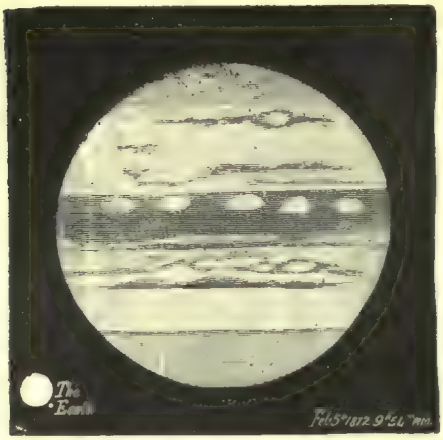


FIG. 383.—Jupiter, February 5, 1872 (*Lohse*).

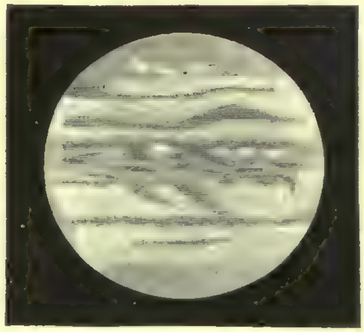


FIG. 384.—Jupiter, February 7, 1872 (*Great Rosse telescope*).

This peculiar marking may be regarded perhaps as an inchoate form of the slant streaks such as that shown in fig. 385, which appear to be rifts in the cloud-covering of the planet and often change in form, slowly in appearance, but so rapidly, when the distance of Jupiter is taken into account, as to indicate hurricane movements in the atmosphere of Jupiter, varying up to 300 miles per hour. Figs. 386 and 387 represent the same rift (not the one shown in fig. 385, which was drawn ten years later) as seen by different observers on March 12 and April 9, 1860, and serve to indicate the rapid changes taking place in Jupiter's atmosphere. In fig. 387 we recognise, in the upper (or southern part) the existence of a number of parallel belts.

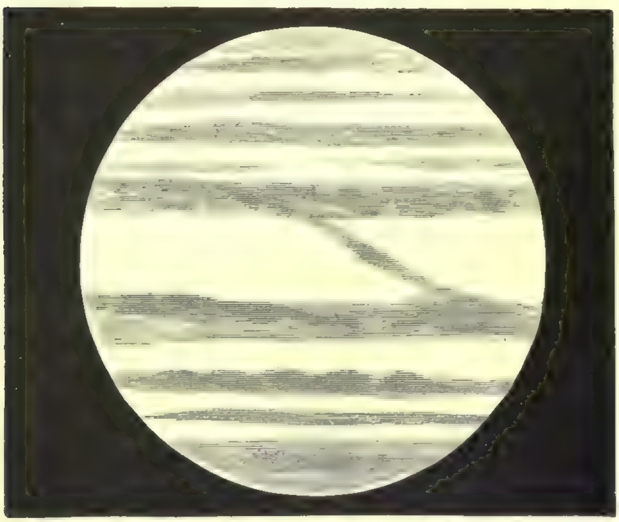


FIG. 385.—Jupiter, showing great slant rift (*Browning*).

This is seen even more strikingly in fig. 388, which is also interesting as illustrating the occasional irregularity of the belts, one large belt in the neighbourhood of many uniform small belts consisting in part of a narrower portion along which are several cloud-masses, while the rest is much widened,

and shows between the expanded sides a great cloud-mass comparable with that shown in fig. 389, presently to be considered.

(1282.) In fig. 389 we see several dark spots, openings apparently in the cloud-belts (the lighter portions being probably the clouded parts of the Jovian atmosphere). But this picture is chiefly interesting for the numerous small bright spots, on light belts, as if clouds had been formed above the belts from the condensation of vaporous masses flung through the zones of cloud.

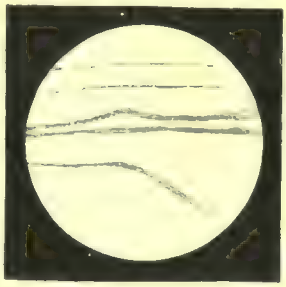


Fig. 386.—Jupiter, March 12, 1860 (*Jacob*).

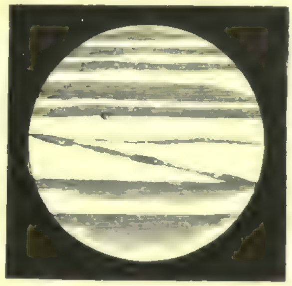


Fig. 387.—Jupiter, April 9, 1860 (*Baxendell*).

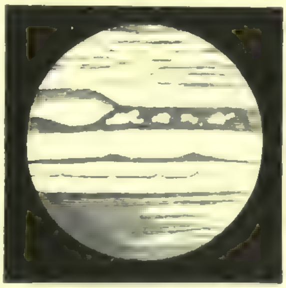


Fig. 388. Jupiter, November 27, 1857 (*Dawes*).

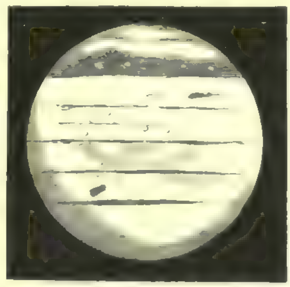


Fig. 389. Jupiter, November 18, 1858 (*Lassell*).

(1283.) But the most remarkable feature which has yet been observed on Jupiter is what has been called the Great Red Spot. In 1870 the ring-shaped oval pictured in fig. 390 was observed on the southern hemisphere of Jupiter, and probably this feature, had it been continuously observed, would have been seen to develop more or less gradually into an oval spot, the white central part changing to a uniform somewhat dull red colour. Be this as it may, Professor Pritchett, of Glasgow, Mo., observed during the opposition of 1877 the oval spot depicted in fig. 391, in the same belt, and of the same dimensions as the oval ring which had been seen seven years before. This spot remained visible as a ring for five or six years, after which it alternated in aspect between the ring form shown in fig. 390 and the spot form shown in fig. 391, sometimes showing in full distinctness the spot form, at others, as in fig. 392, a somewhat duller presentation of that form, at others the irregular appearance seen in fig. 393, while at yet others it appeared as simply a ring, resembling that shown in fig. 390. On April 21, 1886, the red spot was photographed by the Brothers Henry, of the Paris Observatory, and fig. 394 has been formed directly from the photograph, so that the student can place perfect reliance on the details shown in this figure. MM. Henry remark of the negative they obtained that the red spot is more striking and better defined (*plus nette*) in the photograph than by direct telescopic vision.

(1284.) The late Dr. Henry Draper, of New York, one of our ablest spec-

troscopists, thought that the red spot gave signs, when examined with the spectroscope, of shining in part by inherent light. The general spectroscopic examination of Jupiter, while indicating, as might be expected, the presence in Jupiter's atmosphere of large quantities of the vapour of water, has also given occasional evidence (and more could not be expected) of the presence of partially inherent light, especially in the equatorial zone. A strong band in the red, noted by Vogel, agrees in position with a dark band in the spectrum of some of the red stars. As might be expected, the darker markings, which are mostly ruddy in tint, give spectroscopic evidence of strong absorption of the blue rays, a characteristic which also corresponds with what is noticed in the spectra of certain red stars. In fact, it would seem that the inherent light of Jupiter, which forms probably but a small portion of the light we receive from the planet, is closely akin to the light of those red stars which have been regarded as far advanced in stellar old age, and that were it not for the light, almost white, which Jupiter reflects he would shine with a well-marked red colour.

FIG. 390.—Great oval ring on Jupiter in 1870.

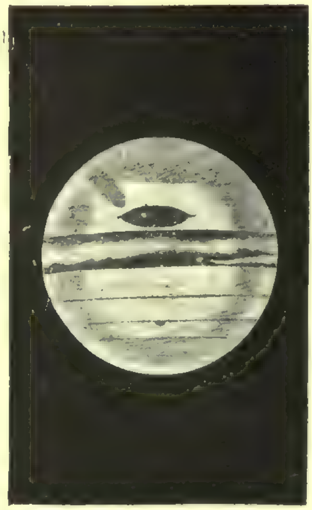


FIG. 391.—Great oval red spot on Jupiter in 1877.

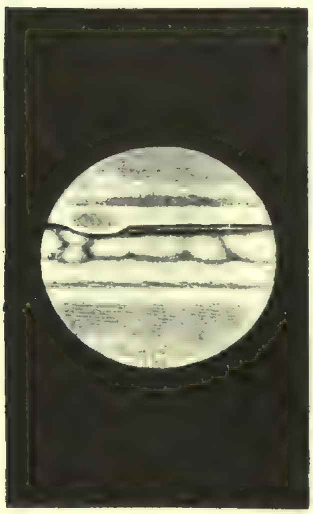


FIG. 392.—Jupiter, May 17, 1883.

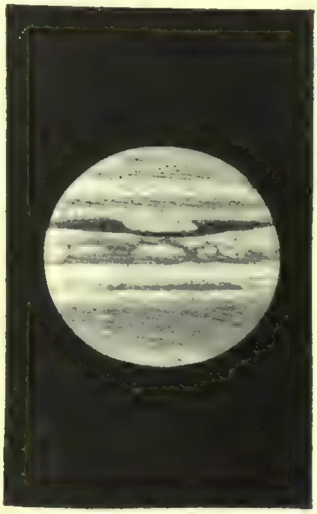


FIG. 393.—Jupiter, September 10, 1883.

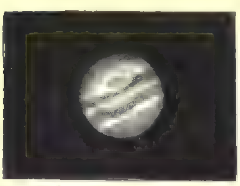


FIG. 394.—Engraved by photographic process from a photograph of Jupiter (*MM. Henry*), April 21, 1886.

It would seem to be from the interior that the inherent light of Jupiter

chiefly, if not wholly, proceeds. The outer portions of his atmospheric envelope are relatively cool, and the cloud masses which float in them probably resemble our own clouds in regard at least to the quality of the light which they reflect. Towards the edge of Jupiter's disc the light fades off (as may be shown by photometric methods or by the appearance of the satellites in transit) in marked degree. It is customary to attribute this solely to the slant incidence of the light; but I shall be able, I think, to show, or at least to render it highly probable, that another, and much more interesting, cause is in question.

(1285.) It remains to be mentioned, before we proceed to discuss the phenomena of Jupiter's atmosphere in their physical significance, that the planet rotates in a mean period of about 9 hours $55\frac{1}{2}$ minutes, the equatorial regions, however, being carried somewhat more swiftly round, probably in about 9 hours 50 minutes, as in the case of the equatorial regions of the sun compared with regions far from the sun's equator. The difference, though slight measured by time, nowhere perhaps amounting to much more than five minutes per rotation, indicates a rapid proper (*i.e.* relative) motion of parts in high latitudes westward from the equatorial regions. For it must be remembered that the rotation of Jupiter on his axis in 9 hours 55 minutes indicates for the equatorial regions a rotational velocity of no less than 7·7 miles per second, the most rapid velocity of this sort known throughout the whole solar system. The velocities in high latitudes are less, but still they remain so high that a difference of five minutes in the rotation period, corresponding to $\frac{1}{119}$ part of the full rotational velocity, signifies very rapid proper motion.

(1286.) The inquiry into the probable condition of Jupiter should begin with a comparison between what we recognise in the planet and what we know respecting our own Earth. For though *a priori* considerations show that Jupiter is not at all likely to resemble the Earth, yet it is always safest to consider direct evidence before inquiring how far such evidence agrees with what might have been anticipated by *a priori* reasoning.

The first point which attracts our attention is the remarkable circumstance that Jupiter's atmosphere should be recognisable from our distant standpoint. Our terrestrial atmosphere could not be recognised at Jupiter's distance with any telescopic power yet applied, except by the effects produced when clouds form and dissipate. But no one who has studied Jupiter with adequate means can for a moment fail to recognise other signs of an atmosphere beside the mere formation and dissipation of clouds.[1]

The depth of terrestrial cloud-masses would at Jupiter's distance be an absolutely evanescent quantity. Consider fig. 383, where the Earth is shown

[1] No one can study Jupiter for many hours (on a single night) without becoming convinced that the cloud-masses seen on his disc have a *depth* comparable with their length and breadth.

beside Jupiter, and try to conceive what the whole depth of the Earth's atmosphere would appear if the Earth were viewed as we view Jupiter. If the whole depth of atmosphere would be all but evanescent, what would the depth of the cloud-bearing layers resemble ? Jupiter's satellites, which look little more than points in ordinary telescopes, are all more than 2,000 miles in diameter. The behaviour of Jupiter's cloud-belts shows that their depth cannot be less, and is probably much greater, than the twentieth part of the diameter of the least satellite. It must be a deep atmosphere in which float such cloud-masses as are pictured in figs. 380–389.

(1287.) In dealing with this question, we must not forget that the atmosphere of Jupiter is attracted by the planet's mass. Some rather remarkable consequences follow from this. In inquiring whether there can be any resemblance between Jupiter and our Earth, we may assume that the atmosphere of Jupiter does not differ greatly in constitution from that of our Earth, because if we shall be led on this assumption to recognise difference, we know certainly that there must be difference, even though our assumption be erroneous—for rejecting the assumption is rejecting the idea of resemblance at the outset. We may further assume that at the upper part of the visible cloud-layers the atmospheric pressure is not inferior to that of our atmosphere at a height of seven miles above the sea-level, or one-fourth of the pressure at our sea-level. Combining these assumptions with the inference deduced from observation, that the cloud-layers are at least 100 miles in depth, we are led to the following singular result as to the pressure of the Jovian atmosphere at the bottom of the cloud-layer :—The atmosphere of any planet doubles in pressure with descent through equal distances, these distances depending on the power of gravity at the planet's surface. In the case of our Earth, the pressure is doubled with descent through about $3\frac{1}{2}$ miles ; but gravity on Jupiter is more than $2\frac{1}{2}$ times as great as gravity on our Earth, and descent through $1\frac{2}{5}$ mile would double the pressure in the case of a Jovian atmosphere of similar constitution and assumed to exist under similar conditions. Now one hundred miles contain this distance ($1\frac{2}{5}$ mile) more than seventy-one times ; and we must therefore double the pressure at the upper part of the cloud-layer seventy-one successive times to obtain the pressure at the lower part. Two doublings raise the pressure to that at our sea-level; and the remaining sixty-nine doublings would result in a pressure exceeding that at our sea-level so many times that the number representing the proportion contains twenty-one figures. I say *would* result in such a pressure, because in reality there are limits beyond which atmospheric pressure cannot be increased without changing the compressed air into the liquid form.[1]

[1] If we supposed the pressure and density to increase continually to the extent implied by the number of twenty-one figures, we should have a density exceeding that of platinum more than ten

(1288.) Jupiter's mean density supplies an argument of irresistible force against the only view which enables us even hypothetically to escape from the conclusions just indicated. Let it be granted, for the sake of argument, that Jupiter's cloud-layer is *less* than fourteen miles in depth, so that we are freed for the moment from the inference that at the lower part of the atmosphere there is either an intense heat or else a density and pressure incompatible with the gaseous condition. We can, in this case, strike off twenty-eight miles, but no more, from the planet's apparent diameter to obtain the real diameter of his solid globe—solid, at least, if we are to maintain the theory of his resemblance to our Earth. This leaves his real diameter appreciably the same as his apparent diameter, and as a result we have the mean density of his solid globe equal to a little more than a fourth of the Earth's mean density, precisely as when we leave his atmosphere out of the question. The time has long since passed when we can seriously proceed at this stage to say, as it was the fashion to say in text-books of astronomy, 'therefore the substance of which Jupiter is composed must be of less specific gravity than oak and other heavy woods.' We know that Brewster gravely reasoned that the solid materials of Jupiter might be of the nature of pumice-stone, so that with oceans resembling ours a certain latitude was allowed for increase of density in Jupiter's interior. But in the presence of the teachings of spectroscopic analysis, and with the evidence we have as to the common origin of all the members of the solar system, few would now care to maintain such a theory as this. Everything that has hitherto been learned respecting the constitution of the heavenly bodies renders it quite unlikely that the elementary constitution of Jupiter differs from that of our Earth. Nor can we now imagine Jupiter to be a hollow globe, a shell composed of materials as heavy as terrestrial elements. Whatever opinion we may form as

thousand millions of millions of times. Of course this supposition is utterly monstrous, and I have merely indicated it to show how difficulties crowd around us in any attempt to show that a resemblance exists between the condition of Jupiter and that of our Earth. The assumptions made were sufficiently moderate, be it noticed, since I simply regarded (1) the air of Jupiter as composed like our own; (2) the pressure at the upper part of his cloud-layer as not less than the pressure above the highest of our terrestrial cumulus clouds (with which alone the clouds of Jupiter are comparable); and (3) the depth of his cloud-layer as about 100 miles. The first two assumptions cannot fairly be departed from to any considerable extent, without adopting the conclusion that the atmosphere of Jupiter is quite unlike that of our Earth—a conclusion which is only another form of that to which my argument tends. The third is, of course, less certain, though I apprehend that no one who has observed Jupiter with a good telescope will question its justice. But it is not at all essential to the argument that the assumed depth of the Jovian atmosphere should be even nearly so great. We do not need a third of our array of twenty-one figures, or even a seventh part, since no one who has studied the experimental researches made into the condition of gases and vapours can for a moment suppose that an atmosphere like ours could remain gaseous, *except at an enormously high temperature*, at a pressure of two or three hundred atmospheres. Such a pressure would be obtained, retaining our first two assumptions, at a depth of about fourteen miles below the upper part of the cloud-layer.

to the possibility that a great intensity of heat may vaporise a portion of Jupiter's interior, we know quite certainly that there must be enormous pressure throughout the whole of the planet's globe, and that even a vaporous matter within it would be of great density.[1]

(1289.) In considering the effect of pressure on the materials—solid, liquid, or vaporous—within a massive globe like a planet, we must not fall into the mistake of supposing that the strength of such solid materials can protect the material from compression and its effects. We must extend our conceptions beyond what is familiar to us. We know that any ordinary mass of some strong, heavy solid—as iron, copper, or gold—is not affected by its own weight so as to change in structure to an appreciable extent. The substance of a mass of iron forty or fifty feet high would be the same in structure at the bottom as at the top, for the strength of the metal would resist any change which the weight of the mass would (otherwise) tend to produce. But if there were a cubical mountain of iron twenty miles high, the lower part would be absolutely plastic under the pressure to which it would be subjected. It would behave in all respects as a fluid, insomuch that if (for convenience of illustration) we suppose it enclosed within walls made of some imaginary (and impossible) substance which would yield to no pressure, then, if a portion of the wall were removed near the base of the iron mountain, the iron would flow out like water[2] from a hole near the bottom of a cask. The iron would continue to run out in this way, until the mass was reduced several miles in height. In Jupiter's case a mountain of iron of much less height would be similarly plastic in its lower parts, simply because of the much greater attractive power of Jupiter's mass. The conception, then, of a hollow interior, or of any hollow spaces throughout the planet's globe, is altogether inconsistent with what is known of the constitution of even the strongest materials.

(1290.) On the supposition that Jupiter's atmosphere is shallow we cannot explain the relatively small mean density of his globe. There is nothing hypothetical in the above considerations respecting a solid globe as large as Jupiter's. We can only escape the conclusion just indicated by assuming that Jupiter's mass is formed of substances unlike any with which we are familiar. This assumption, while it is one which cannot reasonably be maintained in the present position of our knowledge, assumes in reality the inference to which all our reasoning tends—viz. that Jupiter is

[1] At exceedingly high temperatures much greater pressure, and therefore much greater density, can be attained without liquefaction or solidification.

[2] The effect of pressure in rendering iron and other metals plastic has been experimentally determined by Signor Tresca and others. Cast steel has been made to flow almost like water under pressure.

utterly unlike the Earth. Rejecting it, as we safely may, we find the small density of Jupiter without valid explanation.

(1291.) All reasoning has been based on the assumption that the atmosphere of Jupiter exists at a temperature not greatly differing from that of our own atmosphere. If we assume instead an exceedingly high temperature, abandoning of course the supposition that Jupiter is an inhabited world, we no longer find any circumstances which are self-contradictory or incredible:—

To begin with, we may on such an assumption find at once a parallel to Jupiter's case in that of the sun. For the sun is an orb attracting his atmospheric envelope and the material of his own solid or liquid surface (if he has any) far more mightily than Jupiter has been known to do. All the difficulties considered in the case of Jupiter would be enormously enhanced in the case of the sun, if we forgot the fact that the sun's globe is at an intense heat from surface to centre. Now we know that the sun is intensely hot because we feel the heat that he emits, and recognise the intense lustre of his photosphere; so that we are not in danger of overlooking this important circumstance in his condition. Jupiter gives out no heat that we can feel, and assuredly Jupiter does not emit an intense light of his own. But, when we find that difficulties precisely corresponding in kind, though not in degree, to those which we should encounter if we discussed the sun's condition in forgetfulness of his intense heat exist also in the case of Jupiter, it appears manifest that we may safely adopt the conclusion that Jupiter is intensely heated, though not nearly to the same degree as the sun.

(1292.) We have thus been led by a perfectly distinct and independent line of reasoning to the conclusion which other evidence (Art. 1281) has already supplied, that Jupiter is sufficiently heated to emit through a non-luminous cloud-laden atmosphere some small proportion of inherent light.

(1293.) But so soon as we regard the actual phenomena presented by Jupiter in the light of this hypothesis, we find the means of readily interpreting what otherwise would appear most perplexing; and it must be remembered in considering these phenomena that changes taking place in the atmosphere of Jupiter through solar action might be expected to be exceedingly slow—so slight, in fact, as to be scarcely perceptible.[1] It is manifest that, on the one hand, seasonal changes would be slow and slight so far as they depend on

[1] It is not commonly insisted upon in our text-books of astronomy—in fact, I have never seen the point properly noticed anywhere—that the seasonal changes in Jupiter correspond to no greater *relative* change than occurs in our daily supply of solar heat from about eight days before to about eight days after the spring or autumn equinox. It is incredible that so slight an effect as this should produce those amazing changes in the condition of the Jovian atmosphere which have unquestionably been indicated by the varying aspect of the equatorial zone.

the sun, and, on the other, that the sun cannot rule so absolutely over the Jovian atmosphere as to cause any particular atmospheric condition to prevail unchanged for years. Moreover, there is no correspondence whatsoever between changes in the condition of Jupiter and the slow progress of his slight seasonal changes. If Jupiter's whole mass is in a state of intense heat —if the heat is, in fact, sufficient, as it must be, to maintain an effective resistance against the tremendous force of Jovian gravitation—we can understand any changes, however amazing. We can see that enormous quantities of vapour must continually be generated in the lower regions to be condensed in the upper regions, either directly above the zone in which they were generated, or north or south of it, according to the prevailing motions in the Jovian atmosphere. And although we may not be able to indicate the precise reason why at one time the mid zone or any other belt of Jupiter's surface should exhibit that whiteness which indicates the presence of clouds, and at another should show a colouring which appears to indicate that the glowing mass below is partly disclosed, or why at one time the belts generally should be straight and uniform while at others they show manifest signs of intense disturbance, we remember that the difficulty corresponds in character to that which is presented by the phenomena of solar spots. We cannot tell why sun-spots should wax and wane in frequency during a period of about eleven years ; but this does not prevent us from adopting such opinions as to the condition of the sun's glowing photosphere as are suggested by the behaviour of the spots.

(1294.) It may be asked whether, on the hypothesis to which we have thus been led, we would suggest that the ruddy glow of Jupiter's equatorial zone (of the great red spot, &c.) is due to the inherent light of glowing matter underneath his deep and cloud-laden atmosphere, or that that light, originally white, shines through ruddy vapour-masses. The spectroscopic evidence appears to show that the latter view is the more probable. But, whichever view we adopt, we must assume that far the larger portion of the light received from Jupiter is reflected sunlight. Jupiter shines (Art. 1036, note) about as brightly as if he were a giant cumulus-cloud, and therefore almost as white as driven snow. Thus he sends us much more light than a globe of equal size of sandstone, or granite, or any known kind of earth. We get from him about three times as much light as a globe

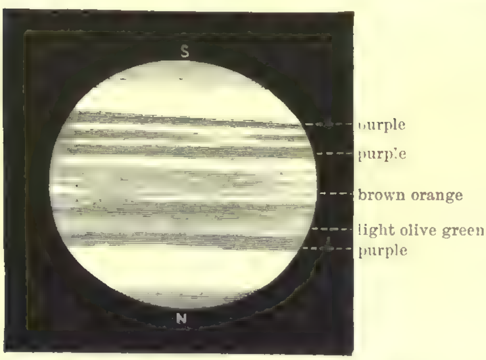


FIG. 395.—Colour of Jupiter as observed by Mr. Lassell.

like our moon in substance, but as large as Jupiter, and, placed where Jupiter is, would reflect towards the Earth ; but not quite so much as we should receive from a globe of pure snow of the same size and similarly placed. It is only because large parts of the surface of Jupiter are manifestly *not* white, that the evidence deduced from his brightness independently compels us to assume that some portion of his light is inherent.[1]

(1295.) But the theory that Jupiter is intensely hot by no means requires that he should give out a large proportion of light. His real solid or liquid globe (if he have any) might, for instance, be at a white heat, and yet so completely cloud-enwrapped that none of its light could reach us. Or, again, his real surface might be like red-hot iron, giving out much heat but very little light.

(1296.) I propose now to consider certain details of evidence, derived from the aspect and changes of aspect of Jupiter, and from the phenomena of his satellites, which appear to me to place the conclusions to which we have thus been led beyond all doubt or question :—

We have seen reason to believe (Art. 1280) that the cloud-masses forming the belts of Jupiter are affected by vertical currents, uprushing motions carrying them from regions nearer the axis, where the absolute motion due to rotation is slower, to regions farther from the axis, where the motion due to rotation is swifter, and motions of downrush carrying them from regions of swifter to regions of slower rotational motion. This view, which we can entertain with more confidence now that we have obtained independent evidence of the great depth of the planet-atmosphere, seems to be strongly confirmed when we study more closely the aspect of the Jovian belts. The white spots—some small, some large—which are seen to form from time to time along the chief belts, present precisely the appearance which we should expect to find in masses of vapour flung from deep down below the visible cloud-surface of Jupiter, breaking their way through the cloud-layers, and becoming visible as they condense into the form of visible vapour in the cooler upper regions of the planet's atmosphere. Then, again, the singular regularity with which in certain cases the great rounded white clouds are set side by side, like rows of eggs upon a string, is much more readily explicable as due to a regular succession of uprushes of vapour, from the same region below, than as due to the simultaneous uprushes of several masses of vapour from regions set at uniform distances along a belt of Jupiter's surface. The

[1] Fig. 395 is worth studying as showing how Jupiter's chiefs belts appeared to be coloured as observed with a Newtonian reflector, two feet in aperture, by the late Mr. W. Lassell, an observer of great skill and experience, especially with reflecting telescopes. He considered that such telescopes, if the alloy and the mirror is well compounded, show delicate colours more faithfully than the best refractors.

latter supposition is indeed artificial and improbable in the highest degree, and in several distinct respects. It is unlikely that several uprushes should occur simultaneously, unlikely that regions whence uprush took place should be set at equal distances from each other, unlikely that they should lie along the same latitude parallel. On the other hand, the occurrence of uprush after uprush from the same region of disturbance, at nearly uniform intervals of time, is not at all improbable. The rhythmical succession of explosions is a phenomenon, indeed, altogether likely to occur under certain not improbable conditions,—as, for instance, when each explosion affords an excess of relief, if one may so speak, and is therefore followed by a reactionary process, in its turn bringing in a fresh explosion. Now, a rhythmical succession of explosions from the same deep-seated region of disturbance would produce at the upper level, where we *see* the expelled vapour-masses (after condensation) a series of rounded clouds lying side by side, as in fig. 382. For each cloud-mass—after its expulsion from a region of slow (absolute) rotational motion to a region of swifter motion—would lag behind with reference to the direction of rotational motion. The earlier it was formed the farther back it would lie. Thus each new cloud-mass would lie somewhat in advance of the one expelled next before it; and if the explosions occurred regularly, and with a sufficient interval between each and the next to allow each expelled cloud-mass to lag by its own full length before the next one appeared, there would be seen precisely such a series of egg-shaped clouds, set side by side, as every careful observer of Jupiter with high telescopic powers has from time to time perceived.

(1297.) That these egg-shaped clouds are really egg-shaped—not merely oval in the sense in which a flat elliptic surface is oval—is suggested at once by their aspect. But it is more distinctly indicated when details are examined. These clouds are shaded on the side farthest from the sun, in such a way as to show that they are rounded convexly towards the observer. They also cast shadows—or rather shaded regions are seen where such rounded clouds would cast shadows—but the outlines of these shaded regions are too soft and their tints are too little uniform to allow us to regard them as shadows cast upon an opaque surface, such as the real surface of the planet may be supposed to be. The phenomena here described are not always to be seen. On some occasions the white cloud-masses have not had the extent to be expected from the planet's position with reference to the sun. But this can be understood when we consider that Jupiter's atmosphere is doubtless variable in translucency, and not always penetrable to the same depth by the solar rays. When the shadows have been shorter it is probable that a layer of clouds interrupted the rays, and

thus the shadows were much closer to the cloud-masses throwing them than they would have been had that layer not been there.

(1298.) It appears, then, that in a semi-transparent atmosphere of enormous depth, surrounding Jupiter, there float vast cloud-masses, sometimes in layers, at others in irregular heaps, at others having well rounded forms. These cloud-masses undergo sometimes remarkable changes of shape, often forming or disappearing in a very short time, and thus indicating the great activity of the forces at work below them,—in other words, the intense heat of Jupiter's real globe. As to the actual depth of the semi-transparent atmosphere in which these cloud-layers and cloud-masses float, it would be difficult to express an opinion. We do not know how many cloud-layers there are, how thick any cloud-layer may be, how great may be the depth of the vast rounded masses of cloud whose upper surface (that is, the surface remotest from Jupiter's true surface) we can alone see under favourable conditions. But we can indicate a minimum than which the atmosphere's depth is probably not less ; and from all the observations which I have examined as bearing on this point, I should be disposed to assign for that minimum at least 6,000 miles. Probably, however, the depth of the Jovian atmosphere is still greater.[1]

(1299.) But we have still to consider the velocities with which rounded masses of cloud travel in the very deep atmosphere of Jupiter. 'There is clear evidence,' I have pointed out in the article 'Astronomy' of the 'Encyclopædia Britannica,' 'that spots on Jupiter are subject to a proper motion like that which affects the spots on the sun. Schmidt, in No. 1,973 of the "Astronomische Nachrichten," gives a number of cases of such proper movements of spots, ranging in velocity from about 7 miles to about 200 miles an hour. It may be noted, also, that from a series of observations of one spot, made between March 13 and April 14, 1873, with the great Rosse reflector, a period of 9h. 55m. 4s. was deduced, while observations of another spot in the same interval gave a rotation period of 9h. 55m. 55·4s.'[2] We have, however, instances of yet greater relative proper motion among cloud-masses.

[1] Jupiter probably has a solid or liquid nucleus, and though this nucleus—glowing, as it must be, with a most intense heat—may be greatly expanded, yet with the enormous attractive power residing in it, containing as it must nearly the whole mass of the planet, its mean density cannot be less than that of the Earth. A globe of the mass of Jupiter, but of the same mean density as our Earth, would have one-fourth of Jupiter's volume—the mean density of Jupiter, as at present judged, being equal to one-fourth that of the Earth. The diameter, therefore, of such a globe would be less than the present diameter of Jupiter in the proportion that the cube root of unity is less than the cube root of 4, or as 1 is less than 1·5874. Say roughly (remembering that the atmosphere of Jupiter must have a considerable mass) the diameter of Jupiter's nucleus would, on the assumptions made, be equal to about five-eighths of his observed diameter, or to about 53,000 miles. This is less than his observed diameter by about 22,000 miles, or the radius of his nucleus would be less than his observed radius by about 11,000 miles, which therefore would be the probable depth of his atmosphere.

[2] The actual difference of velocity would depend in this case on the actual latitudes of the two spots, which were not micrometrically measured.

(1300.) Of course we cannot put the whole of the observed difference between the rotation-rates determined from different spots on one or other set of observations. The only inference we can form is that the rotation-rate derived from some spots is different from the rotation-rate derived from others, and that some spots (if not all) are certainly not constant in position with respect to the solid nucleus of the planet. We find that spots at some distance from the equator indicate a slower rate of rotation than those nearly equatorial. For this may probably be explained by the consideration that masses (vaporous or otherwise) thrown up from the equatorial parts of the true surface of the concealed planet differ less in velocity from the atmosphere into which they were driven than would masses expelled from higher latitudes.

(1301.) This conclusion, that the spots of Jupiter have rapid rates of relative motion, would of itself be of singular interest, especially when we remember that the larger white spots represent masses of cloud 5,000 or 6,000 miles in diameter. That such masses should be carried along with velocities so enormous as to change their positions relatively to each other, at a rate sometimes of more than 150 miles per hour, is a startling and stupendous fact. But the fact is still more interesting in what it suggests than in what it reveals. The movements taking place in the deep atmosphere of Jupiter are very wonderful, but the cause of these movements is yet better worthy of study. We cannot doubt that deep down below the visible surface of the planet—that is, the surface of its outermost cloud-layers—lies the fiery mass of the real planet. Outbursts, compared with which the most tremendous volcanic explosions on our Earth are utterly insignificant, are continually taking place beneath the seemingly quiescent envelope of the giant planet. Mighty currents carry aloft great masses of heated vapour, which, as they force their way through the upper and cooler strata of the atmosphere, are converted into visible cloud. Currents of cool vapour descend towards the surface, after assuming, no doubt, vorticose motions, and sweeping away over wide areas the brighter cloud-masses, so as to form dark spots on the disc of the planet. And owing to the various depths to which the different cloud-masses belong, and whence the uprushing currents of heated vapour have had their origin, horizontal currents of tremendous velocity exist, by which the cloud-masses of one belt or of one layer are hurried swiftly past the cloud-masses of a neighbouring belt or of higher or low cloud-layers.

The planet Jupiter, in fact, may justly be described as a miniature sun,

Taking 200,000 miles as about the circumference of a parallel of latitude passing midway between the spots (only a very rough calculation need be made), we should find that in a period of one rotation, or roughly of ten hours, one spot gained on the other about 51 seconds, or roughly about $\frac{1}{700}$ part of a rotation—that is, in distance (dividing 200,000 by 700) about 286 miles in ten hours, or 28½ miles an hour.

vastly inferior in bulk to our own sun, inferior to a greater degree in heat, and in a greater degree yet in lustre, but to be compared with the sun—not with our Earth—in size, in heat, and in lustre, and, lastly, in the tremendous energy of the processes which are at work throughout his cloud-laden atmospheric envelope.

(1302.) The great red spot deserves separate consideration, on account of its exceptional character, and (in consequence) the special nature of the evidence which it may afford as to Jupiter's condition.

It is clear that, whatever the original cause of this great disturbance in the Jovian atmospheric surroundings, we cannot regard it as eruptional during its whole continuance. The oval form by its regularity suggests the wide-spreading of vaporous matter from a centre of disturbance; but it must be remembered, in considering this view, that the area affected has been no less than 150 millions of square miles, or three-fourths of the whole surface of the Earth. On the other hand, the inferences suggested by the spot's apparent regularity of figure during the greater part of its continuance are not affected by the circumstance that in telescopes of great power the spot does not appear so simply regular as in small telescopes. The extension of atmospheric currents around a centre of disturbance would not give an absolutely regular elliptical form to the disturbed area; but a general ellipticity of form accompanied by irregularities of greater or less extent (probably of greatly varying extent) around the borders of the region of disturbance.

(1303.) What seems clear about the great spot may be summed up as follows:—

When the whole spot was red, the region thus disclosed lay below the general level of the cloud-surface we see and measure, probably by many hundreds of miles. The red light was in part inherent, but probably the actual region whence inherent light proceeded lay far below the surface from which it appeared to emanate. The existence, long continuance, and rapid changes of appearance in the great spot indicated an activity in Jupiter's mass corresponding well with the theory that he is in a condition between that of a sun like our own and that of a world like our Earth.

(1304.) The great spot, as it approached the edge of Jupiter's disc, assumed an appearance which has been pictured and described as showing that the spot remains visible clear to the planet's edge. In this respect it seems to contradict the evidence, otherwise decisive, which shows that the red surface is deep below the lighter-tinted circumjacent surface of the belt. It does not seem to have been noticed that, if the Jovian atmosphere above the ruddy surface disclosed in the area of the great spot is cleared of cloud, the region thus cleared would appear dark when at the planet's edge, and, there-

fore, seem to form part of the dark spot, simply because, thus cleared of cloud, its darkness not indicating the darkness of Jupiter's surface seen through the atmosphere, but the absence of light-reflecting capacity in the cleared atmospheric region.

(1305.) The general questions thus suggested are full of interest. They are these: Is the darkening observed round the edge of Jupiter's disc wholly due to the change in the angle of incidence of the solar rays? or partly to this cause and partly to the semi-transparency of the outer atmospheric layers? or chiefly, if not wholly, to this latter cause?

We have already had much evidence tending to show that the atmospheric region [1]—the layers of mixed gases, vapours, and more or less sparsely-strewn clouds around the globe of Jupiter—is very deep. But the tenuity of the outer layers of that region would have to be enormous, the sparsity of cloud-distribution excessive, that any measurable depth of the atmospheric region should be recognised as partially transparent near the edge of the planet's disc—or, in other words, that any part of the darkening near the edge should have to be attributed to the darkness of the sky beyond made visible through the partially transparent atmospheric surroundings of the planet. Suppose a depth of only 100 miles, which at Jupiter's distance (being but the 865th part of his equatorial diameter) would be undiscernible. A line passing tangentially past a spherical surface, 100 miles within (and concentric with) the actual outer surface of Jupiter's atmospheric surroundings, would traverse a distance of more than 4,160 miles of atmospheric matter.[2] Now an atmosphere must be very tenuous, and clouds in it must be most sparsely distributed, for a line of sight 4,160 miles long to pass through it without being wholly obstructed. At a depth of 1,000 miles the range would be about 13,150 miles, and partial transparency through so great a range as this would imply an atmospheric tenuity or transparency comparable with that of comets. Yet I think I shall be able to show that there is very strong, if not absolutely demonstrative, evidence in favour of occasional transparency of the Jovian atmospheric region at the edge of the planet's apparent disc, to an even greater depth than this—even to a depth involving a range of the line of sight through ten or twelve thousand miles of what we have been accustomed (mistakenly in all probability) to regard as the actual atmosphere of Jupiter.

(1306.) Let us turn to the satellite-system of Jupiter for the evidence

[1] I use the expression 'atmospheric region' rather than atmosphere advisedly. The evidence appears to render it doubtful, in Jupiter's case as in the sun's, whether the gaseous and vaporous regions surrounding the larger masses of the solar system to great depths can be regarded as forming a continuous atmospheric envelope.

[2] By a simple geometrical relation we see that the distance would be, in the neighbourhood of Jupiter's equator, twice $\sqrt{100 \times 43{,}260}$ miles, or about 4,160 miles, and in the neighbourhood of Jupiter's poles 4,036 miles.

necessary, which has been obtained on this subject, noting in the first place that from the observed movements of the satellite-system Prof. George Darwin has shown that the real globe of Jupiter must lie far within the globe we see and measure, which, did it represent the real dimensions of the planet, would produce perturbations within the satellite-system such as have no real existence.

(1307.) It may be well, in the first instance, to consider, briefly, the circumstances under which the satellites are seen, especially as these have not hitherto been accurately dealt with, or the details correctly illustrated :—

Referring back to fig. 374, and remembering that even on the rather large scale of that picture the whole span of the satellite-system of Jupiter (being about $\frac{1}{90}$th of S.E.) would be little more than $\frac{1}{70}$th of an inch, or about double the breadth of the path representing Jupiter's track, we see how misleading those pictures are in which we have a wide system shown around Jupiter in the same diagram which represents his orbit as not many times wider in span, and the Earth's orbit scarcely as wide, or, if wider, then much too large compared with the orbit of Jupiter. It is better to consider the relations of the orbits of Jupiter and the Earth separately first, in such a diagram as fig. 374, and then, remembering that on the scale of that system the radius of the orbit of Jupiter's outer satellite would be but a hair's breadth, to pass to another diagram such as fig. 396, in which the Jovian system is represented on a greatly enlarged scale.

(1308.) Before leaving fig. 374, however, let the student observe that as the distance of the Earth from Jupiter ranges from E_1J_1, the opposition-distance to E_2J_2 the conjunction-distance, and as the phenomena of Jupiter's satellites, which take place in reality at definite epochs, can be watched from all these varying distances, except those very near the maximum (when Jupiter is too near the sun to be satisfactorily observed), certain differences must necessarily arise in the time at which the various phenomena appear to occur. For a phenomenon as observed by sight only appears to occur when the light-message recording its occurrence reaches the observer, and while the light-message from a satellite of Jupiter, just entering the planet's shadow or emerging from it, is always received at the Earth after a certain delay due to Jupiter's distance, this delay will be greater or less according to the Earth's position. Now whereas a constant delay in the light record of each event would leave the orderly sequence of the events unchanged, a varying delay, such as must in this case arise from the Earth's varying distance, must affect the intervals apparently elapsing between the different events, and so cause a certain apparent irregularity, or rather a want of perfect uniformity, in the sequence of events, when, owing to the combined

motions of the two planets, the Earth and Jupiter are approaching, the intervals between successive phenomena appear less than they really are; when the Earth and Jupiter are receding, the intervals appear greater than they really are. This was recognised before the finiteness of the velocity of light had been discovered or even imagined; but at first astronomers (in particular the younger Cassini and the older Maraldi) hesitated to accept Römer's suggestion that the finite velocity of light would explain the observed want of uniformity in the motions of Jupiter's satellites. In fact, the irregularity due to the motion of light was masked by the unexplained peculiarities of the motions of the three outer satellites (and especially of the second). So that it was not until the motion of light with measurable velocity had been demonstrated by observations of the stars, that Römer's suggestion was finally accepted. Bradley, in England, showed that no other course could be adopted, and the younger Maraldi in France, giving up the prejudices which the Cassini family (to which the Maraldis belonged) had adopted against Römer's theory, introduced the equation of light into the analysis of his own singularly beautiful series of observations of Jupiter's satellite system.

(1309). A full consideration of the phenomena actually presented by the satellites of Jupiter, and the discussion of the movements of these bodies, would fill a volume. Here we must be content to notice simply the salient details.

Fig. 396 presents the characteristic relations of the system of satellites, JJ′ being the planet's globe, and A*k*A′, B*l*B′, C*m*C′, D*n*D′ the orbits of the satellites in the order of their distance from Jupiter. J*nn*′J′ is the shadow of Jupiter, the lines JS and J′S′ being supposed to be directed towards the sun. It will be noticed that the shadow is not run to a point as usual in pictures of the system. The fact is that the shadow is about 52,000,000 miles in length, so that its narrowing at *nn*′, little more than a million miles from Jupiter, would not be perceptible on the scale of fig. 396. When Jupiter is in opposition, the lines of sight from the Earth to the several satellites are parallel to the lines S*n*, S′*n*′; the satellites at this time are only seen in transit across the planet's face when traversing the part of their orbits between these two lines on the nearer halves of their orbits; and they are behind the planet, and in its shadow, when traversing the part of their orbits between these same two lines on the farther side of the planet. But when Jupiter is not in opposition these relations do not hold. The maximum divergence from them occurs when Jupiter is in quadrature. At this time, as we learn from fig. 374 (Art. 1268), lines of sight from the Earth to the planet have the direction Q*q*, at quadrature preceding opposition, and Q′*q*′, at quadrature following opposition. At the former epoch the satellites can only be seen in transit when traversing the part of their orbits between the lines Q*q*, q*r* (fig. 396) on the nearer parts of

their orbits ; while, at the latter epoch, the satellites can only be seen in transit when traversing the part of their orbits between the lines Q′q′, q′r′. But at all times the satellites can only be within the shadow of Jupiter when on the parts of their orbits between Jn and J′n′, and their shadows can only fall upon the planet when they are on the parts of their orbits between the lines SJ and S′J′.

(1310.) We see, then, that whereas, when Jupiter is in opposition, the satellites appear to travel to and fro, in such paths as AA′, BB′, CC′, and DD′, passing across the planet's face when on the nearer half of their course, and hiding behind the planet when on the farther half. At other times, though the apparent

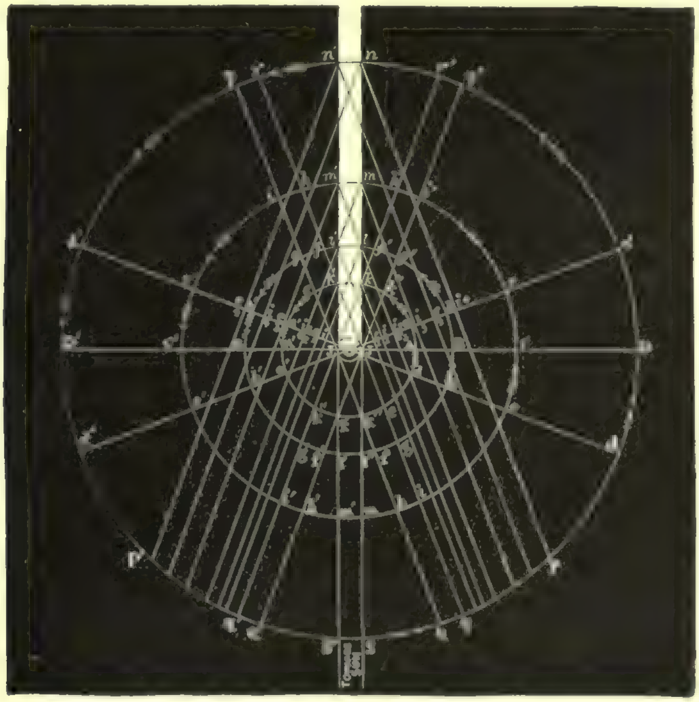


FIG. 396.—Illustrating (accurately to scale) the phenomena of Jupiter's satellites.

paths of the satellites remain the same, the satellites disappear in the planet's shadow, at times not coinciding with the passage behind the planet's disc, and their shadows transit the planet's face, at times not coinciding with the satellites' own transit across that disc. Taking the maximum difference (about 11° 5′) between the direction of the line of sight from the Earth to Jupiter and the line from the sun to the planet, we have the relations in the next two articles.

(1311.) First, at quadrature before opposition :—

(*a*) Satellite I. appears to traverse the range *aa*′ (fig. 396) to and fro ; casts its shadow on the planet's face when traversing the arc k′k″ (seen as

J′J), but appears itself to transit that face while traversing the arc k″e; passes into the shadow when at k (seen as at i_1), not reappearing from eclipse at all, but reappearing from transit when at e (seen, of course, at J′), then passing to a' and back across the planet's face to e, &c., as before.

(b) Satellite II. appears to traverse the range $b\,b'$ to and fro; casts its shadow on the planet's face when traversing the arc l′l (seen as J′J), but appears itself to transit that face while traversing the arc f g; passes into the shadow when at l (seen as at i_2), emerges from the shadow when at l' (seen as at e_1), passes behind the planet along the arc $f\,g$ (seen, of course, as J J′), then passing to b' and back across the planet's face to b, &c., as before.

(c) Satellite III. appears to traverse the range $c\,c'$ to and fro; casts its shadow on the planet's face when traversing the arc m′ m (seen as J′J), but appears itself to transit that face while traversing the arc h i; passes into the shadow when at m (seen as at i_3), emerges from the shadow when at m' (seen as at e_3), passes behind the planet along the arc $h\,i$ (seen as J J′), then passing to c' and back across the planet's face to c, &c., as before.

(d) Satellite IV. appears to traverse the range $d\,d'$ to and fro; casts its shadow on the planet's face when traversing the arc S′S (seen as J′J), but appears itself to transit that face while traversing the arc Qq; passes into the shadow when at n (seen as at i_4), emerges from the shadow when at n' (seen as at e_4), passes behind the planet along the arc $r\,q$ (seen as J J′), then passing to d' and back across the planet's face to d, &c., as before.

(1312.) Secondly, at quadrature after opposition:—

(e) Satellite I. appears to traverse the range a a′ to and fro, transiting the planet's face when on the arc k″k (seen as J′J), and casting its shadow on the planet when on the arc kk″, passing to a, then back, passing behind the planet when on the arc $k''k$ (seen as J J′), emerging from the planet's shadow when at k' (seen as e_1), and so to a′ and back across the planet's face to a, &c., as before.

(f, g, and h) Satellites II., III., and IV. move in corresponding ways, the transits of the satellites themselves across the planet's face preceding the transits of their shadows, and the satellites being occulted behind the planet before they pass into the shadow. The order of events is that indicated in what has just been stated with regard to Satellite I., while the events themselves correspond, except as to this inverse order of their occurrence, with those described in paragraphs b, c, and d above respectively, the only change being as to the lettering; but as corresponding letters (small roman for italic, or accented for unaccented) are used, the student will find no difficulty in following the verbal description. The diagram itself, however, explains all the relations involved, and can hardly fail to be understood, especially after

paragraphs *a*, *b*, *c*, *d* above, describing precisely parallel motions, have been carefully read.

N.B.—The motions are all described as they really take place. It will be remembered that the astronomical telescope inverts, so that the appearances and disappearances described as taking place on one side of Jupiter appear in the telescope to take place on the other, and *vice versa*.

(1313.) The student will have no difficulty in dealing with cases arising when Jupiter is in any position intermediate with the extreme positions considered above. From such a diagram as fig. 396, carefully constructed to scale, he can determine by construction the angle between lines from the Earth and Sun to Jupiter when the two planets are in any known positions on their respective orbits. Then, drawing from the points $n\,n'$, $m\,m'$, $l\,l'$, $k\,k'$, J J', a series of parallels inclined at this angle to the parallels S n, S' n', and athwart those parallels a line at right angles to them through O, he will find all the positions at which the occultations and eclipses of the several satellites begin and end.

(1314.) We have thus far proceeded on the supposition that the shadow of Jupiter is in the same plane as the orbits of the satellites. In reality the satellites do not all travel in the same plane, and the axis of Jupiter's shadow is only in the plane of motion of any satellite when Jupiter is at two opposite points on his orbit. If the satellites' orbits were greatly inclined to each other or to Jupiter's orbit, these considerations would be important, even in so merely general an explanation as I am here giving. (In an exact discussion of the phenomena presented by the satellites they are, of course, of great importance, and all the more difficult to deal with that the inclinations involved are slight.) But as the inclination of the plane of Jupiter's equator to the plane of his orbit is about only 3° 5', and the planes of the orbits of his satellites are all nearly coincident with the plane of his equator, we have, in the first place, a very slight range of inclination of Jupiter's shadow to the equator plane of Jupiter to deal with, and very slight divergences of the satellite planes from that plane to take into account.

(1315.) The character of the orbital motions of Jupiter's satellites may, sufficiently for our purpose here, be presented as follows: Each satellite moves on an orbit inclined at a constant small angle to a fixed plane, the orbit gyrating on this plane somewhat as the plane of the Earth's orbit gyrates on the ecliptic, but in a much shorter period. The fixed plane for each satellite passes through the line of intersection of the plane of Jupiter's orbit with the plane of his equator, lying between these planes and much nearer to the latter. The fixed plane of Satellite I. is inclined only 6″ to the plane of Jupiter's equator; that of II. only 1′ 5″; that of III. 5′ 2″; and

that of IV. 24′ 4″ : so that the *mean* inclinations of the planes of the satellites' orbits to the plane of Jupiter's orbit are—for I., about 3° 5′ ; for II., about 3° 4′ ; for III., about 3° ; and for IV., about 2° 41′. The mean inclination of IV. to the plane of Jupiter's orbit is, therefore, less than that of any of the other satellites.[1]

(1316.) The mean position of all the satellites' orbits is such that, as seen from the sun, they would appear as lines when Jupiter is in about longitude 132° to 133° on one side of his orbit, and in about longitude 312° to 313° on the other, while they would appear with their greatest opening (in all cases slight, and least for the outermost satellite) when Jupiter is in about longitudes 42° to 43° and 222° to 223°. The shadow of Jupiter, which runs to a distance of about 50 millions of miles in the plane of his orbit, would run out to this distance in a long isosceles triangle, having a vertical angle of about 6′ on the plane of the orbit of any satellite when in one of the two former positions, but to a much diminished distance, and with a long elliptical outline on the plane of any satellite, when in one of the two latter positions—that is, the geometrical intersections of the shadow surface and these planes would have these figures. The distances to which the shadow of Jupiter reaches on the orbits of the four satellites, when at their mean inclinations and greatest openings, are about as follows :[2] for I., 744,970 miles ; for II., 747,850 miles ; for III., 769,510 miles ; and for IV., 852,900 miles. Comparing these distances with the distances of the respective satellites from Jupiter, we see that I., II., and III. can never escape eclipse, but that IV. escapes eclipse during considerable portions of the Jovian year—in fact, during the middle third of each half-year from equinox to equinox, or in all one-third of each year. The same general results occur with respect to transits of the satellites'

[1] In a moment of forgetfulness Sir John Herschel attributed the circumstance that the fourth satellite sometimes escapes eclipse and occultation in the farther part of its orbit, and passes, as does its shadow, clear of Jupiter's disc in the nearer part, to the greater inclination of this satellite's orbit plane to Jupiter's, overlooking the fact that the satellite's orbit has a greater mean inclination from the equator plane of Jupiter, and towards the planet's orbit plane, to which, therefore, its mean inclination is less. It is the greater distance of this satellite from Jupiter which causes it to escape eclipse occultation and transit when Jupiter is in those two opposite parts of his orbit where the orbit of the satellite, as supposed to be seen from the sun, appears most open.

[2] We must take Jupiter's polar radius, since the shadow of Jupiter's polar regions forms the part of the elliptical shadow near the extremity of its major axis; and remember that the inclination of the shadow's edge to the line from the sun's centre to Jupiter's is about 3′, by which angle we must increase the inclination of each satellite's orbit to Jupiter's to obtain the inclination of the satellite's respective orbits to the bounding line of the shadow which defines the extremity of the shadow. These angles, then, are about 3° 8′, 3° 7′, 3° 3′, and 2° 44′ for I., II., III., and IV. respectively, and the respective lengths of shadow in miles are—for I., 40,720 cot. 3° 8′ = 744,970; for II., 40,720 cot. 3° 7′ = 747,850; for III., 40,720 cot. 3° 3′ = 769,510; and for IV., 40,720 cot. 2° 44′ = 852,900. These results, though quite rough, even regarded as mean values, suffice for our purpose here. The distances of the satellites given in (Art. 1277) have been used. Laplace gives the distances of the satellites in equatorial radii of Jupiter as I. 5·6984, II. 9·06654, III. 14·46189, IV. 25·4359, which correspond to distances of 246,550, 392,200, 625,500, 1,100,300 miles respectively.

shadows. The shadows of I., II., and III. transit the planet's face in each circuit; the shadow of IV. passes clear of Jupiter during about one-third of all the revolutions of the satellite. With regard to occultations and transits of the satellites themselves, the case is somewhat complicated by the fact that observations are made from the Earth, which does not move in the same plane as Jupiter. The average proportion of cases in which Satellite IV. escapes occultation in transit is appreciably the same as the proportion of cases in which it escapes eclipse and its shadow escapes transit. But the sequence is not quite so regular in regard to occultations and transits, owing to the way in which the Earth passes each year alternately to the north and to the south of Jupiter's orbit plane.

(1317.) A few points must here be noticed in regard to the motions of Jupiter's satellites before proceeding to consider those phenomena of transits, occultations, eclipses, and shadows which throw light on Jupiter's physical condition; but the reader will understand that what is here stated is but a sketch, the subject in its fulness requiring long explanation and closer study of details than I could expect from any of my readers except those who are sure to study the matter where alone it can be properly dealt with, viz., in treatises relating specially to the subject.

Soon after his discovery of Jupiter's satellites, Galileo perceived the use to which the phenomena they presented might be applied, namely, for the determination of the longitude. He was sanguine indeed as to the use of this method for finding the longitude at sea, not being aware, it would seem, of the mechanical difficulties which render the method unavailable on shipboard. With the object of constructing tables of the satellites' motions, he observed them for many years. The tables he formed disappeared unaccountably on the death of his pupil Rimieri, to whom he had entrusted them for publication, and were accidentally discovered a few years ago in a private library at Rome. Notwithstanding the amount of labour bestowed upon them, the tables are far from representing with accuracy the motions of the satellites. Galileo, indeed, and those who followed him in attempting the work of tabulating these motions, altogether underrated the difficulty of the task. A long series of observations by Borelli, Cassini, Maraldi, Bradley, and a host of other observers; the rigid theoretical scrutiny of the subject by Newton, Walmsley, Euler, Bailly, Lagrange, Laplace, and others; and a laborious comparison of the results of observation and theory, by Lalande, Wargentin, Delambre, and Woolhouse, were required to bring the theory of the system to exactness and accuracy.

(1318.) The relations actually presented by the motions of the satellites are somewhat singular. They are partly exhibited by the following table :—

Sat.	Sidereal revolution	Same in seconds	Sidereal motion per second
	d. h. m. s.		″
1	1 18 27 33·505	152853·505	8·478706
2	3 13 13 42·040	306822·040	4·223947
3	7 3 42 33·360	618153·360	2·096567
4	16 16 32 11·271	1441931·271	0·898795

(1319.) It will be observed, at once, that the period of the second satellite is almost exactly double the period of the first, and the period of the third almost exactly double that of the second; and, of course, a corresponding relation holds amongst the sidereal motions of these bodies. This of itself is remarkable, but far more singular is the relation which regulates the extent to which the above relations differ from exactness. It is to exhibit this that I have added the column of sidereal motions, because the relation in question is masked when the sidereal periods only are given. It will be found that the sidereal motion of the first satellite, together with twice the sidereal motion of the third, is *exactly* equal to three times the sidereal motion of the second satellite. Thus:—

$$(8''{\cdot}478706)+2(2''{\cdot}096567)=12''{\cdot}671840=3(4''{\cdot}223947).$$

(1320.) To show the effect of this singular relation, suppose the first and third satellites to start from conjunction; then, after four revolutions of the first satellite, the third has performed nearly one revolution, so that they are very nearly in conjunction again, but have in reality passed their conjunction by a small angle. At the actual moment of conjunction, the first has described three complete circumferences and an arc (A, suppose), which is nearly a complete circumference, while the third has described the arc A only; thus *twice* the arc described by the third satellite added to the arc described by the first gives three complete circumferences, and three times the arc A; and by the above relation the second satellite has moved through one complete circumference together with the arc A. Hence, neglecting complete circumferences, the actual change of position of *each* of the three satellites is the arc A, very nearly equal to a complete circumference. They therefore hold the same relative position at the end as at the beginning of the interval considered. And at this time—that is, when the first and third satellites are in conjunction—the second satellite (about whose position nothing was said) is always in opposition to both.

(1321.) Wargentin, who devoted a life to the examination of the motions of Jupiter's satellites, but who was no adept in the higher branches of mathematics, found, as the result of *observation*, that the relation above described was so closely approximated to, that 1,317,900 years would have to elapse before the three satellites could be in conjunction. This result affords an interesting

measure of the accuracy of observation up to Wargentin's day, since Laplace has shown that the relation is absolutely exact. Librations *may* take place on either side of the mean state (though the most careful modern observations exhibit no trace of such libration), but there is no possibility of accumulative change, save by the influence of effective agencies external to the system. It is somewhat singular that the comet of 1767 passed through the middle of Jupiter's system without producing any observable derangement of the mean motions of the satellites—a fact which proves conclusively that the mass of the comet must be small, its density inconceivably minute.

(1322.) It has been stated that the motion of the fourth satellite presents no approach to a relation of commensurability with those of the others. A simple relation exists, however, with a closeness of approximation which is quite remarkable. In fact, throughout the whole solar system there is no relation of commensurability which brings closely following conjunction-lines nearer to each other than this does.[1] The relation is this :—three times the period of the fourth satellite is 50 d. 1 h. 36 m. 33·813 s., and seven times the period of the third is 50 d. 1 h. 57 m. 53·520 s. ; the difference, 21 m. 19·707 s., is less than $\frac{1}{1123}$ part of the period of the fourth satellite. Thus, when the third satellite has travelled round seven times from a given conjunction-line with the fourth, the fourth has gone round three times and in addition $\frac{1}{1123}$ part of a circumference, that is less than 20′, and the third overtakes the fourth before the latter has passed over 15′ more (since 15 : 35 :: 3 : 7). This conjunction-line, then, is separated from a preceding one (the fourth preceding) by less than 35′.[2]

From the connection between the motions of the first three satellites, it follows, of course, that the periods of the two inner satellites also approximate to commensurability with the period of the fourth. We have, in fact, fourteen revolutions of the second, or twenty-eight revolutions of the first, nearly equal to three revolutions of the fourth ; but the approach is not so close as in the case of the third satellite.

(1323.) From the relation holding between the motions of the first three satellites, it is impossible that all these bodies should be eclipsed at once ; but it will be readily seen that at regular intervals all three are in the same straight line with the planet's centre. If this happen when the sun (and therefore the Earth, which, with reference to Jupiter, may always be considered to be close to the sun) is near the same line, these three satellites will be invisible, one or two being eclipsed, two or one (as the case may be) being

[1] The periods of the satellites Dione and Enceladus produce about as close an approach as that of the third and fourth satellites of Jupiter.

[2] The remarkable relation which causes the 'Great Inequality' of Saturn and Jupiter brings neighbouring conjunction-lines nearly 8½° apart, a distance fourteen times as great as the above.

projected on Jupiter's disc. Such a phenomenon is not unfrequently observable. That the fourth satellite may be hidden at the same time it must be nearly in a line with the other three. This relation is not often presented; and, as already stated, the concurrence of this relation with the requisite configuration, as respects the sun and Earth, is an occurrence very seldom to be observed. It can, of course, only take place when Jupiter is on those parts of his orbit (about two-thirds in all) where the fourth satellite does not escape eclipse or occultation at each conjunction with Jupiter.[1]

(1324.) In fig. 379 (Art. 1278) the theoretical dimensions and character of their shadows are compared with the relative dimensions of the four satellites themselves. The shadows have not, indeed, been *seen* as here shown, no telescope yet constructed having sufficed, it would seem, to exhibit the penumbral band surrounding the true shadow as a distinct feature. Yet it must not be supposed, because the shadows are not seen as here shown, that any reasonable doubt can exist as to their true character.[2]

[1] It is commonly stated that the third satellite cannot possibly escape eclipse or occultation as it passes behind its primary, and must necessarily transit Jupiter's disc when passing before the planet. Though the third satellite can never escape eclipse, it is just possible for it to pass clear of Jupiter's disc so as to escape occultation or transit. A conjunction of many favourable circumstances is, however, required, and the phenomenon must be a very uncommon one—much more so, indeed, than the disappearance of all four satellites. It is necessary that Jupiter should be in opposition when not far from perihelion, at which time it happens (and but for this the phenomenon could never take place) that the Earth is at nearly her greatest distance north of the plane of Jupiter's orbit. The satellite's orbit must have its maximum inclination to Jupiter's orbit, and the satellite must also be at its greatest distance from the last-named plane. The other satellites must also be so situated that the third is at its maximum distance from Jupiter; for it is noteworthy that, although the orbits of the two interior satellites are described as circular, and that of the third as of small eccentricity, yet these orbits have an *ellipticity* due to the mutual attractions of the satellites. This ellipticity is wholly different from the ellipticity of the planetary orbits. The former is centric, the latter eccentric, the sun being in the focus of each planetary eclipse, while Jupiter is at the centre of the ellipse traversed by the inner satellites.

[2] The determination of the extent of the penumbra surrounding the true shadow is a matter of very simple calculation. Thus, in the case of the fourth satellite, we have, first, the distance of the satellite from Jupiter's surface equals 1,160,000 miles (in round numbers). Now the sun, as seen from Jupiter, or from any part of his system, subtends an angle of about 6′; and a moment's consideration will show that the width of the penumbral band is determined by supposing two lines to be drawn from a point on the satellite's limb to the planet (and therefore each 1,160,000 miles long), inclined to each other at an angle of 6′, the chord joining the ends of these equal lines gives the required width (technically, the width is the chord of 6′ to radius 1,160,000 miles). It follows that this chord is about 2,025 miles long. This, then, is the width of the penumbra. If the sun were a point, the shadow would be about as large in appearance as the satellite itself, that is, would be a circle about 3,170 miles in diameter. Half the penumbral band lies within, half without such a circle. Hence, the diameter of the true shadow is only 1,150 miles, and the penumbral band around this being 2,030 miles wide, the diameter of the space covered by umbra and penumbra is 5,210 miles. In this the dimensions presented in the figure have been determined. It is obvious, however, that though the actual extent of the shadows and penumbras must be such as is here shown, yet no telescope could possibly reveal the full extent of the shadow and penumbra since the outer limit of each shadow passes insensibly into the whole, and the outer limit of each penumbra passes insensibly into the general illumination of Jupiter's surface. Different telescopes will also present the apparent dimensions of the shadow differently. It would, of course, be absolutely impossible to compare the efficiency of

(1325.) Note here, in passing, that a difference corresponding to that here discussed respecting the satellites' shadows exists in the nature of the shadow of Jupiter at the parts of the cone entered by the several satellites. Thus the disappearance of IV. takes a much longer time than that of I., not *merely* on account of the slow motion of IV. (as is commonly stated), but on account also of the much wider fringing of the penumbra in the cross-section of the cone of shadow where IV. enters it, as compared with the fringe in the cross-section where I. enters.

(1326.) The significance of the various phenomena presented by the satellites cannot be rightly understood without considering the probable condition of each as to surface brightness, possible changes of brightness, rotation, and so forth, and also taking into account the nature of the shadow thrown by each upon Jupiter.

(1327.) As to the former points, the evidence is by no means so satisfactory as could be wished. We have a superabundance of evidence all unsorted and unsifted by observers differing greatly in skill and experience, and using telescopes of all degrees of power; so that the evidence is in a large degree self-contradictory and bewildering.

It seemed clear to the earlier observers, to the time of Sir William Herschel and Schröter, that all Jupiter's moons keep the same face directed towards their primary, each rotating, like our own moon, once in each mean sidereal revolution. But the evidence on which this conclusion was based, though strong in the case of the two inner satellites, which seem to vary in brightness similarly in each part of their apparent orbit, is doubtful in the case of III., which, being brightest at both elongations, might possibly be regarded as rotating *twice* in each revolution, and fails wholly in the case of the fourth satellite, which varies in lustre in ways quite inconsistent with the theory of a single rotation to each revolution. This was noticed before the reason of the synchronism between our moon's rotation and revolution had been recognised. Extending to Jupiter's system the considerations which have enabled us to understand the law of our moon's rotations, we perceive that while the comparatively short periods of revolution of the Jovian moons suggest that

one telescope directly with that of another in this respect; because no amount of practice in the use of the micrometer would enable an observer to estimate the apparent width of the shadow with the requisite exactness. But it is not difficult to compare the work of two telescopes in revealing the extent of Jupiter's shadows indirectly. For it is very obvious that a telescope which does not reveal faint shadows well would exhibit the shadow of the fourth satellite as *perceptibly smaller* than that of the third; whereas a telescope more efficient in this particular respect would show the shadow of the fourth satellite *larger* than that of the third. Or, even if only the fourth satellite and its shadow were transiting the disc, it is obvious that the former telescope would make the shadow seem smaller than the satellite, while the latter would make the shadow larger than the satellite.

they may all probably have had their periods of rotation and revolution coerced into coincidence, the two inner moons of Jupiter are more likely than the third and fourth, whose distance and period are much greater, to have undergone such a change, while the outermost moon might be expected to have had much the best chance of retaining a rotation-period independent of its period of revolution. But if the age of Jupiter's satellites corresponds to that of our moon, the retarding tidal action caused by a planet whose mass is 315 times that of the Earth must (even in the case of the fourth satellite) have brought about a synchronism between the rotation and revolution periods long ere this.

(1328.) There are reasons for believing that the moons of Jupiter are not (all of them, at any rate) in the same condition as our moon. Changes of lustre occur such as cannot be wholly explained by the rotation of these bodies on their axes, either like our own moon once in each circuit around their primary, or like the Earth in her course round the sun, with an independent rotation-period. The researches of Auwers and Engelmann render this probable. The most natural explanation is that clouds gather from time to time in the satellite's atmosphere, or other changes take place which cause more light to be reflected than from the rock-surface of the satellite, so that, when the skies of a satellite are cloudy, the satellite looks bright to us; and when the clouds clear away, the satellite looks dark. But, of course, this explanation, though reasonable and in satisfactory accordance with analogy, is not based on demonstrative evidence; and it may be that Auwers and Engelmann have mistaken subjective for objective changes, or even that a state of things exists in the satellite-system of Jupiter unlike anything known to us either on the Earth or on the moon.[1]

(1329.) Satellite III. is usually the brightest of the satellites as seen off the planet's disc, which we should expect from his superior size (fig. 379); but IV., which comes next in size, is nearly always the faintest. Satellites

[1] It has been customary to dwell on the beautiful aspect which these four moons must present as seen from Jupiter, and on the quantity of light they must pour upon his surface during his short nights. We know now that the moons are not probably observed at all from Jupiter, unless the planet possesses inhabitants able to live amidst fire, and to see through cloud-masses hundreds of miles thick. But if the ideas which were entertained by Sir David Brewster and Dr. Whewell (in his Bridgewater Treatise. He advocated very different views in his 'Plurality of Worlds') had been sound, so far as these points were concerned, it might yet have occurred to them to inquire in what degree the four moons of Jupiter could make up to the supposed inhabitants of the planet for the small amount of light received from the sun. Had they sought to determine in what degree the light of Jupiter surpasses that of full moonlight, the calculation (neither long nor difficult) required would have shown them that, assuming the four moons have surfaces of the same reflective capacity as our moon, and that they are all full together (which, however, can never happen), their combined light, illuminated as they are by the same distant sun which shines on their primary, would not exceed, would not even equal, but would amount to but only about one-sixteenth part of full moonlight!

I. and II. appear usually when off Jupiter's disc of about equal brightness, but II. is slightly the brighter on the average. These relations vary, however. Sometimes IV. has appeared brighter even than III., sometimes singularly dark. As regards intrinsic lustre, also variable for each satellite, II. comes first, then I., then III., and IV. last of all.[1] This quality can only be satisfactorily determined by observations of the satellites when off the disc of Jupiter; for, when a transit is on that disc, its apparent relative lustre must depend entirely on the intrinsic lustre of the particular part of the disc on which it happens to be. We know that the eye cannot readily appreciate the differences of lustre in different parts of Jupiter's disc,[2] whereas the passage of a satellite athwart such different parts serves as a most delicate means of determining their relative luminosity. So that, when we find that Satellites I. and III. vary in appearance when transiting the disc of Jupiter, I. being sometimes light grey, at others dark grey, and III. having even been seen white in one transit and almost black in the next, we must consider such changes as more probably (though, of course, not necessarily) due to differences or changes in the luminosity of the zone of Jupiter traversed by the satellites, than to differences or changes in the satellite itself.

(1330.) In passing over the planet's disc, the first and third satellites usually appear bright when near the edge, dark when in the mid-portion of their transit. The fourth generally grows rapidly fainter in appearance as it approaches the limb, shines with a faint lustre for ten or fifteen minutes when on the limb, is then lost to view for about the same interval, after which it appears as a dark spot, growing darker and darker until it is as dark as its own shadow. The second satellite is never seen otherwise than as a white spot on any part of the planet's disc.

(1331.) That the satellites do actually change in aspect must, however, be admitted, for in good telescopes (used by a keen observer) markings can be recognised on them both when on and off the disc, and in some cases these markings vary perceptibly. Thus the first two drawings of fig. 397 show III., as seen by Dawes, both when the satellite was on and off Jupiter's disc; while in fig. 398 we have four drawings of the satellite, by Secchi, presenting

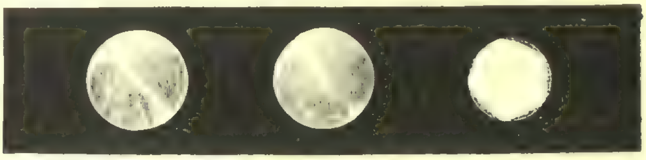
[1] From a series of observations specially directed to the determination of the relative brightness of the several satellites, and their respective degrees of whiteness, Mr. E. J. Spitta deduces the following average results:

	Magnitudes below Jupiter	Albedo (Jupiter's 0·624)
I.	8·12	0·656
II.	8·40	0·715
III.	7·48	0·405
IV.	8·73	0·266

[2] The eye is so readily deceived that Chacornac, though an experienced observer, wrote a dissertation explaining why the part of Jupiter's disc near the edge is brighter than the central parts, the real fact being (as proved by photometric measurements) that the part near the edge is darker than the rest, its apparent brightness being an optical illusion due to contrast with the dark background of the sky.

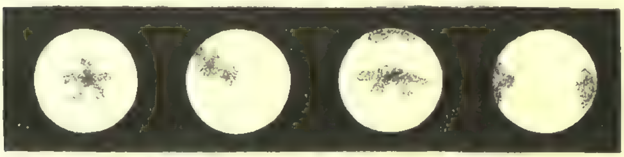
entirely different aspects. (The satellite appeared somewhat ruddy to Secchi when thus drawn.) The polygonal aspect of IV., as shown in the third drawing of fig. 397, has been noticed by other observers, but it is not always, or indeed usually, presented.

(1332.) The satellites, as we have seen, confirm by their behaviour in transit the conclusion, to which we had already been led, that the exterior portion of the disc of Jupiter is much less luminous than the parts near the middle. I wish now to consider specially the light which they throw on the question raised in Art. 1286: Is the darkening at the edge due wholly (it must be partially due) to the smaller angle at which the light rays are inclined to the surface near the edge, or partly to a certain degree of transparency



III. January 24 and 31, 1860. III. February 11, 1849. IV. On various occasions.

Fig. 397.—Drawings of Jupiter's third and fourth satellite by Dawes.



August 26, 19 h. 20 m. August 26, 21 h. 10 m. Au ust 27. September 9.
Satellite III. in 1855.

Fig. 398.—Drawings of Jupiter's third and fourth satellites by Secchi.

in the outer parts of what has been measured as if it were the actual globe of Jupiter?

(1333.) In four different ways the satellites have given evidence as to the great depth of the atmospheric surroundings of Jupiter, and I would invite the special attention of the student to the fact that the evidence thus given is cumulative. If this or that line of evidence seems less decisive than another, or if this or that observation on any given line appears doubtful, the general evidence retains its value, even though such evidence, instead of being regarded as doubtful, is rejected altogether.[1]

[1] This consideration in problems of the kind is overlooked, the rejection of some special observation in a long array all independently pointing in one direction being illogically regarded as necessitating the rejection of the conclusion which that observation had been supposed to support. I have noted cases of the kind, particularly in the reasoning of specialists, as preposterous—considered logi-

First, in approaching the edge of the disc of Jupiter, both before transit and before occultation, satellites have been observed to behave in such a way as to show that the light of the edge fluctuates in brightness and distinctness, if not actually in visibility. A satellite has been seen to hang on the limb, and then to recede. The second satellite was observed by Mr. Gorton in 1863 to disappear and reappear several times before occultation. On the same occasion Mr. Wray saw it projected within the limb for twenty seconds.[1] Gambart states that I. on October 19, 1823, entering on the planet's face before transit, disappeared (by passing within the limb) and reappeared several times. Secchi and Main on different occasions saw the planet's edge alternately approach and recede from a satellite several times during five minutes—a phenomenon which Mr. Webb considered obviously due to unsteady air. But, considering the cases of appearance and disappearance indicating fluctuation of the visible limb, this seems as likely an explanation as the effects of unsteady air, which an observer of any skill can nearly always recognise as such. Mr. Grover on one occasion saw the advance of II. towards occultation arrested for a full minute, corresponding to more than 500 miles of thwart motion, when the satellite was but three or four of its own diameters from the limb. 'Definition splendid,' says Mr. Webb, 'and observation very clear.' But the most remarkable, and fortunately the best attested, case of the kind we are considering is that noted on June 26, 1828, by Admiral

cally—as though, because some particular effect observed by Newton and attributed by him to gravity had been shown to be due in reality to magnetism, physicists should have been told that the theory of gravity must be rejected in company with that particular observation.

[1] To appreciate fully the meaning of the phenomena described in this and following paragraphs, it must be remembered that at Jupiter's opposition distance the diameter of the smallest of the satellites subtends an angle of about 1″, the diameter of III. being about 1⅔″. Their rates of motion as seen on the middle of the disc, where there is no foreshortening, and at the edge of the disc, where the foreshortening even in the case of I. is slight, are as follows:—

	Velocity in orbit in miles (which is also velocity at mid-transit)			Thwart velocity where crossing edge of Jupiter's disc		
	Per sec.	Per min.	Per hour	Per sec.	Per min.	Per hour
I. . .	10·751	645·06	38703·60	10·603	636·18	38170·80
II. . .	8·528	511·71	30702·31	8·487	508·93	30535·92
III. . .	6·759	405·52	24331·32	6·744	404·64	24279·84
IV. . .	5·063	303·80	18228·24	5·060	303·59	18215·64
Jupiter's Equator						
Assumed period of Rotation, 9 h. 50 m. .	7·667	460·7	27642	0	0	0
Assumed period, 9 h. 55 m.	7·55	453·0	27180	0	0	0

From the numbers on the lowest line, it will be seen that only the two innermost satellites, as seen in mid-transit, gain on the markings of Jupiter's surface.

The rates of passage of the shadows across the planet's disc and the rate of passage of the satellites through the planet's shadow may be taken as equal to the velocities indicated—per second, hour, and minute—in the first three columns.

Smyth at Bedford, by Sir Thomas (then Mr.) Maclear at Biggleswade, and by Dr. Pearson at South Kilworth, stations respectively twelve miles and thirty-five miles from Admiral Smyth's. On this occasion Satellite II., seen first as shown in fig. 399. Admiral Smyth's account states that the satellite gradually made its entry, remaining 'for some minutes on the edge of the limb' [it will be observed that more than four minutes are required for the entrance of the satellite from first to last contact], 'presenting an appearance not unlike that of the lunar mountains coming into view during the moon's first quarter, until it finally disappeared on the body of the planet' (fig. 400). 'At least twelve or thirteen minutes must have elapsed,' he proceeds, 'when, accidentally turning to Jupiter again, to my astonishment I perceived the same satellite *outside the disc.* It was in the same position' (fig. 401) 'as to being above a line with the apparent lower belt, where it remained distinctly visible for at least four minutes, and then suddenly vanished.' Figs. 399, 400, and 401 are from Smyth's own drawing. His observations were made with an excellent but small telescope $3\frac{1}{2}$ inches in aperture. Mr. Maclear wrote, a few days later, to say that he had observed the same phenomena with a $3\frac{1}{3}$-inch telescope, but had regarded them as illusions; and 'about the same time,' says Smyth, 'Dr. Pearson, having favoured me with a visit, asked me whether I had noticed anything remarkable on the 26th, for that he had, in accidentally looking at Jupiter, *seen the second satellite reappear.*' (Dr. Pearson's telescope was $6\frac{4}{5}$ inches in aperture.) The only point which can be regarded as doubtful about this observation is the duration of the interval between the disappearance and the reappearance of the satellite. Possibly the twelve or thirteen minutes may be reduced to three or four; but it is certain that the satellite, after disappearing, or rather after entering fully on the planet's disc, reappeared, remained visible for four minutes, and then suddenly entered again within the boundary of Jupiter's disc, each minute representing 500 miles of thwart motion of the satellite, while the satellite's own diameter adds more than 2,000 miles to the change of apparent outline for which we have to account. Let it be noticed that there is no other available explanation, unless we reject the observation altogether, which, since it was made by three experienced observers, we cannot reasonably do. The satellite certainly did not stop, recede, and then suddenly rush forward five or six thousand miles. Jupiter certainly did not shift bodily to and fro. Since the satellite was on this side

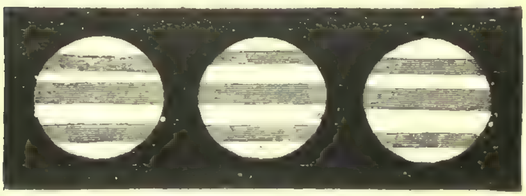


FIG. 399. FIG. 400. FIG. 401.
Remarkable phenomena preceding the transit of Satellite II. (*Smyth, Maclear*, and *Pearson*).

of the planet, refraction in Jupiter's atmosphere had nothing to do with what was observed. It is as certain as observation can make anything that the outline of Jupiter changed, *so far as visibility was concerned*, while these seemingly contradictory observations were in progress.[1] The change may seem remarkable; but any other way of explaining the observed event would involve conceptions altogether more remarkable—in fact, wholly inadmissible. The occurrence of a very slight change of condition throughout the portion of Jupiter's atmosphere where the observed change took place, as some slight increase of heat, would account for a change from appreciably perfect to recognisably imperfect transparency—that is, from invisibility to visibility—for the range of atmosphere through which the line of vision passed was many hundreds of miles long at the depth to which such a change must be supposed to have extended. The fact that the part of the white belt which showed the change was just passing into sunlight; and so, being subjected to an increase of heat, might of itself suffice to explain the change in relative transparency, which caused a tract several thousand miles deep to become so transparent as to be invisible, having a few minutes before been visible.

(1334.) *Secondly*, we have clear evidence that in certain cases, doubtless under exceptional conditions, the satellites (especially the brighter ones) have been visible through the atmosphere and partially cloud-laden atmosphere which forms the apparent outline of the planet. We have seen already that Mr. Wray, on an occasion when other observations indicated the tenuity and the fluctuating condition of the atmosphere at the part of the planet approached by Satellite II. (the brightest of all), saw that planet projected within the limb for nearly twenty seconds. On January 3 Mr. Hodgson saw I. projected on the disc for nearly one minute with (seemingly) clear space around it; doubtless an effect of contrast making the part of the planet's edge adjacent (optically) to the satellite appear darker than the rest. Mr. Carlisle,

[1] Mr. Webb makes the following remarks respecting these observations. He says they have 'the highest attestation; the authority of such an observer as Smyth would alone have established the wonderful fact' (a statement which, though we must accept the wonderful fact, we are constrained, after Mr. Burnham's study of Smyth's observations, to reject), 'but it was recorded by two other very competent witnesses:—Explanation is here set at defiance; demonstrably neither in the atmosphere of the Earth nor Jupiter. Where and what could have been the cause? At present we can get no answer.' I find only one attempt to get round the evidence, viz. (see Chambers' 'Astronomy,' 3rd edition, p. 124), by supposing that Satellite II. was eclipsed either by Satellite III. or by Satellite IV. Suggestions such as these are ingenious enough as suggestions; but, advanced as explanations after there has been ample time for investigation, they belong to the class of ideas of which it may be said that 'they darken counsel by words without knowledge.' It would have been so easy to put the matter to the test. Neither III. nor IV. was anywhere near where it could have eclipsed Satellite II., either at the time of observation or for *three years before or after*. At the opposition of Jupiter in 1828 (near the end of April) the orbits of the satellites, as supposed to be seen from the sun, had nearly their greatest apparent opening—a condition rendering eclipse of satellite by satellite impossible.

at Stonyhurst, saw II. for about three-quarters of a minute after last contact in occultation, at which time the line of sight to the part of the satellite farthest from the planet's edge passed about 375 miles plus the satellite's own diameter within the apparent exterior boundary of the planet. Doubtless, however, Mr. Carlisle saw only the part of the satellite nearest to the edge of the planet, and probably we must not suppose that the lines of sight to the portion of the satellite remaining visible passed anywhere to a much greater depth than 500 miles within the planet's atmosphere. Several observers have noticed that when satellites pass obliquely into the shadow of the planet, or when the fourth satellite passes behind the planet just above or below the shadow so as to skirt the penumbral region, the light of the satellite is subject to variations. Mr. Ranyard has collected a number of such observations in a paper published in the 'Monthly Notices' for June, 1883; for example, Dr. T. D. Siminton, in describing an eclipse of the fourth satellite, which he observed in March, 1883, when it just entered the northern edge of the planet's shadow, says that after the satellite had disappeared it continued to be seen by glimpses for a minute or two before it again emerged from the shadow. Mr. Gorton, in describing an occultation of the second satellite, remarks that it seemed to disappear and appear again several times. Professor E. C. Pickering, in describing an occultation of a star by Jupiter, which he observed on April 14, 1883, says that for about two minutes before final disappearance the star alternately disappeared and reappeared without any obvious cause, though the seeing was pretty good and uniform throughout. Professor Pickering gives a curve representing the variations in the light of the star for two and a half minutes before its final disappearance. During this time the planet moved through a distance of 2,600 miles, and the rays from the star passed through the planet's atmosphere at the time when the first change of intensity was observed, at a height of 890 miles above the level at which the rays were last transmitted just before the light of the star was extinguished. In 1878, Mr. Todd, of Adelaide, with an 8-inch refractor and in the singularly clear air of South Australia, saw II. and I. three times certainly and twice doubtfully as though 'through the edge of the disc,' his assistants confirming the observation. Strenuous efforts have been made to deprive these observations of significance. One compares them to the occasional (apparent) projection of stars slightly within the edge of the moon's disc, a phenomenon manifestly explicable either as an effect of irradiation or as due to the existence of deep ravines or valleys chancing to lie at the edge of the visible lunar hemisphere. The appearance of a disc still continuing visible within the apparent edge of Jupiter's disc, after an observation of the continuous approach of the disc to that edge and its passage behind it, cannot

possibly be explained as an effect of irradiation. Figs. 402 and 403 show what these effects would be, fig. 402 showing the appearance of the satellite at the moment of first contact, fig. 403 showing its appearance when half occulted, the shaded portion being the irradiation fringe, wider outside the satellite than outside the planet's edge, because the satellite is brighter than the outer part of the planet's disc. Fig. 404, on the other hand, shows what Mr. Todd really saw. It is especially noteworthy (and would of itself dispose of the irradiation explanation if this had any validity otherwise) that Mr. Todd says, 'the satellites are visible through the dusky, hidden by the bright parts of the limb, these being the parts where irradiation would be greatest.' It has also been suggested that the eye of the observer wearying

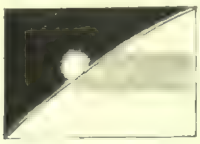


Fig. 402.

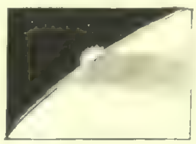


Fig. 403.

Possible effects of radiation on a satellite undergoing occultation.

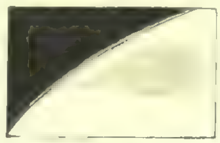


Fig. 404.

Occultation as actually observed by Mr. Todd and others.

as he watched occultation, a false image of the edge of Jupiter's disc might have deceived him—this false image lying outside the satellite, though the true outline lay still within it; but this explanation overlooks the fact that the false image would include the satellite as well as the planet, and the false picture of the satellite would be outside the false picture of the planet, just as in the real image (were the satellite as yet unocculted). Moreover, in the Adelaide observations, which are the most striking of all—as might be expected from the more favourable conditions—Mr. Todd's observations were confirmed by the unwearied eyes of his assistant.[1] It is noteworthy that objections of this sort are almost invariably raised by those who have only

[1] On September 14, 1879, the observers at Melbourne watched the occultation of the star 64 Aquarii by Jupiter with the great reflector. The star was 35 seconds in disappearing, and remained visible for 10 seconds longer as a speck of light seen through ground glass; this speck 'also disappeared gradually.' I was in Melbourne a few months after this observation was made, and had the opportunity of discussing all the circumstances with the observers themselves. They were satisfied that the phenomenon was not due to irradiation (64 Aquarii is of the sixth magnitude only); and if irradiation was not in question, the star was seen below a depth of about 300 miles of the Jovian atmosphere, and through a range of about 10,000 miles. And although it must be admitted that in this case irradiation supplies a possible explanation, yet, as irradiation cannot at all explain the cases considered in the text, we may conclude with some probability that a small star was actually seen by the Melbourne observers through several thousand miles of the Jovian atmospheric surroundings. It would in any case be a somewhat wide expansion of the possibilities attached to the effects of irradiation, to suppose that the position of Jupiter's edge could be affected—when he was within a fortnight of opposition, and therefore retrograding nearly at his greatest rate—by an irradiation-fringe corresponding to Jupiter's motion during 45 seconds (or longer in the opinion of the Melbourne observers).

observed the phenomena of Jupiter's satellites occasionally; and the remarkable observations have nearly all been made by those who have observed these phenomena systematically.

(1335.) *Thirdly*, the shadows of the satellites are not always round, and have even been observed occasionally to be double. The Melbourne observers have seen the shadow of III. perceptibly elongated in the direction of the belts ('flattened at its poles' is their description) when in mid-transit, and others have observed the same shadow similarly affected. On April 5, 1861, Mr. T. Barneby saw the shadow of III. for a while as a broad dark streak, as if the cone of shadow were visible in a semi-transparent medium; 'but it afterwards appeared as a circular spot, perfectly dark and much larger than' the shadow of another satellite visible at the same time. Burton has often noticed ellipticity in the shadows The shadow of II. has been seen irregularly shaped, a phenomenon readily explicable if the surface on which the shadow fell was not uniformly dense, so as to be more transparent in some parts than in others. Cassini once failed to find the shadow of I. *at all*, at a time when calculations showed that it should have been on the disc—a circumstance only to be explained by supposing that the shadow fell on a part of the atmospheric surroundings too transparent to receive it, the cloud-layer or other surface where it really fell lying beneath a layer sufficiently dense to hide the shadow from view. This is illustrated in fig. 405. The observer on Earth, looking at the spot S S′ where the shadow should have been seen, would receive through the opening S S′ in the cloud-layer C C light from the part *a b* of the lower cloud-layer *c c′*; while, looking towards *s s′*, the place of the actual shadow, he would receive light from the part A B of the upper layer. The shadow of I. has been seen grey instead of black by Mr. Gorton; as has that of II. by Messrs. Buffham, Birt, and Grover; also by Terby and Flammarion on March 25, when the shadow of III. close beside it was black. But even more decisive than such observations as these are those cases in which the shadow of a satellite has appeared double. Sir J. South, with his great achromatic, and (less clearly) with a smaller telescope, saw each of the two shadows of satellites on Jupiter to be attended by a fainter shadow. A later observation of a double shadow to a satellite was made by Mr. Trouvelot, at Cambridge, Mass., with a telescope $16\frac{1}{3}$ inches in diameter on April 24, 1877. He watched the shadow and its faint companion[1] for 1h. 20m. to make sure

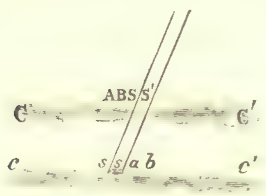


FIG. 405.—Illustrating the possible invisibility of the shadow of one of Jupiter's satellites.

[1] Though the companion shadow appears faint, it was really the darker shadow of the two. The light which made it seem faint was, of course, that received from the upper layer of clouds through which the shadow was seen.

that it was no spot on Jupiter which seemed for the time to accompany the true shadow, and remarks justly that, this not being the case, the phenomenon can only be explained by assuming that, while the main shadow was received on the upper layer (it will be observed that, as usual in the case of such phenomena, the shadow falls on a dark belt, itself doubtless an inner layer), a small portion of the light around the shadow-cone found its way through that layer, and a more transparent region beneath it, to fall on another layer of cloud (or, perhaps, he suggests—though this is altogether unlikely—the real surface of the planet), so that then a second shadow was formed. And he remarks, naïvely, that the depth beneath the surface at which the inner layer lies can be determined (*cette profondeur, du reste, est susceptible d'être mesurée approximativement par les donnés de l'observation*), as I propose to show; but he gives no reason for omitting the determination, which, therefore, we must obtain for ourselves.[1] Fortunately the work is easy enough, though it seems to have struck M. Trouvelot as too great to be attempted. Let j J be the quadrant of Jupiter on which the double shadow was seen, S, s', the places of the two shadows' centres. From the diagram given by M. Trouvelot it seems that the distance between the two shadows was about equal to the diameter of the satellite, say 2,500 miles, and that the shadows were at a distance of three-quarters of a radius from the centre of the disc, projecting $j\,s'$ S J on the radius C J, as C L, K J, L K the projection of the distance between the shadows, equal to about 2,500 miles,

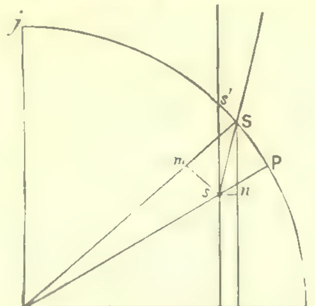


Fig. 406. Showing the depth of the layer on which the second shadow was seen.

C J being 43,260 miles; but we put it at 2,000 to be on the safe side; also K J is equal to about one-fourth of C J. We also find that, at the time of observation five days following Jupiter's station preceding opposition, the line of sight j C from the Earth to Jupiter's centre was inclined about 10° to the line joining the centres of Jupiter and the sun. Drawing, then, S s from S the centre of the dark shadow, inclined 10° to S K, meeting s' L in s, which is thus determined as the centre of the fainter shadow, and drawing $s\,n$ and $s\,m$ perpendicular to S K and C S, we find that S s is equal to 2,000 miles divided by sin 10°—that is, in round numbers, to 11,500 miles—whence we readily find

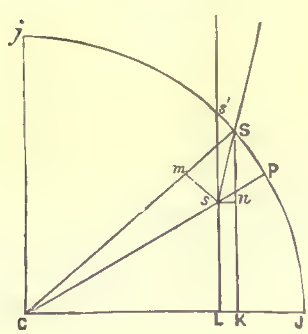
[1] There would be something provoking (if the work of those who provide the raw materials of research could at all provoke the earnest seeker after truth) in the placid way in which those who claim to be the only true scientific workers pitchfork their observations into the journals and proceedings of scientific societies without the slightest effort to deduce from their observations their real significance and value. They leave others to do this, and then quietly say of the work of these others, 'That is mere theorising; you must come to us for practical work in the form of actual observations.' There is nothing really practical, or even sensible, in observing without reflecting, or in observing for observing's sake, leaving true research to take care of itself.

that[1] the inner layer on which the faint (or faintly seen) shadow was received lay 7,480 miles below the layer (itself probably deep) on which the darker shadow was seen. Remembering that $s\,n$ was probably nearer 2,500 than 2,000, we are certainly far within the truth in saying that, on this occasion, this particular part of the atmospheric surroundings of Jupiter was partially transparent to a depth of fully 8,000 miles.

(1336.) *Fourthly.* It has occasionally happened that a satellite (generally the fourth) entering the shadow of the planet has reappeared for a short time after having disappeared, having evidently entered a part of the shadow less dense than the part first entered. This could not happen unless at the time a portion of the atmospheric surroundings of Jupiter had been partially transparent to a depth of several thousand miles.

(1337.) All the known facts—Jupiter's aspect and changes of aspect, its physical condition as indicated by its apparent volume and mass, the mathematical study of the satellites' motions, their behaviour on special occasions, when approaching and entering on the disc both before transit and before occultation, the peculiarities at times presented by their shadows, and the occasional visibility of satellites after the cone of the planet's shadow has been fairly entered—combine to prove that, as in the sun's case, the real globe of Jupiter lies far within the globe we see and measure. I might add the occasional flattening of parts of the outline of Jupiter observed by Schröter and others; but this is not so well attested as corresponding evidence in the case of Saturn, evidence which, as considered farther on, is almost as effective, though indirectly, in regard to Jupiter as in regard to his brother giant. When we consider that antecedent probabilities would lead us to expect, and to expect very confidently, to find Jupiter because of his much greater mass in a much earlier stage—that is, a much hotter stage of orb life—than our Earth, it may be regarded as practically proved that Jupiter's condition rather resembles that of a small sun which has nearly reached the dark stage,[2] than that

[1] The angle $C\,S\,s = \sin^{-1}\dfrac{C\,K}{C\,S} - 10^\circ$
$= \sin^{-1}\frac{3}{4} - 10^\circ$
$= 38^\circ\ 35'$,



whence $m\,S = S\,s \cos 38^\circ\ 35' = 11{,}500 \times 0{\cdot}7{,}817 = 8{,}208$ miles;
$C\,m = 43{,}260 - 8{,}208 = 35{,}052$ miles;
and $m\,s = S\,s \sin 38^\circ\ 35' = 7{,}172$ miles;
whence $C\,s = \sqrt{(7{,}172)^2 + (35{,}052)^2} = 35{,}780$ (very approximately),
and $s\,P = 43{,}260 - 35{,}780 = 7{,}480$ miles.

[2] Although it seems probable that Jupiter still retains some inherent light, and that a portion of such inherent light reaches us along with reflected sunlight, it is certain that such inherent light as Jupiter actually emits through his atmospheric surroundings forms but a small part of the light which we receive from him. This is shown by the darkness (which, however, is not necessarily blackness) of the shadows. It is not, however, shown, as some have asserted, by the disappear-

of a world which is within a measurable time-interval from the stage of orb-life through which our own Earth is passing.

ance of the satellites in Jupiter's shadow. For even if a fourth of Jupiter's light were inherent, the illumination of the satellites when in his shadow would be so much less than that which they receive from the sun, that they would not be visible in the shadow. It is true that, as seen from his nearest moon, Jupiter presents a disc nearly 1,300 times as large as the full moon; but even the full intrinsic brightness of this disc is less than $\frac{2}{15}$ of the moon's; see data given in Art. 1036, note, from which it is evident that the intrinsic brightness of Jupiter is $\frac{6238}{1736 \times 27}$ or about $\frac{2}{15}$ that of the moon, and $\frac{1}{4}$ of this multiplied by 1,300 would be but $43\frac{1}{3}$ times full moonlight. Now the illumination of the satellites of Jupiter when not in shadow is $\frac{1}{27}$ of our sunlight, or about 22,900 times full moonlight (see Art. 1163); so that the inner satellite would have its illumination reduced, when in the planet's shadow, to about $\frac{1}{540}$ of its lustre when not shadowed, and would be quite invisible.—A. C. R.

CHAPTER XIV.

THE PLANET SATURN.

(1338.) THE mean distance between the orbit of Jupiter and that of Saturn, the next (or sixth) primary planet in order of distance from the sun, is about half as large again as the entire span of the system of the four terrestrial planets, and is not much less than Jupiter's distance from the sun; so that in passing from the fifth to the sixth primary planet, the distance from the sun is nearly doubled.

Saturn travels around the sun in 10759·22 days, or 29 years (tropical) 167·2 days, at a mean distance of about 885,015,000 miles. The eccentricity of his orbit is ·056. Hence his distance from the sun when he is in aphelion exceeds his mean distance as 1,056 exceeds 1,000, while his distance when he is in perihelion is less than his mean distance as 944 is less than 1,000. Thus his greatest, mean, and least distances are as 132, 125, and 118. Calling the Earth's mean distance 1, the greatest, mean, and least distances of Saturn are as 10·07, 9·54, and 9·00. (Observe that 9·00 is not here the same as 9, but indicates a value between 9·005 and 8·995.) Plate X. shows the position of Saturn's perihelion at S, and of his aphelion at S′; and it will be manifest from a comparison of that plate with the first plate of planetary orbits and the dates there shown, that we must get the nearest view of Saturn if the Earth is on the side of her orbit towards S when Saturn is there, or technically when Saturn is in opposition in December and January; whereas, if the Earth is towards S′ when Saturn is in opposition, the planet is at its greatest opposition distance from the Earth. The distances of Saturn from the Earth under these conditions range from about 8·00 at an opposition near S to about 9·07 at an opposition near S′, calling the Earth's mean distance from the sun 1. Plate X. also shows how much Saturn ranges above and below (that is, north and south of) the plane of the Earth's orbit, in which the picture is supposed to be drawn—see the little arrowed I's set near the orbit, midway between ☊ and ☋, the places where Saturn crosses the plane of the ecliptic ascendingly or passing from south to

north of it, and descendingly, or passing from north to south of that plane, respectively.

(1339.) In Saturn's case, as in that of Jupiter, it will be well to consider the orbit of the planet and that of our Earth together ; but we need not consider them in quite so detailed a manner.

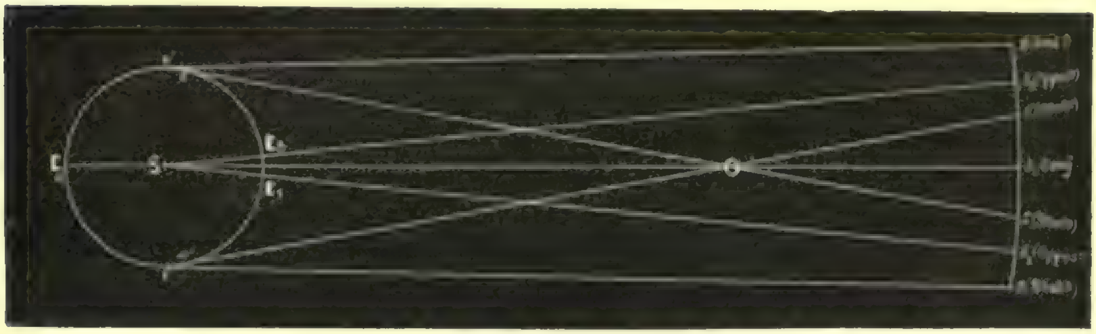


FIG. 407.—The Earth's orbit ($E_1E_2E_3$), and part of Saturn's orbit ($S_1S_2S_3$).

Fig. 407 shows the Earth's orbit E_1 E_2 E_3 about the sun, S, and a portion s_1 s_2 s_3 of Saturn's orbit, traversed by the planet while the Earth is passing from E_1 round to E_2 and so onward to E_3 ; S E_1 s_1, S E_2 s_2, and S E_3 s_3 being straight lines passing severally through simultaneous positions of the Earth and Saturn. Saturn is shown at his mean distance throughout. At s_1 he is in opposition (to the sun), the Earth being at E_1, so that the sun at S and Saturn at s_1, are on opposite sides of her. As Saturn passes on to s_2 the Earth passes round through E_3 to E_2, and Saturn being now at s_2 is in conjunction with the sun at S. Passing on over the same distance to s_3, Saturn is again in opposition, the Earth being at E_3, with the sun at S and Saturn at s_3 on opposite sides of her.

In fig. 407, S E_1, S E_2, and S E_3 are supposed each equal to 1; S s_1, S s_2, and S s_3 are each equal to $9\frac{1}{2}$; and therefore E_1 s_1 and E_3 s_3 are each equal to $8\frac{1}{2}$, representing Saturn's average distance when in opposition, while E_2 s_2 is equal to $10\frac{1}{2}$, representing Saturn's average distance when in conjunction. As with Mars and Jupiter, so with Saturn and all the superior or outer planets, the time of opposition is the best for observation, the planet being then not only at its nearest, but seen under most favourable conditions at its highest in the midnight sky. When Saturn is in conjunction, or at his farthest from the Earth, we cannot see him because his place in the heavens is close by the place occupied by the sun, in whose beams the faint light of Saturn is altogether lost.

Saturn changes even less in aspect than Jupiter. It is not necessary to indicate in fig. 407 the greatest divergence between lines from the

Earth and sun to Saturn, when Saturn is in quadrature to the sun, because Saturn, when at or near a station, is near enough to quadrature for the lines e *s*, *e s*, *e′s′*, e′*s′*, showing his stationary directions, to indicate sufficiently (on the smaller scale of fig. 407, as compared with fig. 374) his quadrature-directions also. So far as the globe of the planet is concerned, a slight diminution of the light on the side of Saturn's disc farthest from the sun is all we could expect to recognise when Saturn is in or near quadrature ; no gibbosity can be recognised as in the case of Mars, nor even so marked a lessening of the light as in the case of Jupiter. But owing to the existence of a system of rings around Saturn, certain very characteristic peculiarities of appearance, presently to be described, arise in consequence of the Earth's measurable displacement on either side of the line joining the sun and Saturn.[1]

(1340.) Saturn presents a somewhat flattened form, such as is shown in fig. 408, which gives a rough view of his globe (and of the strange system of rings surrounding him, presently to be described) for comparison with the Earth's globe, shown at E.[2] The compression of Saturn's globe is estimated at about $\frac{1}{10}$, though the best modern observations make it somewhat greater. An equatorial diameter of Saturn is about 70,930 miles long ; his polar diameter being about 62,840 miles. Thus Saturn's volume is about 645 times the Earth's ; and since his mass is only 94·384 times the Earth's, his density is only 0·146, or about $\frac{6}{41}$sts of hers. This is the lowest mean density yet known to exist throughout the whole solar system ; and probably it is no mere coincidence that it is found in company with the most remarkable configuration—in fact, with relations

Fig. 408.—The globes of Saturn S, and the Earth E, on the same scale.[2]

[1] The student will do well to read over again here what is said in Arts. 1268 and 1269 respecting the quadratures of Jupiter, the considerations to be attended to in the case of Saturn being precisely similar, and differing only in regard to the amount of displacement which the direction-lines to the planet undergo. He will also find it well to go through such constructions in Saturn's case as are illustrated for Jupiter's case in fig. 374.

[2] It may interest some readers to learn that this picture was engraved from the first astronomical views I ever drew, viz. a set of three views of Saturn intended to illustrate an essay on Saturn which I wrote for the *Cornhill Magazine* in 1864. (It was deemed 'too difficult' for the readers of that magazine.) Although this picture appears without acknowledgment in Sir R. Ball's *Story of the Heavens*, I have no doubt it was one of a number lent (without my authority) for that work, and that he had no idea who had drawn it.—R. A. P.

indicating, like that low density, extreme youth. Saturn's axis is inclined 28° 10′ 22″ from uprightness to the plane in which the Earth travels, but only 26° 49′ 28″ from the plane of Saturn's own orbit. So that if the planet had seasons they would be alien to the Earth's, the spring of his northern hemisphere occurring when he was in about longitude $167\frac{3}{4}$° (see Plate X.)

(1341.) When Galileo first turned his most powerful telescope towards Saturn in July, 1610, he saw the planet, not as a single body, but apparently triple. Two equal small discs appeared to be set symmetrically on either side of a larger one. With his weaker telescope the planet looked simply elongated, and he compared its shape to that of an olive. In November Galileo announced to Kepler that Saturn consists of three stars in contact with one another, expressing this in the anagram (after the somewhat childish fashion of the age) which, when duly transposed, ran : '*Altissimum planetam tergeminum observari*; that is, 'I have observed the most distant planet to be threefold.'[1] By December two smaller bodies, set at equal distances on the eastern and western sides of Saturn, had diminished in relative size. During the oppositions of the next two years they continued to grow less till they vanished altogether. When announcing this mysterious phenomenon to Welser, in a letter dated December 4, 1612, Galileo expressed more than wonder– something of anxiety was mixed with the astonishment he naturally experienced. 'What is to be said,' he asked, 'concerning so strange a metamorphosis ? Are the two lesser stars consumed after the manner of the solar spots ? Have they vanished and suddenly fled ? Has Saturn, perhaps, devoured his own children ? Or were the appearances, indeed, illusion or fraud, with which the glasses have so long deceived me, as well as others to whom I have shown them ? Now, perhaps, is the time come to revive the well-nigh withered hopes of those who, guided by more profound contemplations, have discovered the fallacy of the new observations, and demonstrated the utter impossibility of their being valid. I do not know what to say in a case so surprising, so unlooked for, and so novel. The shortness of the time, the unexpected nature of the event, the weakness of my understanding, and the fear of being mistaken have greatly confounded me.'

It affords a suggestive lesson to note that in Galileo's observations of Saturn, the true nature of the appendage which he mistook for two

[1] Kepler was not to be deterred by the theoretical hopelessness of an attempt to determine the true reading of such an anagram, from resolutely attacking Galileo's logogriphe. He had already stated that he expected to hear of the discovery of two Martian moons, and he therefore resolutely forced Galileo's anagram into the barbarous Latin --*Salve umbistineum geminatum Martia proles*, which, if we regard *umbistineum* as not altogether an impossibility, may be supposed to signify, 'Hail, twin companionship ! (as more congruous), Martian offspring.'

attendant planets was in reality implied. A resolute application of deductive methods to the facts observed would have shown so sound and clear a reasoner as Galileo, that only one reason was available, that, namely, which Huyghens deduced forty-six years later. However, he left the matter there ; and though we may well reject the story that in his disgust at the strange phenomena he had witnessed, he determined to observe the planet no more (for such a feeling would have been utterly unworthy of him), yet he failed to interpret Saturn's changes of aspect. Nor did Hevelius succeed better, though he used more powerful instruments, and in his treatise '*De nativâ Saturni facie*' concealed his perplexity under sesquipedal words, well calculated to impress the ignorant. He called Saturn's aspects the *mono-spherical*, the *tri-spherical*, the *spherico-ansated*, the *elliptico-ansated*, and the *spherico-cuspidated* ; which had a dignified sound : but he omitted to point out that the meaning of these various phases it was utterly beyond his power to determine.

(1342.) Huyghens, if he considered less the dignity of verbal science, dealt with the problem in a more scientific spirit. Analysing the various theories which had been advanced up to March 1656, when the rings were turned edgewise towards the earth (for the fourth time since the invention of the telescope), he rejected them all, except a partly suggested idea of Roberval's, that perspective had something to do with the changes of Saturn's aspect. He showed by irresistible reasoning that the phenomena can be explained, and can only be explained, by assuming the existence of a thin flat ring surrounding the planet, and nowhere touching it, the ring being inclined to the plane of the planet's motion, so that at two parts of Saturn's circuit the ring is turned edgewise towards the Earth, and so disappears from view ; while at two intermediate positions the ring appears (through the effects of perspective) opened out to its greatest apparent width. He announced this discovery in the year 1659, in the form of an anagram formed from the words '*annulo cingitur tenui, plano, nusquam cohærente, ad eclipticam inclinato.*' 'It [Saturn] is surrounded by a thin flat ring, nowhere touching [the planet and] inclined to the ecliptic.'

Huyghens showed clearly that the various changes in the appearance of Saturn as observed by Galileo, Scheiner, Riccioli, Hevelius, and others, corresponded with the aspects which a planet girdled round by such a ring as he described would have presented. Fig. 409 is from his *Systema Saturnium*, and serves to give a good idea, not only of the unsatisfactory material on which he had to work, but also of the performance of the telescopes employed during the first half-year after the invention of the instrument. It is to be regretted that we cannot form an idea from these drawings of the probable breadth of the ring-system two centuries and a

half ago. But manifestly we cannot place any trust in the proportions presented by these pictures.

(1343.) Huyghens's interpretation of the rings was naturally received with much doubt, and even, in some quarters, with ridicule, for the validity of

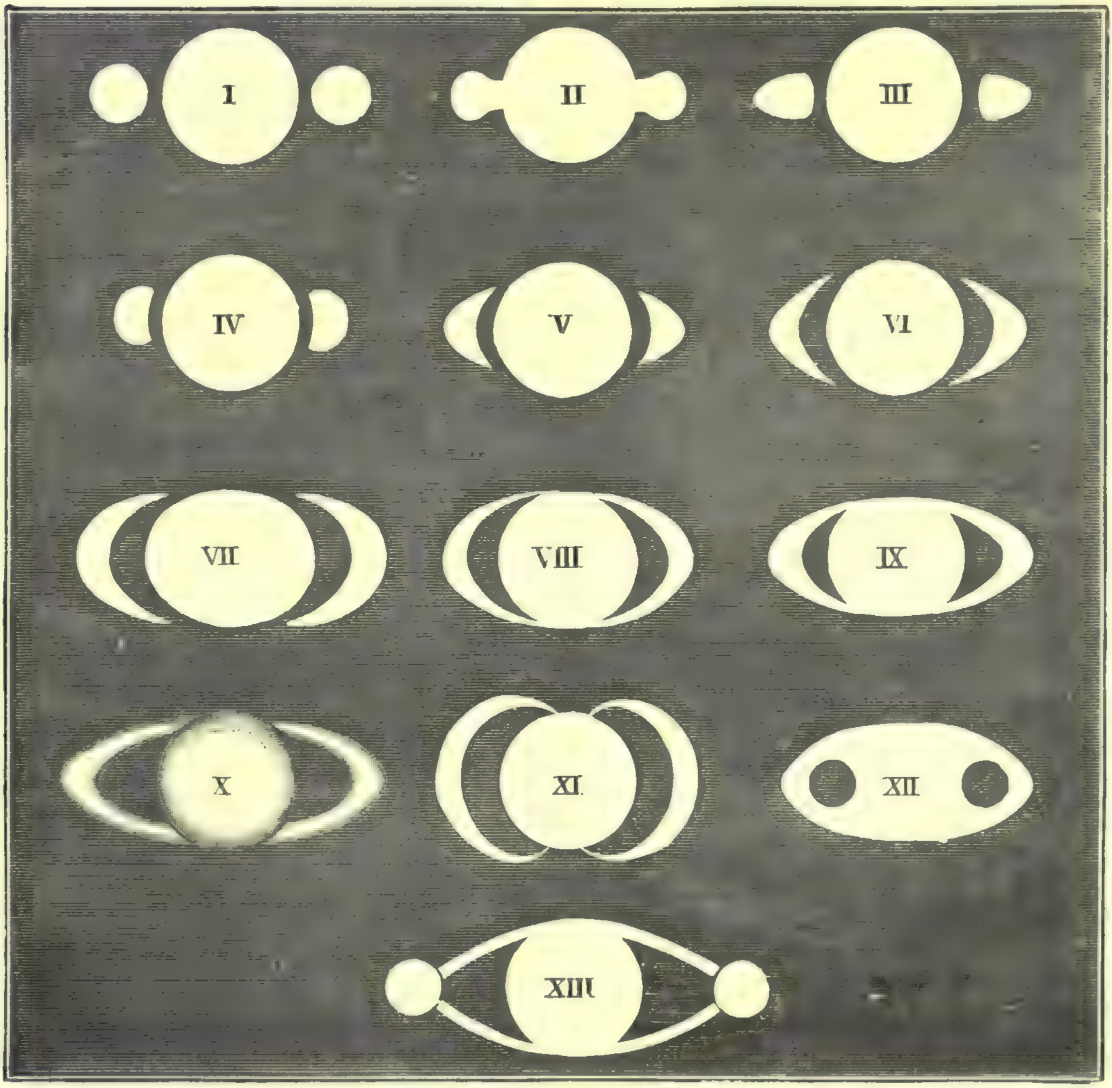


Fig. 409.—Saturn as pictured during the first half of the seventeenth century: I. by Galileo in 1610; II. by Scheiner in 1614, 'showing ears to Saturn'; III. by Riccioli in 1640 and 1643; IV., V., VI., and VII. by Hevelius, showing changes of aspect; VIII. and IX. by Riccioli between 1648 and 1650, ring most open in May 1649; X. by the Jesuit Eustachius de Divinis; XI. by Fontana; XII. by Gassendi and Bianchini; XIII. by Riccioli.

his explanation depended entirely on reasoning. Even so late as 1665, we find a writer in the 'Philosophical Transactions' directing Huyghens's attention to a drawing by Ball (fig. 410), as calculated to 'make him think that it is not one body of a circular figure that embraces the disc, but two,' a sugges-

tion which, oddly enough, led to the idea that Ball had recognised the division between the rings of which we shall presently have to speak. The drawing referred to was made by William Ball and his brother Dr. Ball, at Mamhead near Exeter, October 13, 1665, with a very good telescope 38 feet long, and a double eyeglass (meaning what we now call an eyepiece), William Ball noting that he had never seen the planet more distinctly. It is manifest that Wallis, or whoever wrote the account in the Royal Society's Transactions, refers to the depressions at A and B as 'a little hollow above and below.'

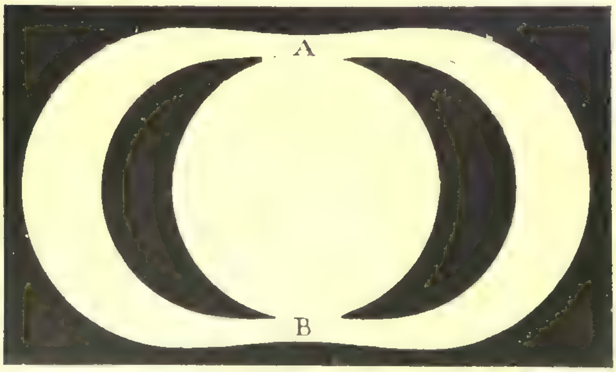


FIG. 410. Ball's drawing[1] of Saturn in 1665. Facsimile from 'Philosophical Transactions.'

Professor Adams has suggested[1] that these words were not used by Ball himself, but were added by somebody else—possibly Oldenburg, who was then Secretary of the Royal Society—to explain the peculiarities of the figure, which seems to have been made from a piece of paper cut out in the form of the planet. Such a piece of paper is still in the possession of the Royal Society with the writing of Oldenburg upon it. The defect in the paper cutting probably originated, says Professor Adams, in the following way: 'In order to make the cutting the paper was first folded twice, in directions at right angles to each other, so that only a quadrant of the ellipse had to be cut. The cut started rightly in a direction perpendicular to the major axis, but through want of care, when the cut reached the minor axis, its direction formed a slightly obtuse angle with that axis instead of being perpendicular to it. Consequently, when the paper was unfolded, shallow notches or depressions appeared at the extremities of the minor axis.' The mistake seems to have been shortly afterwards discovered, and Mr. Lynn has shown that the plate was soon suppressed, but not before several copies had got into circulation. The remark upon the diagram

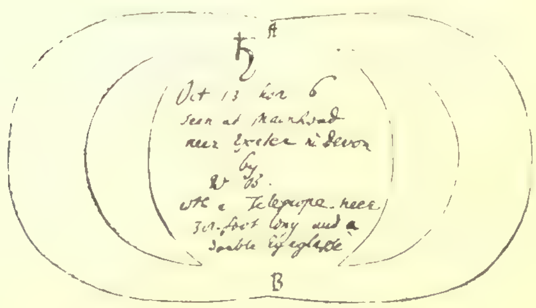


FIG. 411.—An exact copy of Ball's paper cutting of Saturn.

[1] See paper by Professor J. C. Adams on William Ball's observations of Saturn in the *Monthly Notices* for January 7, 1883.

by the writer in the 'Philosophical Transactions,' that the appearance may perhaps have been caused by the planet being surrounded by two rings instead of one, caused Ball to be credited with the discovery that the ring was double, and the division between the rings was spoken of as Ball's division. The history of the mistake is curious, for during the last century the dark gap between the rings seems to have been known by the name of its true discoverer, Cassini. Captain Noble has traced the mistake back to Kitchener, who seems to have found the passage in the 'Philosophical Transactions' and concluded that Ball discovered the duplicity. Thenceforward the chief division between the rings was known as Ball's division till 1882, when Mr. Lynn threw doubt upon Ball's discovery, and bit by bit the story of the mistake was unravelled. [A. C. R.]

When, in December 1671, the ring disappeared, as Huyghens had predicted that it would, the doubts with regard to Huyghens's theory disappeared with the ring, and henceforth observations of Saturn's ring-system were properly interpreted.

(1344.) In 1675–6 Cassini discovered a dark marking on the southern face of the ring, then turned earthwards (the ring being nearly as fully opened as it ever is, in the year 1678). This marking, as drawn by Cassini, is shown in fig. 412, and though it can hardly be described as indicating the existence of an actual division, yet it entitles Cassini to whatever credit may be due for first possessing a telescope powerful enough to show traces of the great division. Maraldi long after observed a similar marking on the northern face of the ring. W. Herschel, having noted and measured the dark marking on the northern face of the ring before 1789, recognised the marking in the southern face afterwards brought into view, and found on measurement that the marking thus seen corresponded precisely with the marking on the northern face. Then, and not till then, could it be regarded as demonstrated that what had been called the ring of Saturn is divided into two rings separated from each other by a broad circular gap.

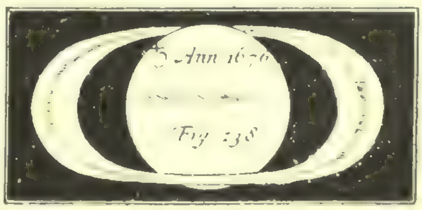


Fig. 412. Cassini's picture of Saturn, showing the great division as a shading. Facsimile.

(1345.) It will be well to leave the history of discoveries relating to Saturn's ring-system for awhile, in order to sketch the discovery of the various members of his system of satellites. We can deal with these in convenient compass, and, unlike those of Jupiter, the satellites of Saturn have disclosed very little of interest respecting their primary; whereas the study of the ring-system has proved full of interest and exceedingly instructive, so

that it will be well to consider what remains to be described in regard to the rings, and the discussion of their physical condition, without the interruptions which would arise if we considered the various discoveries respecting the rings, the satellites, and the physical aspect of the planet, in their true chronological order.

(1346.) On March 25, 1655, Huyghens with his 12-feet telescope detected a satellite (the one now known as the sixth, or Titan) revolving round Saturn in a period of 15 days 27¾ hours, at a distance of 769,280 miles. Titan is larger considerably than the largest of Jupiter's moons, and cannot be much inferior to Mars. As six satellites (counting our own and Jupiter's four moons) had now been discovered, and only six primary planets were known, it was maintained (with the customary confidence of half knowledge) that it would be idle to look for more satellites. But in October 1671 Cassini discovered another Saturnian satellite revolving round Saturn in a period of nearly 79 days 8 hours at a distance of 2,235,730 miles from Saturn. This satellite, Iapetus, is the outermost or eighth of the system as now known. It is probably about as large as the third or largest of Jupiter's moons, and is remarkable as having by far the widest orbit and longest period of all known satellites. On December 23, 1672, Cassini (having crossed the Rubicon) discovered a third satellite (Rhea, the fifth in distance), revolving round Saturn in a period of 4 days 12½ hours, at a distance of 331,850 miles; and in March 1684 he discovered two more, revolving within the orbits of the other three; viz., the satellites now known as the 3rd and 4th, Tethys and Dione, revolving round Saturn respectively at distances of 185,490 and 227,490 miles in periods of 1 day 21¾ hours and 2 days 17¾ hours. Rhea is considerably inferior in brightness to Iapetus, while Tethys and Dione are considerably inferior in brightness to Rhea. Probably Rhea is about 2,000 miles in diameter, Tethys and Dione each about 1,500 miles. No new satellites were discovered for more than a century. On August 19, 1787, Sir W. Herschel suspected the existence of a sixth satellite, and on August 27, 1789, he was able to assure himself that the object was really a satellite (Enceladus, 2nd in order of distance from Saturn), travelling in a period of 1 day 8 hours 53 minutes, at a distance of 149,810 miles from Saturn's centre. Within three weeks, while studying the motions of this satellite, he discovered another (Mimas, the nearest to Saturn), travelling in a period of 22 hours 37½ minutes at a distance of 116,750 miles from Saturn's centre. Mimas and Enceladus, judging from their brightness, or rather faintness, are probably each about 1,000 miles in diameter. The eighth satellite (seventh in order of distance, and called Hyperion) was discovered in September 1848, independently by Mr. Lassell

at Liverpool, and by Mr. Bond at Cambridge, Mass.[1] It travels between the orbits of Titan and Iapetus, being thus the seventh in order of distance from Saturn, its actual distance from Saturn's centre being 930,410 miles, its period of revolution 21 days $7\frac{1}{4}$ hours. Hyperion is so faint that probably we cannot assign to it a greater diameter than 800 miles. (It appears not improbable that Hyperion is the largest of a ring of small bodies travelling between the giant satellites, Titan and Iapetus.) The system of Saturn and the proportions of the orbits of his several satellites and of his rings are shown in the second picture in Plate X.

(1347.) The satellites of Saturn have not thrown so much light on the conditions of their primary as have those of Jupiter ; nor do we know as much about their own condition, rotation, and kindred relations :—

It has been suspected that Iapetus turns on its axis once, while revolving once around Saturn, a relation which would seem antecedently unlikely when the physical conditions on which such synchronism between rotation and revolution depends (Art. 1143) are taken into account ; moreover, the occasional faintness of this satellite has not been observed at such times only as would correspond with the law of rotation.[2] Sir W. Herschel considered that at its easterly elongation, the light of Iapetus was less than its light at its westerly elongation, in the same degree that a fifth-magnitude star is less than one of the second magnitude. But Cassini found the amount of change variable, and Mr. Banks had a similar experience in 1866. It is unfortunate that the large telescopes of recent times are not likely to be turned to researches of this kind. The discovery of some as yet unseen minute attendant on one of the hundreds of millions of fixed stars, seems so much more likely to be associated hereafter with the name of the observer than the recognition of new details in an object which has been associated with someone else's name, that the astronomers to whom the large telescopes are entrusted will not waste their opportunities on work of the later class.

(1348.) Bessel, from his researches into the perturbations of Titan (and especially into the motions of Titan's peri-Saturnium), estimated the limited total mass of the ring-system at $\frac{1}{118}$th of Saturn's. Very little reliance, however, can be placed on this estimate formed before the satellite Hyperion had

[1] It seems that Bond preceded Lassell by a couple of days in making the discovery of Hyperion—a point on which those who take a narrow view of such matters earnestly insist. The many duplicate discoveries independently made should lead us to recognise the fact that discoveries are generally only the last and most fruitful step in a chain of investigations or thoughts which have been contributed to by many minds.—A. C. R.

[2] In Professor Grant's fine work, the *History of Physical Astronomy*, the outer satellite is spoken of as the fifth throughout the paragraph relating to its changes of apparent brightness. It was the fifth until Sir W. Herschel had discovered Mimas and Enceladus, and in Cassini's later account of his inquiries into its apparent brightness was so numbered throughout. But it is to Iapetus, not Rhea, that his inquiry and Sir W. Herschel's (*Phil. Trans.* for 1792) related.

been discovered. Bessel's result, indeed, was considerable, so unlikely, on *à priori* considerations, that it might have been regarded as suggesting the probable existence of an unknown satellite, a ring of satellites travelling near the orbit of Titan, than so large a mass in the ring-system. Had Bessel suggested the search for new Saturnian satellites as the result of his calculations he would have done something to counterbalance the mischievous effects of his carelessness in regard to Neptune, as mentioned in Chap. XVI. But unfortunately this most able observer and skilful calculator was scarcely ever moved to original conceptions.

(1349.) The orbits of the seven inner satellites are little inclined to the plane of Saturn's equator, as the following table shows.

	Inclination to ecliptic	Longitude of node		Inclination to ecliptic	Longitude of node
Ring system .	. 28°10′	167°43′	Rhea . .	28°11′	166°34′
Mimas . .	. 28°10	167°43′	Titan . .	27°34′	167°56′
Enceladus .	. 28°10′	167°38′	Hyperion. .	28° ±	167° ±
Tethys . .	. 28°10′	167°38′	Iapetus . .	18°44′	142°53′
Dione . .	. 28°10′	167°38′			

But the orbit of Iapetus, like that of the outer satellite of Jupiter, is nearer to the orbit-plane of the primary: it is also considerably inclined to the plane of the ring-system, which is (or appreciably is) the plane of Saturn's equator.

(1350.) The motions of Titan and other satellites have also been studied by Professor George Darwin, in order to determine the arrangement of the matter forming the mass of Saturn, with the result of showing that the greater portion of the mass of Saturn is, as it were, concentrated within a much smaller globe than that which we see and measure—in other words, that either there is a great increase of density towards the centre, or that the outer regions of Saturn, as we see him, are atmospheric. This last is, of course, the more probable conclusion; but it must be noticed here as in the case of Jupiter (Art. 1287), that in speaking of the outer parts as atmospheric I do not mean it to be understood that they are to be regarded as of the nature of an atmosphere as we understand the term in speaking of our Earth's atmosphere. Gaseous matter of extreme tenuity, and laden with clouds probably most sparsely strewn, the whole probably altogether wanting in continuity of texture, is what we must conceive as forming the surroundings of the giant planets as of the sun,[1] unless we are to imagine laws of atmospheric pressure and density utterly unlike any with which we are familiar on Earth.

[1] I have long insisted that the corona cannot be a 'solar atmosphere'—that is, a gaseous envelope in which one layer rests on another layer. The outer part of the gaseous matter surrounding the sun as well as Jupiter and Saturn is probably in the condition which Crookes has described as 'the fourth state of matter,' namely, a condition in which the molecules are moving in very long free approximately parallel paths.

In the corona, in addition to the luminous gas

(1351.) Sir William Herschel noticed that the satellites appeared to hang on the edge of Saturn at their entry in transit in 1789, Iapetus on one occasion being twenty minutes in apparent contact with Saturn; but the observation has not been confirmed with telescopes of better defining power. That with such telescopes as Herschel's deceptive effects of this kind might be produced is shown by the circumstance that on one occasion, in 1862, Mr. Lassell, with one of his powerful but ill-defining reflectors, saw Titan, as he supposed, occulted by Saturn at a time when Mr. Dawes, using a small refractor of exquisite defining power, saw Titan pass close by, yet distinctly clear of the planet's outline. Occultations and transits of the shadows of satellites have been observed on several occasions when the rings have been turned edgewise so that the apparent paths of the satellites have crossed the disc of Saturn, but transits of the satellites have been very seldom seen, and Mr. Dawes is the only observer who has ever seen a satellite (Titan) eclipsed on the planet's shadow.

(1352.) Saturn's globe presents features akin to Jupiter's; but all the details are seen on a much smaller scale and less highly illuminated, Saturn in opposition being about twice as far from the Earth as Jupiter, and receiving only about $\frac{1}{90}$th, whereas Jupiter receives about $\frac{1}{27}$th, as much sunlight (per square mile in the middle of the disc of either—as supposed to be seen from the sun) as our Earth. The conditions of observations are thus almost twelve times as favourable in Jupiter's case as in Saturn's. Yet from the seventeenth century, and especially from the time when Campani's glasses were directed on Saturn (1664 or thereabouts), the existence of belts in the planet's globe has been noticed, and the correspondence of these in shape with the shapes of the outlines of the rings has been recognised. A great equatorial belt of considerable breadth (see Plate XXV.), yellowish-white in colour, seems to be a permanent feature of the planet. In powerful telescopes this belt presents a mottled aspect, or seems to be flecked with minute clouds—whose real dimensions, however, must be enormous. The darker belts are of a greyish or greenish tint. Sir William Herschel found them yellow, and the equatorial belt white; but his eye was not good for tints of the sort.

(1353.) The belts are not always parallel to the equator. By watching

of which we have spectroscopic evidence, there are clouds (commonly called coronal structures) of finely divided solid or liquid matter, the particles of which, the polariscope shows, are small compared with the wave-length of light. These particles cannot be spoken of as suspended in a gaseous envelope as clouds float in our atmosphere. They are probably either moving freely under the influence of solar gravity or under its influence combined with a repulsive force due to evaporation towards the heated centre. Each molecule thrown off from the heated side of the particle producing a corresponding kick backwards. The elliptic outline of Jupiter and Saturn would seem to show that the upper limit of their cloud surfaces is determined by motion of rotation as well as gravity, and does not correspond to a spherical isothermal shell about a heated centre where dissociation or some physical change takes place, as may possibly be the case with the photosphere. -A. C. R.

irregularities in a triple belt[1] surrounding the planet from December 4, 1793, to January 16, 1794, Sir William Herschel was led to assign to Saturn a rotation-period of 10 hours 16 minutes (0·44 seconds, but no reliance can be put on this part of Herschel's result).[2] The observations of Schröter seemed to indicate a rotation-period considerably longer; but Herschel's result has been confirmed in recent years by that careful observer Professor Asaph Hall, of Washington, who, observing a white equatorial spot on Saturn from January 7, 1876, to the earlier part of January, 1877, deduced a rotation-period of 10 hours 14 minutes. The description of this spot is interesting as affording evidence of the activity of the processes taking place in Saturn's atmospheric surroundings. The spot was from 2″ to 3″ (say almost 12,000 miles) in diameter, round at first, and brilliantly white, as if an immense mass of white-hot matter had been flung up from the interior, though in all probability the whiteness was not thus due to heat. The spot spread itself towards the east, the bright part remaining near the western extremity of the streak thus formed. The interpretation put on this by Professors Hall and Newcomb was that the luminous matter was, at it were, swept away from the bright nucleus of the spot in the direction of the planet's rotation; but, assuming the luminous matter brought from below, where the rotation is slower, it is more reasonable to say that the brighter portion lagged towards the west, or in a direction contrary to that of the planet's rotation. The rotation-period deduced by Professor Hall was taken from the brightest part of the streak; had the middle of the streak been taken, a shorter rotation-period would have been deduced. Thus, as Herschel from a non-equatorial spot, or rather darkening, deduced a rotation-period of 10 hours 16 minutes, while Hall's equatorial bright spot gave a period of 10 hours 14 minutes, and its middle would have given a shorter period still, we may infer that, as in the case of Jupiter and the sun, the equatorial zone of Saturn has a rotation-period measurably shorter than that of the non-equatorial zones.

[1] The belts observed by Herschel on this occasion were those forming what has been called Herschel's quintuple belt, a name which has led to some misapprehension. What he describes (*Phil. Trans.* 1794, p. 62) was a dark belt close to the equatorial bright belt; and he remarks that the dark belt showing two narrow divisions, the planet appeared to be surrounded by a quintuple belt composed of three dark and two bright belts. The more natural way of describing what he saw would have been to say that Saturn seemed to be surrounded by three dark belts.

[2] A rotation period of 10 hours 29 minutes 16·8 seconds is said in several old treatises to have been Herschel's final determination. There must be some authority for a statement so definite and precise. Professor Newcomb says that 'a suspicious coincidence is that this period agrees with that assigned for the time of the rotation of the ring'—but this is not the case. He also limits the mention of this period to 'nearly all modern writers,' which is also a mistake, if by modern he means 'recent' (as may be supposed, for all writers, since Herschel detected the rotation, are necessarily modern writers). The period 10 hours 29 minutes 16·8 seconds is given in the old *Penny Cyclopædia*, in Rees' *Cyclopædia*, also old, and in Smyth's *Celestial Cycle*, nearly half a century old. I have been unable, however, to trace this period to its source.

We may regard Sir W. Herschel's estimate, based as it was on more than a hundred rotations of Saturn, as the most trustworthy yet obtained.

(1354.) The colours of Saturn's disc are thus described by Mr. Browning, or rather the following pigments are those which he employed to get the nearest approach to the observed tints: 'The rings are yellow ochre shaded with the same and sepia; the globe yellow ochre and brown madder, orange, and purple, shaded with sepia; the pole and the narrow belts situated near it on the globe, pale cobalt blue . . . But there is a muddiness about all terrestrial colours when compared with the colours of objects seen in the skies. These colours could not be seen in all their brilliancy and purity unless we could dip our pencil in the rainbow, and transfer the prismatic tints to our paper.'

(1355.) The spectrum of Saturn resembles Jupiter's. Huggins and Janssen both detected evidence of the presence of aqueous vapour. Vogel detected in addition the same line belonging to the red stars which he had found in Jupiter's spectrum. The atmospheric bands can be noticed also in the spectrum of the rings, but they are not so strong.

(1356.) The arguments used in Arts. 1287 and 1289 with regard to Jupiter apply with equal force to Saturn. Indeed, when we consider his much smaller mean density we find the evidence in his case stronger, even though, owing to his greater distance, details indicating the great depth of his atmospheric surroundings are not so precise. In one respect, however, evidence of this kind in Saturn's case is stronger and more decisive than in Jupiter's. It had been noticed by Schröter and others, though in dealing with Jupiter it was scarcely necessary to mention the circumstance, that occasionally parts of the planet's outline appear flattened, as though a portion of the atmospheric envelope had either been drawn inwards, or (a far more probable interpretation) had passed from the form of non-transparent cloud to perfectly transparent vapour. Now, although even in Jupiter's case these phenomena were too transient and too delicate to be regarded as positive evidence (though what we shall learn about Saturn renders them so), and corresponding phenomena might be regarded as altogether unlikely to be seen in Saturn's case, so much further from us and from the sun, yet the exceptionally small density of Saturn, suggestive of relative youth, and the rings, which seem to show that as yet he has not finished the fashioning of his own system, would seem to render it barely possible that evidence of a fluctuating outline [1] occasionally available in Saturn's case might be even clearer than in the case of the nearer planet.

[1] I am particular to describe the peculiarity now to be considered as evidence of a 'fluctuating outline,' because, with singular pertinacity, one might almost say perversity, those who reject

(1357.) On April 7, 1805, Herschel, who had observed and measured Saturn over and over again, and knew perfectly well the planet's customary regularity of figure, found Saturn, as observed with his 10-feet and 20-feet reflectors, strangely distorted in aspect. On May 5, 1805, in order to have the testimony of all his instruments, he turned the 40-feet reflector upon Saturn. 'I used a magnifying power of 360,' he says, 'and saw its form exactly as I had seen it in the 10- and 20-feet instruments. The planet is flattened at the poles; but the spheroid that would arise from this flattening is modified by some other cause, which I suppose to be the action of the ring. It resembles a parallelogram, one side whereof is parallel to the equatorial, the other to the polar diameter, with the four corners rounded off so as to leave the equatorial and the polar regions flatter than they would be in a regular

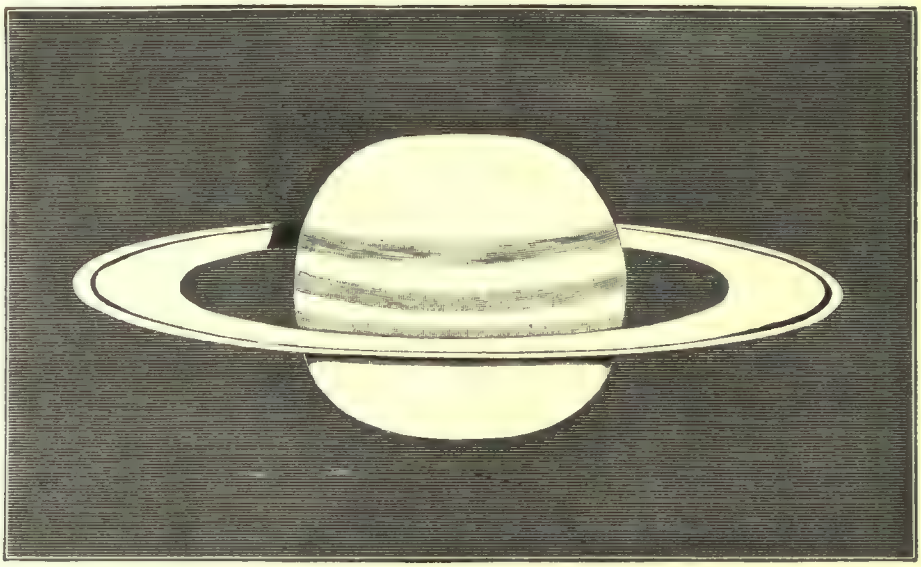


FIG. 413. 'Square-shouldered aspect' of Saturn.

spheroidal figure.' He made the longest equatorial and polar diameters as 36, 35, and 32 the inclination of the longest diameter to the equator being about $43\frac{1}{3}$°. Fig. 413 represents the shape of Saturn as described and measured in 1805 by Herschel; but the ring was somewhat less open than it is shown in fig. 413, for the ring's plane had passed through the sun in June 1803. Nothing could be clearer than that Herschel was not here

the observations in question insist on regarding them as to be tested by subsequent measurements, which is as though the existence of waves on a stormy sea should be denied on the evidence of calm seas subsequently observed. Observations by Bessel, Main, and a number of other measurers are quoted against the observations of William Herschel, greatest of observing astronomers, as though he had claimed that Saturn is nominally irregular in figure, whereas his own observations, both upon and after those indicating irregularity, gave as perfect evidence of regular ellipticity of figure as any made by the best measurers, even by those who, giving all their time to such work, were measurers and nothing more.—R. A. P.

describing the peculiarity of shape he had observed as normal ; he evidently regarded it as abnormal, and even suggests a cause, though not one which can be admitted as possible.

(1358.) In 1806, when, if the square-shouldered aspect had been an illusion due to the ring, the peculiarity should have been more marked, Herschel found that, though Saturn still appeared distorted, the distortion was considerably reduced—the longest equatorial and polar diameters being now as 36, 35·41, and 32. In the summer of 1807, Herschel found the outline of Saturn less regularly distorted ; the northern regions were flattened, the southern bulged outwards.[1] The measurements made by Sir W. Herschel in 1789, agree with those made by him before 1805, in pointing to the degree of ellipticity which the best modern measurements indicate.

(1359.) Bessel, completely misunderstanding the nature of Herschel's observations, as indicating fluctuations of figure—not a persistently abnormal figure—was at the pains to show by precise measurement in 1832, when Saturn presented his ordinary elliptical aspect, that the planet was elliptical, with the degree of flattening which Herschel and others had already sufficiently demonstrated. Similar observations have been repeated, naturally with similar results, by Main and other observers of the same type. But we owe to Sir William Herschel the first sufficient determination of Saturn's normal figure, as well as the discovery that the planet's outline occasionally fluctuates in such sort that instead of the normal ellipse we have abnormal peculiarities. Similar peculiarities have been noted by other observers—in particular by the two Bonds in America, by Coolidge, a careful American observer, by Airy, when Astronomer Royal, and by the regular observers at Greenwich, who report in the Greenwich Records for 1860–61 'from time to time the planet Saturn assumed that year the square-shouldered aspect.' It must be admitted, however, that the occasions when Saturn appears abnormally-shaped are few, and for one observer who has witnessed such peculiarities there are hundreds who have not. But the fortunate few have all been skilful observers and all patient watchers of the planet.

(1360.) Since, then, we cannot reasonably reject the evidence, and there is no possible way of attributing it to illusion, we must seek for an explanation. Fortunately the very circumstance that the observed changes are most remarkable, limits our choice among conceivable explanations, and so makes

[1] It is interesting to note that John Herschel, then only a boy, verified this observation on June 16, 1807, that being his first recorded astronomical observation. Admiral Smyth, who, in his *Celestial Cycle*, gives an imperfect account of the elder Herschel's observations of Saturn's anomalies of figure, describing the 'square-shouldered aspect' as if it were normal, goes on to speak of Hevelius's account of Saturn's apparent changes of figure (Art. 1341), and Huyghens's corrections of them, as if they were in some way connected with Herschel's observations of anomalies in the figure of the planet itself.

the true explanation more obvious. We cannot suppose—even though we could regard the real surface of Saturn as near his apparent surface—that two solid and liquid zones each with a surface many times greater than the whole surface of the Earth had risen and sunk through three or four thousand miles. Such a displacement would indicate a mechanical action, the thermal equivalent of which would have sufficed to make the whole surface of Saturn glow with similar lustre, increasing his brightness many thousand fold. We cannot imagine clouds rising and sinking to such elevations, or forming and dissipating at those elevations, over the enormous areas (hundreds of millions of square miles) necessary to explain the observed change. The only explanation remaining is, that to the depth of several thousand miles the atmospheric surroundings of Saturn are so far variable in condition that they may at one time (and for a season) be transparent, and at another be so far non-transparent that to the observer on Earth, looking as he does through an enormous range of atmospheric matter for each layer at the planet's edge, they limit the range of view, and, in fact, form, at a greater height than usual, the planet's apparent edge, or *vice versâ*, atmospheric regions which had been non-transparent, at least through these long side-ranges—may so far change in condition as to become transparent—or both these changes may occur simultaneously over different zones of the planet. The last is the simplest and most probable way of explaining the more remarkable changes; for by means of it we can halve our estimate of the range in depth of atmospheric matter through which we need suppose such changes to take place.

(1361.) Since we have had decisive evidence of atmospheric matter surrounding Jupiter, such as this explanation requires us to assume in Saturn's case, while Saturn's low mean density, little more than half Jupiter's, permits us to recognise as probable a far greater depth of variable atmospheric matter than Jupiter probably has, it appears to me not only that the fluctuating apparent outline of Saturn thus finds reasonable explanation, but that we might almost by *à priori* reasoning have been led to expect that his figure would be in some degree fluctuating, at least occasionally, and that the spheroidal figure which the measuring astronomers have established corresponds only to the mean position about which the clouds forming the visible outline usually lay.

(1362.) Of subsidiary evidence we have in Saturn's case much less than in Jupiter's. There is, however, one piece of evidence in the case of the ringed planet which is wanting in the case of his giant brother. The shadow of Saturn is thrown on the ring.[1] Its proper form, supposing Saturn a true

[1] It is often called the shadow of the ball on the ring. The capacity of certain observers for taking all that is poetical—to say nothing of what is appropriate—out of their descriptions, is surprising.

spheroid, is that indicated in the second and third figures of Plate XXV. But the shadow is seldom seen thus. In fig. 414, Nos. 1 to 8, are shown several views of the shadow on the rings as seen by Mr. T. G. Elger, who is not only a skilful observer, but one who has given much systematic attention to Saturn. Now, doubtless these views indicate peculiarities of form partly subjective.

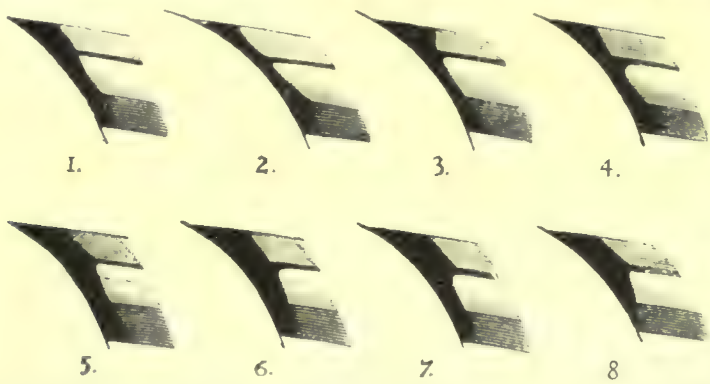


FIG. 414.—Various views of the shadow of Saturn on his rings during the opposition of 1888. (*Elger.*) From the *Monthly Notices* of the R.A.S., 1888.

The way in which the shadow narrows where the rings are brightest may be fairly regarded as due to irradiation; and we may even thus come to regard the figure of the shadow as affording a neat photometric test of the brightness of different parts of the ring. But the irregularities of figure cannot all be thus explained. Nor can they be at all explained, as has been suggested, by peculiarities in the contour of the rings themselves. It has been suggested that they may indicate differences in the transparency of the deep atmospheric surroundings of the planet itself, which are made evident by the transmission of direct sunlight, though they are not recognisable by means of the fainter dispersed sunlight which we receive from the planet's limb.

(1363.) From observations made in 1789, when the ring was turned edgewise to the sun and Earth, Sir William Herschel concluded that the ring rotates in 10 h. 32 m. 15·4 s. It seems likely that Herschel really observed the motion of some group or knot of small satellites, on or near the outer edge of the inner bright ring. He certainly saw several 'lucid protuberances,' or what seemed to him such, travelling along the ring in a manner indicating $10\frac{1}{2}$ hours for the period of circuit.[1] It will appear presently that this period

We have 'port-hole' markings, and 'pipe-bowl' markings in Jupiter, Saturn's 'square-shouldered' aspect, and Saturn's 'ball,' the 'beads' before eclipse, and the like—to show that the average observer cannot rise above what he actually sees, to consider what it actually means, and to be impressed by the thoughts thus suggested.—R. A. P.

[1] Schröter and Harding, at the next disappearance of the ring, saw lucid protuberances, which were unchanging in position. Why Professor Grant should consider these results in direct contradiction to Herschel's, and, therefore, to the theory of rotation, he has not explained. Bond, of Cambridge, Mass., from similar observations, raised a similar objection. And the fact that W. Struve always found the eastern side of the ring-system farther from the planet than the western, has been urged as equally an objection to

belongs only to a part of the ring-system, somewhat within the outer edge of the inner bright ring, not to that system regarded as a whole.

(1364.) Other divisions besides Cassini's have been detected in the two chief rings of Saturn. One of these, shown in the second and third views of Plate XXV., has been called Encke's, because he first called special attention to it in 1837. Short, in the last century, Kater, Jacob, Dawes, Lassell, Quetelet, Secchi, and others in the earlier part of the present century, and many other observers later, have detected not only a well-marked division in the outer ring, but several other divisions both in that ring and the broader inner ring. These lines, as described by different observers, appear to vary in position and strength, a circumstance which may be partly accounted for by differences in the telescopes employed, and in the conditions of observation. I do not think any useful purpose would be served by noting all the multitudinous observations of divisions, changes in the divisions, disappearance of division-traces, and so forth, which industrious observers of the last half-century have noted. It results from a consideration of the whole array of evidence. that many divisions, most of them exceedingly fine, appear from time to time in different parts of both the bright rings, one of which, in or near the middle of the outer ring, appears to be permanent so far as existence is concerned, but it seems variable in position and in breadth. The inner part of the inner bright ring appears to be traversed by many concentric divisions not separately visible, giving rise (owing to their more obvious character near the ends of the longer axis of the ellipses into which they are foreshortened) to the peculiar darkening or shading which will be noticed in the second and third figures of Plate XXV., at the inner edges of the bright rings, where these edges attain their greatest apparent distance from Saturn's globe. (A little consideration will show that this is a natural result of the foreshortening of multitudinous concentric divisions not separately discernible.) Notches, dark markings, as well as bright spots have been also noticed on the inner edge of both the chief bright rings.

(1365.) But all these peculiarities in the ring-system are surpassed in interest by the dark ring inside the inner light ring discovered in 1850, first on November 11 by Professor G. P. Bond, then independently (before the

Herschel's observed and measured rotation. In reality it is obvious that, while Herschel observed a feature which did move in such a way as to indicate rotation, Schröter, Harding, and Bond were observing some such features as the inside corners of the several rings, when turned nearly edgewise to the observers, so that the variations of brightness, which they noticed, did not shift in position. As for W. Struve's observations, the appearance of a greater extension of the ring-system on the eastern side, upon every occasion when Struve observed the planet, must obviously be regarded as merely accidental—a strange chance, perhaps, but, strange or not, necessarily a chance—seeing that no conceivable physical condition could maintain such a relation, whether the rings rotated or not.

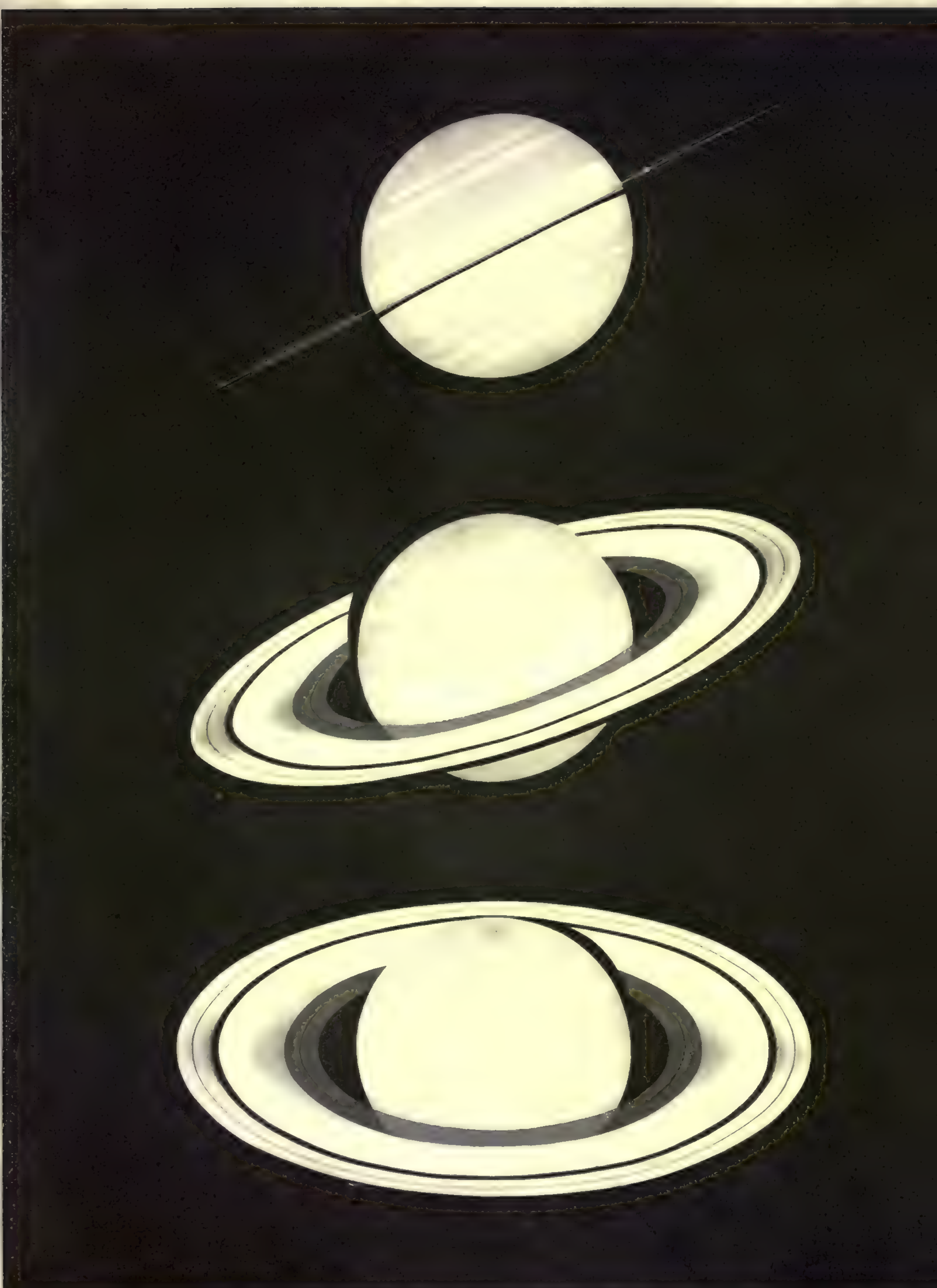


Fig 1 SATURN AND HIS RINGS FEBRUARY 2nd 1862
Fig 2 SATURN AND HIS RINGS NOVEMBER 3rd 1858
Fig 3 SATURN AND HIS RINGS MARCH 25th 1856
AS SEEN IN AN INVERTING TELESCOPE

news of Bond's discovery had reached England) by Mr. Dawes on November 29. Mr. Lassell, while on a visit to Mr. Dawes, saw the dark inner ring when his attention was directed to it; but it is quite incorrect to associate his name with the discovery.[1] The general character of the ring may be recognised from the second and third figures of Plate XXV., and from fig. 415. But the distinction between the dark ring and the inner edge of the neighbouring bright ring is sometimes much more marked than in fig. 415, or even than in Plate XXV., while at other times it is scarcely to be recognised at all, the

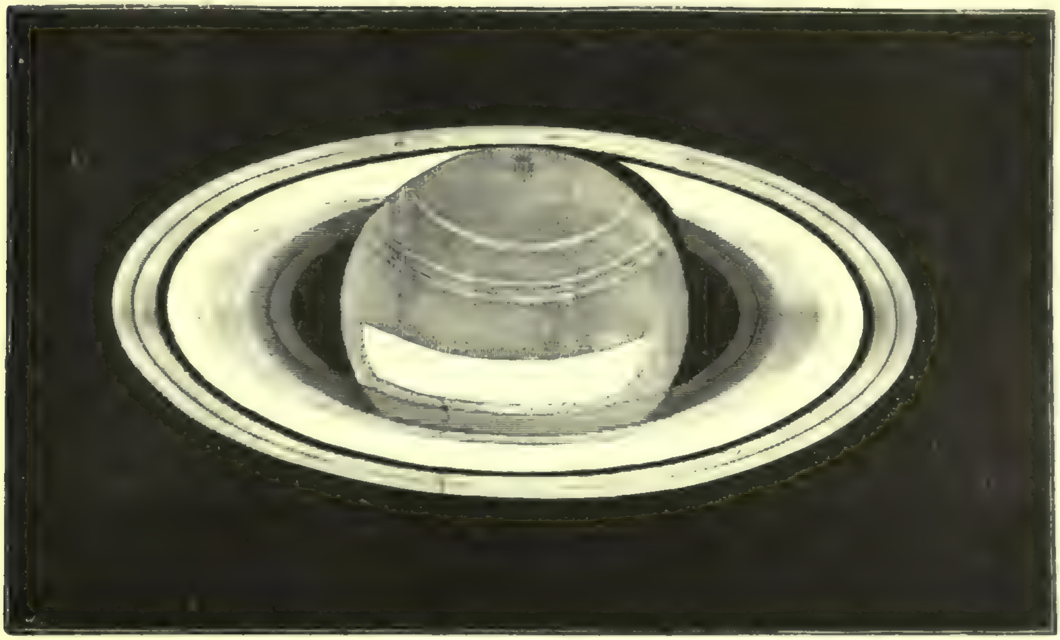


FIG. 415.—Saturn and his system of Rings, at their greatest opening in 1885. (*Proctor.*)

bright inner ring merging into the dark ring after the manner shown in fig. 416, which presents the planet as drawn by Mr. Ranyard with an 18-inch silver on glass reflector at the beginning of 1884. It will be observed that in this picture there is no trace either of the outer division shown in fig. 415, or of a division which has been frequently seen between the inner bright ring and the dark ring. It will be observed also that whereas in Plate XXV. and fig. 415 the outline of the planet can be traced through the dark ring, in fig. 416 it is only just traceable. The continuous aspect and the absence of transparency

[1] The dark ring was seen and measured by Galle at Berlin in 1838. Possibly he ought to be regarded as the real discoverer, since it was only through the neglect of Encke, the director of the Berlin Observatory, that Galle's observations were shelved. The astronomers of the Roman Observatory, ten years earlier, saw the dark ring, both outside and across the planet, but carelessly omitted to report their observations. Sir William Herschel saw nothing of it, and Schröter particularly noted, in 1796, the absolute blackness of the spaces between the globe and the inner bright ring.

in the ring seem to be variable, and to correspond with what we should expect from the diminished opening of the rings. When the ring-system is well open the dark ring is almost purplish in tone, and generally strongly contrasted with the neighbouring bright ring, though the inner edge of that ring is always much darker than the outer. At such times, also, the dark ring, where it crosses the planet, is distinctly transparent. Its aspect has been

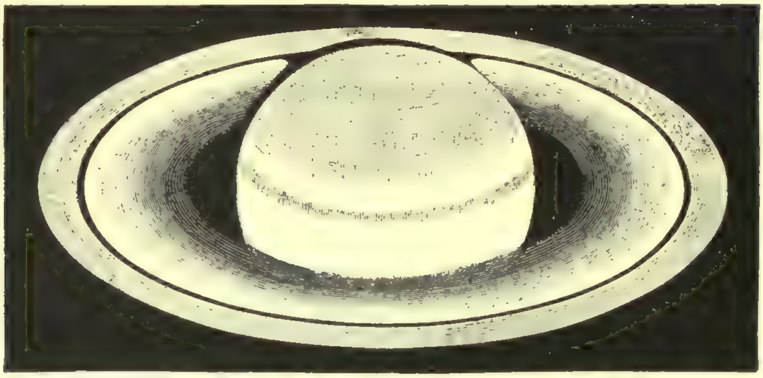
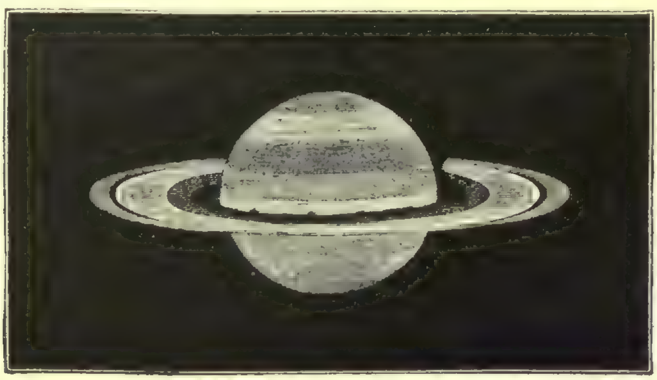


FIG. 416.—Saturn's system as seen at the beginning of 1884. (*Ranyard.*) From the *Monthly Notices* of the R.A.S., 1884.

compared to that of a crape veil. But as the ring-system closes up, the distinction between the dark ring and the neighbouring bright ring becomes less marked, the dark ring appears greyish or slate-coloured, the traces of division less distinct (or less frequently to be noticed), while the outline of the planet is either not seen at all through the dark ring, or only seen with difficulty and indistinctly.

(1366.) But apart from changes due to changes in the angle at which

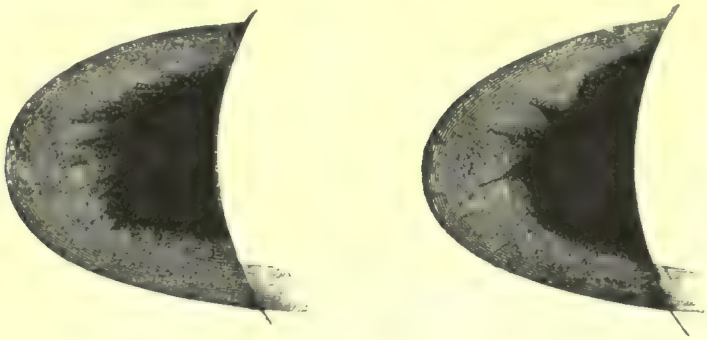


1861: April 7. (*De La Rue.*)

FIG. 417.—Saturn, April 11, 1861. (*De la Rue.*) From the *Monthly Notices* for 1861.

the ring-system is viewed, the dark ring undoubtedly undergoes remarkable changes which cannot be so explained. Divisions form and disappear within

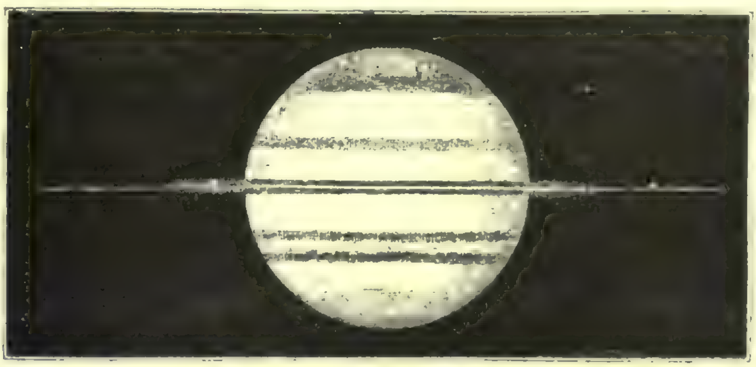
short periods of time; the colour and brightness of the dark ring changes markedly even from night to night. At times it is uniform in tint, at others manifestly mottled; and occasionally irregularities of the most marked character can be recognised in its outline. Mr. De la Rue has seen dark projections from its inner edge upon the planet's face, as shown in fig. 417. The drawing here, however, is not quite correct, the rings being too open;



I. March 21. II. March 27.

FIG. 418. Preceding Ansa of dark ring. (*Elger.*) From the *Monthly Notices* for 1888.

and the irregularities apparently on the inner side of the dark ring may be shadows of irregularities on the inner bright ring. Such irregularities as are shown in Mr. Elger's drawings, in fig. 418, are unmistakable however. Such peculiarities have been seen by other observers—round the inner edge of the dark ring always near the extremities of the long or apparent axis. When the ring is turned edgewise a slight nebulosity has been noticed by Mr. Wray

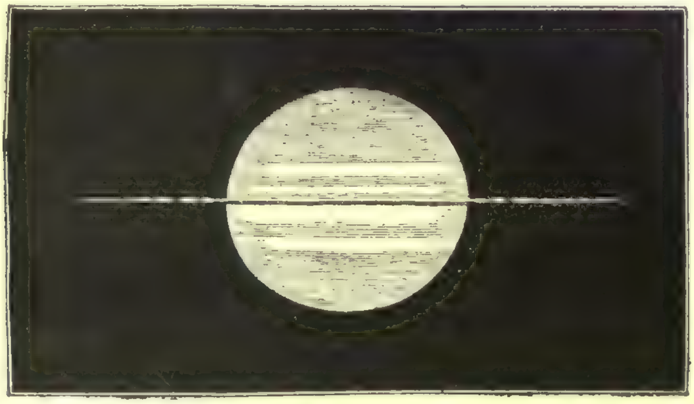


1861 : Dec. 26. (*Wray.*)

FIG. 419.—Saturn, December 26, 1861. (*Wray.*) From the *Monthly Notices* for 1863.

(fig. 419) on each side of the planet to about the distance occupied by the dark ring. Usually, however, the ring turned edgewise either disappears, or has an irregular appearance, as shown in fig. 420. In some telescopes the ring never disappears even when the Earth is on the dark side, a faint

brownish or purplish light still showing the ring's place. Fig. 421, showing Saturn self-photographed, though very rough, is interesting as showing the relative brightness of the outer and inner rings and of the planet's disc.



1861 : Nov. 12. (*Jacob.*)

FIG. 420.—Ring turned edgewise. (*Jacob.*) From the *Monthly Notices* for 1862.

(1367.) Saturn's rings present many problems of interest—geometrical problems arising from the various positions they assume with reference to the sun and Earth, and to different parts of the planet's own surface; dynamical problems (belonging chiefly to the higher mathematics), arising from the analysis of their movements and stability; and physical problems, arising from the consideration of their probable origin, development, and condition.

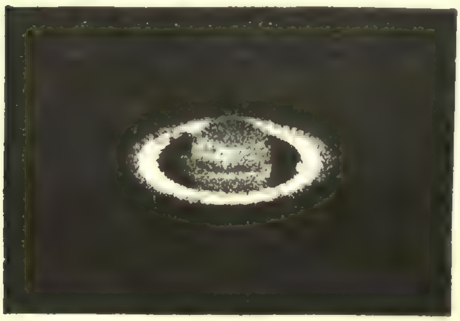


FIG. 421. Photograph of Saturn. (*Bros. Henry.*) Process block untouched by engraver.

(1368.) I have dealt very fully with problems of the first sort in two chapters of my 'Saturn and its System,' one on 'Periodic changes in the appearance of Saturn's system,' the other on the 'Habitability of Saturn.' Several tables also in that work and explanatory notes in Appendix II. relate to these matters. Here I can only deal briefly with such problems.

First, with regard to the aspect of the ring system. Since Saturn travels round his orbit with his axis inclined 26° 49½′ (from uprightness) to the plane of his motion, the axis remaining appreciably parallel to itself throughout the circuit, the axial presentation of his globe to the sun varies in the same way as the Earth's (illustrated in Plates XVIII. to XXI.), or as Mars's (illustrated in Plate XXIV.), and in much the same degree—rather more than the Earth's, slightly less than that of Mars. The outlines of the rings vary

in aspect in precisely the same way as the equator and latitude parallels. The period during which all the changes are passed through is 29 years 167·2 days, during about half of which one side of the ring-system is illuminated by the sun, the other side being illuminated during the other half. It is only necessary to study Plate XXV., remembering that the change from the uppermost aspect, corresponding to a Saturnian equinox, to the lowermost requires 7 years 4⅓ months of our time, the return to the former aspect (but the slant of the ring the other way) the same period, the apparent passage of the rings towards the other pole (the uppermost in the figures of Plate XXV.) the same time, and the final return to the uppermost aspect in Plate XXV. yet 7⅓ years more. We perceive that, were Saturn a world like our own in other respects, the conditions of habitability would be seriously affected by the concealment of large zones of the planet from the sun's rays during long periods of time; this happening, too, always in those parts where the planet's winter months were passing. In the sense in which we speak of an eclipse of the sun, it may be said that the sun is always eclipsed on Saturn; considering either hemisphere, we may say (as Sir John Herschel expressed it) that the sun is eclipsed for 14 years 8⅔ months at a stretch. Dr. Lardner objected (apparently misunderstanding the sense in which Herschel said this) that the eclipse does not continue so long as this at any place on Saturn. But he mistook the actual relations, and not only underrated the duration of the eclipses, but was led to suppose that, even where eclipses might continue during long periods, they were not continuous, the sun being visible in the morning and evening if eclipsed at mid-day, or visible at mid-day if eclipsed in the morning and evening. As a matter of fact, there are zones of Saturn where the winter eclipses last for several years of our time, being total during a large proportion of the interval. In my 'Saturn and its System' I have given a full table of the duration of eclipses of all kinds—morning and evening, total, and mid-day—for all latitudes on Saturn. The following extracts will suffice here, since there is but little space for details of the sort:—

In Saturnian latitude 15°, north and south, the autumnal equinox is followed by 1 year 29 days of our time without eclipse; then follows a period of 227 days during which the sun is eclipsed in the morning and evening, 129 days of total eclipse throughout the day, and then 2 years 180 days of mid-day eclipses. In latitude 30° the corresponding periods are 2 years 70 days without eclipse, 1 year 4·2 days of morning and evening eclipses, 1 year 170 days of total eclipses, 2 years 255 days of mid-day eclipses. In latitude 45° there are 3 years 130 days without eclipse, 1 year 13 days of morning and evening eclipses, then 2 years 350 days of total eclipse lasting right on to the winter solstice, and therefore followed by the same period of

total eclipses in the following quarter of the Saturnian year, or 5 years 347 days of our time without any sign whatever of the sun except such glimpses as might be obtained of him through the ring.

As viewed from the Earth the changes of aspect of Saturn and his system are very similar to those which would be seen from the sun, because the distance of the Earth from the sun is small compared with Saturn's. Although the plane of Saturn's rings is inclined $1\frac{1}{3}°$ more to the plane of the Earth's path than to that of his own orbit, the former inclinations being nearly $28°$ $11\frac{1}{3}'$, the latter only $26°$ $49\frac{1}{2}'$, this does not (as has been mistakenly asserted), affect the aspect of Saturn in corresponding degree, the range of the Earth north and south of the plane of Saturn's orbit being small because of the comparative smallness of the range of the Earth from the sun. The maximum inclination at which we view the Saturnian rings from the Earth is about $27\frac{1}{4}°$. More important differences between the solar and terrestrial aspect of the rings arise when near the Saturnian equinoxes, when the edge of the ring is turned towards the sun ; whereas, at this time the plane of the ring passes but once through the sun, it may pass three times through the Earth, because of her motion around the sun.[1]

As regards the appearance of the Saturnian rings from different parts of the planet, the imagined stories of the Saturnian skies on account of the rings have no real existence, and could have none even if we could suppose the surface of Saturn, as we see it, to be the real surface of the planet's globe and the abode of intelligent living creatures. From large parts of the planet the rings could not be seen at all ; the outer edge of the outer ring would just be visible in Saturnian latitude 66° 25′ north and south, the inner edge of the outer ring in latitude 62° 59′ ; the outer edge of the inner ring in latitude 62° 16′ ; the inner edge of the inner ring in latitude 51° 13′ ; and the inner edge of the dark ring in latitude 40° 45′. But an immense breadth of the part of the ring highest above the horizon would be in shadow during the midnight hours,—at least, during the half Saturnian year when the sun's light illuminated the ring ; during the other half of the Saturnian year the rings would not be illuminated at all. The zone of glorious light in which the shadow of the planet, like the hand of a mighty dial, sweeps majestically onwards, marking the hours of the night, is a purely imaginary conception.

[1] It is sometimes stated that the plane of the rings may pass once, twice, or thrice, through the Earth. It should be obvious, however, that passing twice through the Earth leaves the Earth on the same side of the ring's plane as before the first passage, whereas the single passage, or the set of passages, must carry the plane of the rings not only athwart the sun but athwart the Earth's orbit, and if before the passage the Earth was on the northern side of the ring's plane, and had been so for more than fourteen years, after the passage she must be on the southern side (and remain so for a similar time), and *vice versâ*.

The mathematical inquiry into the movements of the Saturnian ring-system, regarded as a problem in dynamics, was begun by Laplace towards the end of the last century. He proceeded on the assumption, very natural in his day, that the rings are continuous solid bodies. His analysis was a masterpiece of mathematical skill, which was justly accepted as conclusive, so long as the initial assumption of the solidity of the ring-system remained unquestioned.[1]

Laplace showed, first, that a ring around such a planet as Saturn must rotate in its own plane, or it would inevitably give way under the attractions to which it would be subjected, and, yielding like an arch under a weight too great for its strength, would fall in ruins around the planet's equator. He showed, further, that a broad ring could not be preserved by rotation, because either the outer parts would move too quickly, and, breaking off, would fly outwards owing to excess of centrifugal tendency, or the inner parts would move too slowly, and, being broken off, would be drawn inwards towards the planet's surface. Hence Laplace concluded that Saturn's ring must be divided into several concentric rings. He showed, next, that a perfectly uniform solid ring, of moderate breadth, might rotate for ever around a perfectly uniform spheroidal planet, if subject to no disturbing influences; but that if such a ring were once disturbed, equilibrium would never be restored, and the ring must ere long be destroyed. Since the perfect uniformity and freedom from disturbance necessary for equilibrium unquestionably do not exist, it follows that the equilibrium of the Saturnian rings must, in some way, be rendered stable. Laplace suggested (but this part of his work was not completed by exact mathematical investigation) that if each of the narrow solid rings into which he supposed the system divided were non-uniform (weighted, as it were) the irregularity, properly disposed and combined with eccentricity of position, might prevent the destruction of the ring. He supposed that each ring being thus separately kept in equilibrium, the whole system might be preserved.

[1] There was no mere deference in this to a deservedly distinguished name, as there was (and has been even until now) in the unquestioning acceptance given by many, who are competent to form an independent opinion, to the results of those speculations which Laplace embodied in his famous but impossible nebular theory. The results to which Laplace was led by his inquiry into the stability of the Saturnian ring-system were based on sound and profound mathematical researches. They were not hypothetical at all, save as regards the initial assumption; but, if that assumption (which few saw reason to question) were correct, they were demonstrated facts. Yet, whereas these results, which still remain among the most interesting products of mathematical analysis known, have been given up, perforce, because observation and physical theory show that they are inconsistent with fact and even with possibilities, the mere fancies shadowed forth in the nebular hypothesis continue to be spoken of as demonstrated truths, merely because they were suggested by Laplace, although they were never made the subject of any mathematical inquiry at all, and are demonstrably inconsistent with physical laws (unknown to Laplace) now established, and known and accepted by all students of physical science.

It was supposed that Sir W. Herschel's recognition of an occasional eccentricity in the position of the ring-system, and his discovery of the system's rotation, confirmed in a striking manner Laplace's conclusions. And because these conclusions *were* Laplace's, they continued to be accepted, none noting how artificial and improbable the arrangement was which he had suggested as possibly preserving the equilibrium of the system, or how ill his conclusions agreed with the generally uniform appearance of the rings.[1]

It was not till the year 1851[2] that the problem thus far dealt with by Laplace was again taken in hand. The discovery of the dark ring and suspected changes in the ring-system led to the suspicion that the rings might be fluid, or even that they might not be continuous bodies at all. Professor Pierce, of Harvard College, showed that solid rings must be much narrower than Laplace had supposed; a result which went far to render Laplace's general conclusions improbable. The stability of the ring-system was chosen as the subject of a prize Essay at the University of Cambridge in March 1855, and in 1857 the prize was adjudged to Mr. (afterwards Professor) J. Clerk Maxwell. Taking up the question where Laplace had left it, Maxwell showed that the irregularity and eccentricity of each ring, required for stability, would be much greater than Laplace had suspected; and that even with rings most eccentrically weighted, disturbances much slighter than those which actually affect the system would destroy the adjustment of the weight, and with it the stability of the ring. It seems, then, suddenly to have dawned on Maxwell that the idea of solid flat rings on such a scale as Saturn's, and exposed to such energetic attractions, is entirely inconsistent with physical laws, not known to Laplace, but well known when Maxwell began his researches. Whereas he might have started out with the assurance that the rings cannot be solid, for such rings, even of the hardest steel,

[1] In such cases the more powerful mathematicians and physicists, and those of the weaker sort, agree, though from different motives, in keeping up the theory which has had the support of a name deservedly eminent. The stronger object to any inquiry or doubts which might seem to suggest that a powerful mathematician, applying profound mathematical processes, had failed to deduce just conclusions; the weaker, unable to form an independent opinion, deem it safest to stand by the opinions of those whom they know to be able and well informed. Luckily, in the case of Saturn's ring-system, observation compelled renewed mathematical inquiry. If ever proper inquiry is made into Laplace's nebular hypothesis, that also will be still more decisively rejected.

[2] Prof. George Darwin has recently drawn attention to the fact that M. Edouard Roche, Professor of Mathematics at Montpellier, successfully attacked the problem in the year 1848. His memoirs on the subject were published in the *Transactions of the Montpellier Academy of Sciences* for 1849, and have only recently become known in England. Prof. Clerk Maxwell evidently knew nothing of them. An important result obtained by Roche may be stated thus: A satellite cannot travel round its primary at a distance of less than $2\frac{11}{25}$ of the planet's radius from the planet's centre. Within this distance the tidal action of the planet upon the satellite will break it up. Assuming Saturn's real diameter to correspond to his visible diameter, Saturn's rings are within Roche's limit, and they must consequently consist of masses so small that they cannot be further torn to pieces by the tide-generating force of the planet.—A. C. R.

would be plastic, and, to all intents and purposes fluid, under the forces to which they would be subject, he suddenly remembered this at the close of a laborious inquiry which had led him to the same conclusion by a circuitous and very difficult route.

From this point onwards, however, Maxwell's researches were conducted on sound principles and worthy of all praise. He dealt with the theory that the rings may be fluid, and showed that were they so the system would soon be broken up by the waves resulting from the various perturbing forces to which the rings would be subject. Since, then, regarded either as solid or as fluid, the rings must necessarily be broken up into discrete masses, it remains only that they should be regarded as formed of such masses, minute bodies, solid or liquid, or both, with or without vaporous surroundings, travelling in flights, like sands on the sea-shore for multitude, around the central mass of Saturn. Maxwell showed how the movements of such bodies would go on, practically according to the same laws which exist within the solar system (and may specially be recognised within the zone of asteroids) save for the occurrence of occasional collisions among the minute satellites, and the consequent loss of a certain minute portion of the *vis viva* of the system. He further showed that, as a result of such disturbances taking place within the Saturnian ring-system, there would be a tendency to the widening of the several rings, and to the formation of new rings, consisting, however, but of widely-strewn satellites, within those already existing.

One peculiarity of the Saturnian ring-system Maxwell failed to recognise. The great division discovered by the elder Cassini, and the other less remarkable, probably less constant, divisions, might of course exist in any such system of multitudinous small satellites as a mere result of casual relations. But as the features of the solar system are more and more carefully studied, the clearer it becomes that all the more definite characteristics have resulted from the operation of processes continuing according to determined laws during immense periods of time. Thus the study of such features is nearly always rewarded by the recognition, in the first place, of the nature of the processes which have been at work, and, in the second place, of the conditions out of which such processes have produced the existing state of things. The existence, then, of these well-marked gaps, or relatively vacant spaces in the Saturnian ring-system, though it seemed to Maxwell scarcely worth noticing, is a point well worth careful study in the light of those laws of evolution which have since become almost axiomatic in science.

What we have learned already from the zone of asteroids, suggests independently an inquiry whether in the zones of small bodies which we have

now learned to recognise in the Saturnian system similar characteristics may not be found. Since we cannot discern individual members of these rings of multitudinous small satellites, we cannot hope to detect evidence of eccentricity in their orbital motions. Evidence that some of them move (for a time, at least) in orbits inclined to the general plane of the rings, we may perhaps hope to find ; because, when the ring-system is turned edgewise to the Earth, we may, perhaps, recognise the satellites on either side of the median plane of the system under the aspect of a faint cloudiness bordering the bright line into which the ring is for the time projected. This phenomenon (Art. 1366) is actually presented, and must doubtless be thus explained, since it is unlikely that any vaporous surroundings, if such exist, would extend visibly so far from the plane of the rings as the faint nebulous light, seen in the first figure of Plate XXV. But the feature of the asteroidal ring, the analogue of which we have the fairest chance of recognising in the Saturnian rings, is the absence of bodies moving (for the time) at such mean distances as would make their periods synchronise with the motions of Jupiter. There is indeed no Jupiter to disturb the Saturnian ring-system. Titan, the Jupiter of Saturn's satellite-family, is too far from the rings to effectively disturb their motions ; moreover, his period (15 days $22\frac{2}{3}$ hours) exceeds the period of a satellite moving round the outside of the outer ring (less than 15 hours) too greatly for any such synchronism as would alone be effective in disturbing the movements of a member of the ring-system. But Mimas, the innermost satellite, though much smaller, is so near to the rings that its perturbing action, despite its small mass, cannot but be most effective ; and where a simple relation of synchronism exists, Mimas's disturbing action, renewed repeatedly (and after very short intervals) opposite one or two parts of the orbit of one of the minute ring-satellites, would compel such satellite to move in a period no longer synchronising with the periodic motion of Mimas. Now this would last for a time only, and not a very long time, if the satellite thus considered travelled alone ; but we have only to consider the aspect of the Saturnian rings to see that that the region which they occupy is crowded as no other portion of the solar system is crowded in which bodies travel freely. A minute satellite leaving its proper zone must soon come into conflict[1] with one or other of the multitudinous satellites travelling in neighbouring zones.

[1] Clerk Maxwell used to describe the matter of the rings as a shower of brickbats, amongst which there would inevitably be continual collisions taking place. The theoretical result of such impacts would be a spreading both inwards and outwards of the ring. The outward spreading will in time carry the meteorites beyond Roche's limit, where in all probability they will, as Prof. Darwin suggests, slowly aggregate, and a minute satellite will be formed. The inward spreading will in time carry the meteorites at the inner edge of the ring into the atmosphere of the planet, where they will become incandescent and disappear as meteorites do in our atmosphere.—A. C. R.

Now, as soon as this is noted we perceive that, in the first place, the Saturnian rings demonstrably consisting of multitudes of minute bodies, there must be gaps in the Saturnian rings where the movements of these satellites synchronise with the movement of Mimas, and that, in the second place, the existence of gaps at distances corresponding to such synchronism would of itself suffice to prove the structure of the Saturnian ring-system to be such as other considerations have shown it to be. Thus we apply the test with antecedent certainty that the result will be to show the great gap, at any rate in the Saturnian rings, to be a product of perturbations renewed again and again on account of the synchronism of the satellites' movements with those of Mimas.

Taking half the period of Mimas (22h. 37m. 28s.), or 11h. 18m. 44s., we find that the distance from Saturn's centre, at which members of the ring would move in this period, is 73,535 miles. As we expected—as, in fact, we *knew* beforehand that it must be—the ring shows a gap at this distance, the inner edge of the outer ring having a radius of 74,740 miles, the outer edge of the inner ring having a radius of 73,035 miles, according to the best measurements, accepted (by myself in 'Saturn and its System') long before this particular relation was thought of or anticipated. Take, next, one-third the period of Mimas, or 7h. 32½m.: the distance from Saturn's centre at which members of the ring would move in this period is 56,018 miles, corresponding, within the limits of errors of observation, to the inner edge of the bright ring.

(1368.) A recent observation made by Prof. Barnard shows that the crape ring gradually increases in density from its inner edge to its outer margin, where it merges into the inner bright ring. Prof. Barnard observed, with the 12-inch refractor of the Lick Observatory on the morning of November 2, 1889,[1] the satellite Japetus as it passed through the shadow of the ball

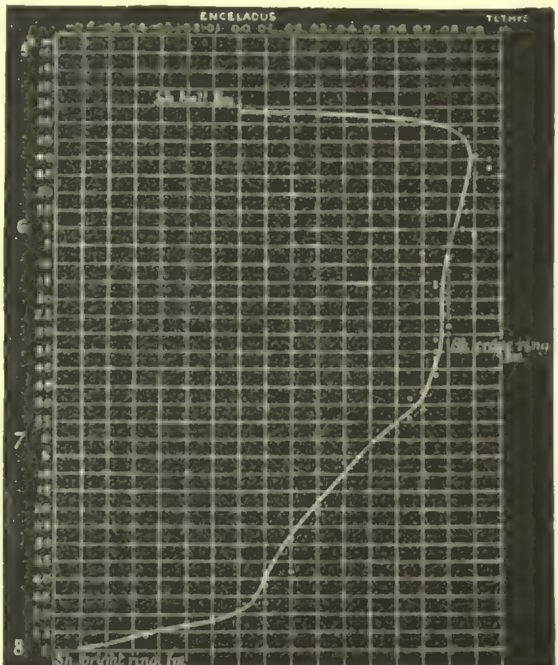


FIG. 122. Light curve showing the brightness of Japetus in the shadows of the globe, crape ring and bright ring of Saturn, Nov. 1, 1889. The vertical scale corresponds to time, the horizontal to brightness.

[1] *Monthly Notices* for January 1890.

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of the planet into the sunlight, shining between the ball and the rings, and then entered the shadow of the crape ring. On emerging from the shadow of the ball, in which it had been entirely lost to view, it rapidly increased in brightness, remained at nearly its maximum brightness for about an hour, and then, on entering the shadow of the crape ring, gradually decreased in brightness till its light was reduced to less than half. It then commenced to enter the shadow of the inner edge of the bright ring, where it was again entirely lost to view, the bright ring being evidently as opaque to the penetration of the sun's rays as the globe of Saturn.

The accompanying curve was drawn by Prof. Barnard to show the brightness of Japetus during its passage behind the crape ring as compared with that of Enceladus and Tethys, which, according to Prof. Pickering, differed in brightness by about one magnitude. If we assume the diameter of Japetus to be about 3,700 miles, it would, owing to its slow motion in its orbit about Saturn, take about half an hour to pass through its own diameter; the rapid diminution in brightness during the last half hour is therefore probably affected by the gradual eclipse of the disc of the satellite, but the gradual decrease in brightness during the previous hour shows that the crape ring is partially opaque, and is at its densest part capable of reducing the sun's light to less than one half.

It has been suggested by those who are inclined to believe in the meteoric theory of nebulæ, that impacts between the stones, or meteoric bodies forming the rings of Saturn, would probably give rise to incandescent bright lines in the spectrum of the ring. Dr. Huggins has, however, made several photographs of the spectrum of the rings,[1] the slit being placed across the ansæ so as to give distinct spectra of the body of the planet and the various parts of the ring; but though the solar lines are well seen in these spectra from about F to N, he has been unable to detect any other bright or dark lines.

[1] *Monthly Notices* for June 1889, p. 404.

CHAPTER XV.

THE PLANET URANUS.

(1369.) From Saturn's orbit to the orbit of the next planet, Uranus, the space is absolutely greater than the whole distance separating Saturn from the sun. In other words, the distance is less from the sun to the orbit of the sixth planet than from the orbit of the sixth planet to that of the seventh. Relatively this is the greatest range of distance between any neighbouring pair of planets in the solar system, though absolutely the space we shall have to travel in passing from the seventh planet to the eighth is greater still. In no other instance is the distance traversed in passing from one planet to the next double the distance of the inner planet.

Uranus is the third of the giant planets in distance, but only fourth in the order of mass and size. He travels round the sun in a period of 84 years 6·5 days at a mean distance of 1,779,834,000 miles; but as the eccentricity of his orbit is 0·046, his greatest mean and least distances from the sun are as the numbers 1046, 1000, and 954, or roughly as 23, 22. and 21. Calling the Earth's mean distance 1, Uranus's mean distance is 19·18338. Plate X. shows the position of all the characteristic points of Uranus's orbit, as well as the amount of his motion above and below; that is, north and south of the plane of the Earth's orbit, in the manner already explained in dealing with Jupiter and Saturn. The student will find it a useful exercise to construct a picture of the orbits of the Earth and Uranus in the same manner as in fig. 374, Art. 1267 for Jupiter, and in fig. 407, Art. 1339 for Saturn.

(1370.) Uranus has a globe which is doubtless flattened at the poles like the globes of his brother giants Jupiter and Saturn; but the evidence on this point cannot be considered as quite conclusive (see Art. 1378). His greatest diameter is about 32,000 miles, though the measurements thus far made are not very accordant. The evidence as to his axial rotation from the observation of spots and markings is also very discordant. In the telescope he appears as a greenish disc of about four seconds in diameter. Roughly

speaking, he compares in bulk with the Earth as the Earth does with our moon. His diameter is about four times that of the Earth, and his volume about 66 times greater than that of the Earth. The mass of Uranus is only 14·4 times that of the Earth, so that his density is only about ·2212 of the Earth's density; that is, he is about $1\frac{1}{4}$ times as heavy as the same bulk of water. Bodies at his surface, if the real surface of the planet corresponds to the disc we see, would weigh about nine-tenths of their weight at the Earth's surface.

(1371.) The question has been raised whether Uranus was known to the astronomers of old times. There is nothing altogether improbable in the supposition that in countries where the skies are unusually clear, the planet might have been detected by its motions. Even in our latitude Uranus can be quite readily seen on clear and moonless nights, when favourably situated. He shines at such times as a star of between the fifth and sixth magnitudes—that is, somewhat more brightly than the faintest stars visible to the naked eye. In the clear skies of more southerly latitudes he would appear a sufficiently conspicuous object, though, of course, it would be wholly impossible for even the most keen-sighted observer to recognise any difference between the aspect of the planet and that of a star of equal brightness. The steadiness of the light of Saturn causes this planet to present a very marked contrast with the first magnitude stars whose lustre nearly equals his own. But although the stars of the lower orders of magnitude scintillate like the leading orbs, their scintillations are not equally distinguishable by the unaided eye. Nor is it unlikely that if Uranus were carefully watched (without telescopic aid) he would appear to scintillate slightly. Uranus would only be recognisable as a planet by his movements. There seems little reason for doubting, however, that even the motions of so faint a star might have been recognised by some of the ancient astronomers, whose chief occupation consisted in the actual study of the star groups. We might thus understand the Burmese tradition that there are eight planets, the sun, the moon, Mercury, Venus, Jupiter, and Saturn, and another named Ráhu which is invisible. If Uranus was actually discovered by ancient astronomers, it seems far from unlikely that the planet was only discovered to be lost again, and perhaps within a very short time. For if anything positive had been learned respecting the revolution of this distant orb, the same tradition which recorded the discovery of the planet would probably have recorded the nature of its apparent motions.

Be this as it may, we need by no means accept the opinion of Buchanan, that if the Burmese tradition relates to Uranus, Sir William Herschel must be 'stripped of his honours.' The rediscovery of a lost planet, especially of

one which had remained concealed for so many centuries, must be regarded as at least as interesting as the discovery of a planet altogether unknown. Nor was there any circumstance in the actual discovery of Uranus which would lose its interest, even though we accepted quite certainly the conclusion that the Herschelian planet was no other than old Ráhu.[1]

(1372.) Herschel's narrative of his detection of Uranus is in many respects instructive. He had conceived the idea of measuring the distances of the nearer stars by noting whether as the Earth circles around the sun the relative positions of stars lying very close to each other seemed to vary in any degree. To this end he was searching the heavens for double stars, most of which were in his day supposed not to be physically associated together—but seen near together only because lying nearly in the same direction, the brighter star of a pair being very much nearer than the fainter. Herschel was seeking for cases of this kind when on Tuesday, March 13, 1781, as he writes, between ten and eleven in the evening, while examining the small stars in the neighbourhood of Eta in Gemini, he perceived one that appeared visibly larger than the rest. 'Being struck with its uncommon magnitude,' he says, 'I compared it to Eta and the small stars in the quartile between Auriga and Gemini, and, finding it so much larger than either of them, suspected it to be a comet.' He was then engaged in a series of observations requiring very high powers, and had ready at hand magnifiers of 227, 660, 932, 1536, 2010, &c. all of which he had successfully used. 'The power I had on,' he proceeds, 'when I first saw the (supposed) comet was 227. From experience I knew that the diameters of the fixed stars are not proportionally magnified with higher powers, as those of the planets are; therefore I now put on the powers of 660 and 932, and found the diameter of the comet increased in proportion to the power, as it ought to be on the supposition of its not being a fixed star, while the diameters of the stars to which I compared it were not increased in the same ratio. Moreover, the comet, being magnified much beyond what its light would admit of, appeared hazy and ill-defined with these great powers, while the stars presented that lustre and distinctness which from many thousand observations I knew

[1] It is, after all, at least as likely that Ráhu—assuming there really was a planet known under this name—might have been Vesta, the brightest of the small planets which circle between Mars and Jupiter, as the distant and slow-moving Uranus. For although Vesta is not nearly so bright as Uranus, shining, indeed, only as a star of the seventh magnitude, yet she can at times be seen without telescopic aid by persons of extremely good sight; and her movements are far more rapid, and therefore more likely to attract notice, than those of Uranus. In the high tablelands of eastern countries, keen-sighted observers might quite readily have discovered her planetary nature, whereas the slow movements of Uranus would probably have escaped their notice.

In the summer of 1887 Vesta and Uranus were comparable under favourable conditions. In the transparent skies of my Florida home I found them both quite conspicuous without telescopic aid.

they would retain. The sequel has shown that my surmises were well founded.'

(1373.) There are three points to be specially noted in this account. First, the astronomer was engaged in a process of systematic survey of the celestial depths, so that the discovery of the new orb cannot be properly regarded as accidental, although Herschel was not at the time on the look-out for as yet unknown planets. Secondly, the instruments he was employing were of his own construction and device, and probably no other in existence in his day would have led him to the discovery that the strange orb was not a fixed star. And, thirdly, without the experience he had acquired in the study of the heavens he would not have been able to apply the test which, as we have seen, he found so decisive. So that, whether we consider the work Herschel was engaged upon, the instruments he used, or the experience he had acquired, we recognise the fact that of the astronomers of his time he was the most capable of discovering Uranus otherwise than by a fortunate accident. Others might have lighted on the discovery—indeed, we shall presently see that the real wonder is that Uranus had not been for many years a recognised member of the solar system—but probably no one except Herschel could within a few minutes of his first view of the planet have pronounced so confidently that the strange orb (whatever it might be) was not a fixed star.

(1374.) We need not consider here the series of researches by which it was finally demonstrated that the newly-discovered body was not a comet but a planet, travelling on a nearly circular path around the sun, at about twice Saturn's distance from that orb. With this part of the work Herschel had very little to do. As the newly-discovered body travelled onwards upon its apparent path, the professional (that is, salaried) astronomers were able to determine what its real path might be. At first they were misled by erroneous measures of the stranger's apparent size, which suggested that the supposed comet had in the course of the first month after its discovery approached to within half its original distance. At length, setting aside all these measures, and considering only the movements of the stranger, Professor Saron was led to the belief that it was no comet, but a new member of the solar system. It was eventually proved, chiefly by the labours of Lexell, Lalande, and Laplace, that this theory fully explained all the observed motions of the newly-discovered body, and before long (so complete is the mastery which the Newtonian system gives astronomers over the motions of the heavenly bodies) all the circumstances of the new planet's real motions became very accurately known. It was now possible, not only to predict the future movements of the stranger, but to calculate his motions during former years.

This last process was quickly applied to the planet, with the object of determining whether, among the records of observations made on stars, any might be detected which related in reality to the newly-discovered body. The result will appear at first sight somewhat surprising. The new planet had actually been observed no less than nineteen times before that night when Herschel first showed that it was not a fixed star, and those observations were made by astronomers no less eminent than Flamsteed, Bradley, Mayer, and Lemonnier. Flamsteed had seen the planet five several times, each time cataloguing it as a star of the sixth magnitude, so that five such stars had to be dismissed from Flamsteed's lists. But the case of Lemonnier was even more singular; for he had actually observed the planet no less than twelve times, several of his observations having been made within the space of a few weeks. Arago, who yearned for an astronomy which should be altogether French, lamented bitterly the want of system displayed by Lemonnier in 1769; had he but reduced and arranged his observations, said Arago, his name instead of Herschel's would have been attached for all time to the new planet. But, alas! Lemonnier's astronomical papers were a scene of chaos. Bouvard tells us that one of Lemonnier's records of Uranus was written on a paper bag which had contained hair powder!

(1375.) The discovery of Uranus was an altogether novel and unlooked-for circumstance. It was not supposed that fresh discoveries of like nature would be made, still less that a planet would hereafter be discovered under circumstances far more interesting even than those which attended the discovery of Uranus. Accordingly great difficulty was found in fitting Uranus with a name. Lalande proposed the name of the discoverer, and the new planet was long known on the Continent by the name of Herschel. The symbol of the planet (♅), the initial letter of Herschel's name with a small globe attached to the cross-stroke, still reminds us of the honour which Continental astronomers generously proposed to render to their fellow-worker in England.[1] Lichtenberg proposed the name of Astræa, the goddess of justice—for this 'exquisite reason,' that since justice had failed to establish her reign upon earth, she might be supposed to have removed herself as far as possible from our unworthy planet. Poinsinet suggested that Cybele would be a suitable name; for since Saturn and Jupiter, to whom the gods owed their origin, had long held their seat in the heavens, it was time to find

[1] There is a certain incongruity, accordingly, among the symbols of the primary planets. Mercury is symbolised by his *caduceus*, Venus by her looking-glass, Mars by his spear and shield, Jupiter by his throne, Saturn by his sickle; and, again, when we pass to the symbols assigned to the planets discovered in the present century, we find Neptune symbolised by his trident, Vesta by her altar, Ceres by her sickle, Minerva by a sword, and Juno by a star-tipped sceptre. Uranus alone is represented by a symbol which has no relation to his position among the deities of mythology.

a place for Cybele, 'the great mother of the gods.' Had the supposed Greek representative of Cybele—Rhæa—been selected for the honour, the name of the planet would have approached somewhat nearly in sound, and perhaps in signification, to the old name Ráhu. But neither Astræa nor Cybele was regarded as of sufficient dignity and importance among the ancient deities to supply a name for the new planet.[1] Prosperin proposed Neptune as a suitable name, because Saturn would thus have the eldest of his sons on one side of him, and his second son on the other. Bode at length suggested the name of Uranus, the most ancient of the deities; and as Saturn, the father of Jupiter, travels on a wider orbit than Jupiter, so it was judged fitting that an even wider orbit than Saturn's should be adjudged to Jupiter's grandfather.[2]

Herschel himself proposed another name. As Galileo had called the satellites of Jupiter the Medicean planets, while French astronomers proposed to call the spots on the sun the Bourbonian stars, so Herschel, grateful for the kindness which he had received at the hands of George III., proposed that the new planet should be called Georgium Sidus, the Georgian Star.[3]

(1376.) In considering Herschel's telescopic study of the planet we must remember that, owing to the enormous length of time occupied by Uranus in circling round his orbit, the astronomer labours under a difficulty of a novel kind. As Jupiter and Saturn circle on their wide orbits they exhibit to us—the former in the course of eleven years, the latter in the course of twenty-nine and a half years—all those changes of aspect (slight in Jupiter's

[1] Both these names are found among the asteroids, the fifth of these bodies (in order of discovery) being called Astræa, the eighty-ninth being named after Cybele, the great mother of gods and goddesses. One of Saturn's satellites is called Rhea.

[2] In accepting the name of Uranus for the new planet, astronomers seemed to assert a belief that no planet would be found to travel on a yet wider path; and accordingly when a more distant planet was discovered, the suggestion of Prosperin had to be reconsidered; but it was too late to change the accepted nomenclature, and accordingly the younger brother of Jupiter has had assigned to him a planet circling outside the paths of the planets assigned to their father and grandfather. It may be noted, also, that a more appropriate name than Uranus would have been Cœlus, since all the other planets have received the Latin names of the deities.

[3] An American, born, however, under the British flag in Nova Scotia, has translated this name the Star of the Georges; but Prof. Newcomb may rest satisfied that Herschel thought only of his patron George III., the first English-born Hanoverian King of England, when he named the star. Had he meant to honour the whole race of Georges he would probably have called his planet Georgiorum Sidus.

A letter of Herschel to the President of the Royal Society, Sir Joseph Banks, concludes by remarking that, in addressing his letter to the President of the Royal Society, he takes the most effectual method of communicating the proposed name to the *literati* of Europe, which he hopes 'they will receive with pleasure.' But they altogether declined, and for some time two names came into general use—one in Great Britain, and the other on the Continent, neither being the name eventually adopted for the planet. In books published in England for more than a quarter of a century after the discovery of Uranus we find the planet called either the Georgium Sidus, or the Georgian. For a shorter season the planet was called on the Continent either the Herschelian planet, or simply Herschel. Many years elapsed before the present more sensible usage was definitely established.

case) which correspond with the varying presentations of these planets towards the sun.

Although similar changes occur in the case of Uranus, they occupy no less than eighty-four years in running through their cycle, or forty-two years in completing a half-cycle—during which, necessarily, all possible presentations of the planet are exhibited.[1]

When we add to this circumstance the extreme faintness of Uranus, we cannot wonder that Herschel should have been unable to speak very confidently on many points of interest. His measures of the planet's globe were sufficiently satisfactory, and, combined with modern researches, show that Uranus has the dimensions indicated in Art. 1370. Sir W. Herschel was unable to measure the disc of Uranus in such a way as to determine whether the planet is compressed in the same marked degree as Jupiter and Saturn. All that he felt competent to say was that the disc of the planet seemed to him to be oval, whether he used his seven-feet, or his ten-feet, or his twenty-feet reflector. Arago has expressed some surprise that Herschel should have been content with such a statement. But in reality the circumstance is in no way surprising. For, as a matter of fact, Herschel had been almost foiled by the difficulty of measuring even the planet's mean diameter. The discordance between his earliest measures is somewhat startling. His first estimate of the diameter made it ten thousand miles too small (its actual value being about thirty-four thousand miles); his next made it nearly three thousand miles too great; while his third made it ten thousand miles too great. His contemporaries were even less successful. Maskelyne, after a long and careful series of observations, assigned to the planet a diameter eight thousand miles too small; the astronomers of Milan gave the planet a diameter more than twenty thousand miles too great; and Mayer, of Mannheim, was even more unfortunate, for he assigned to the planet a diameter exceeding its actual diameter of thirty-four thousand miles by rather more than fifty thousand miles. It will be understood, therefore, that Herschel might well leave unattempted the task of comparing the different diameters of the planet. This task required that he should estimate a quantity (the difference between the greatest and the least diameters) which was small even by comparison with the *errors* of his former measurements.

(1377.) But, besides this, a peculiarity in the axial pose of Uranus has

[1] It is commonly recognised among telescopists that the observing time of an astronomer's life—that is, the period during which he retains not merely his full skill, but the energy necessary for difficult researches—continues but about twenty-five years at the outside. So that few astronomers can hope to study Uranus in all his presentations as they can study Mars, or Jupiter, or Saturn.

to be taken into account. We have spoken of the uprightness of Jupiter's axis with reference to his path ; and by this it is intended to indicate the fact that if we regard Jupiter's path as a great level surface, and compare Jupiter to a gigantic top spinning upon that surface, this mighty top spins with a nearly upright axis. In the case of Uranus the state of things is altogether different. The axis of Uranus is so bowed down from uprightness as to be nearly in the level of the planet's path. The result of this is that when Uranus is in one part of his path his northern pole is turned almost directly towards us. At such a time we should be able to detect no sign of polar flattening even though Uranus were shaped like a watch-case. At the opposite part the other pole is as directly turned towards the Earth. Only at the parts of his path between these two can any signs of compression be expected to manifest themselves ; and Uranus occupies these portions of his path only at intervals of forty-two years.

Herschel would have failed altogether in determining the pose of Uranus but for his discovery that the planet has moons. For the moons of the larger planets travel for the most part near the plane of their planet's equator. Herschel could, indeed, only infer this in the case of Uranus, but the inference, he thought, was safe because the relation seems to depend on physical relations.

(1378.) Assuming the planet's axis and rotation to be at right angles to the orbits of the satellites, the ellipticity would not be very visible in 1781, the date of the discovery of the planet ; but it would have increased till 1797, and then diminished again till 1818, when the orbits of the satellites were nearly square to the line joining the Earth and Uranus. Again in 1839 and December 1881 the ellipticity would be at its maximum. Sir William Herschel thought the planet's outline decidedly elliptical in 1792 and 1794, after having noted it as round in 1782. Mädler made a series of measures at Dorpat in 1842 and 1843, and found the ellipticity to be $\frac{1}{9 \cdot 22}$. Buffham in 1870[1] described the ellipticity as 'obvious' ; and Safarik, from a series of observations extending from 1877 to 1883,[2] thought the ellipticity 'considerable' and 'greater than that of Saturn.' Prof. C. A. Young, from a careful series of measures made in May and June 1883, with the great 23-inch achromatic of the Halstead Observatory and a filar-micrometer, determined the ellipticity to be $\frac{1}{13 \cdot 99}$; while Schiaparelli, in the previous year with an eight-inch telescope, determined it as about $\frac{1}{11}$. Such measures, however, do not prove that the equatorial bulge exactly coincides with the plane of the orbits of the satellites. Prof. Young, Dr. Brackett, Mr. McNeill, and M. Tisserand all have seen markings on the planet's disc resembling the belts

[1] *Monthly Notices*, vol. xxxiii. p. 164. [2] *Astron. Nach.* 2,505, and *Observatory*, vi. p. 182.

of Jupiter and Saturn; and they all agree that the trend of the belts does not coincide with the position angle of the major axis of the orbits of the satellites—their estimates of the discordance[1] differ from 15° to as much as 30°. Possibly the error lies in judging the direction of the belts, which are only faint markings just on the limit of visibility.

(1379.) For six years Herschel looked in vain for Uranian satellites. His largest telescopes, supplemented by his wonderful eyesight and his long practice in detecting minute points of light, failed to reveal any trace of such bodies. At length he devised a plan by which the light-gathering power of his telescopes was largely increased. On January 11, 1787, he detected two satellites, though several days elapsed before he felt justified in announcing the discovery. At intervals, during the years 1790–1798, he repeated his observations; and he supposed that he had discovered four other satellites. He expresses so much confidence as to the real existence of these four bodies that it is very difficult for those who appreciate his skill to understand how he could have been deceived. But he admits that he was unable to watch any of these satellites through a considerable part of its path, or to identify any of them on different nights. All he felt sure about was that certain points of light were seen which did not remain stationary, as would have happened had they been fixed stars. No astronomer, however, has since seen any of these four additional satellites, though Mr. Lassell has discovered two which Herschel could not see (probably owing to their nearness to the body of the planet). As Mr. Lassell employed a telescope more powerful than Herschel's largest reflector, and has given much attention to the subject, no one has a better right to speak authoritatively on the subject of the four additional satellites. Since, therefore, he was very confident that they have no existence, we are bound to regard that view as the most probable; yet new Uranian satellites may hereafter be revealed.

The four known moons travel backwards; that is, they circle in a direction opposed to that in which all the planets of the solar system, and all the moons of Jupiter and Saturn, as well as our own moon, are observed to travel. Much importance has been attached to this peculiarity; but in reality the paths of the Uranian moons are so strangely situated with respect to the path of Uranus that the direction in which they travel can hardly be compared with the common direction of the planetary motions.

(1380.) The great slope or tilt of the paths of the satellites is even a more singular feature than the direction of their motion. Taken together with the undoubted retrograde motion of the satellite of Neptune, and the great inclination of the equator of Uranus, as indicated by the trend of the belts as

[1] *Astron. Nach.* and *Observatory*, vi. Nov. 1883, p. 331.

well as by the measurements of the polar compression, we seem to have evidence that the two outer planets of the solar system, while moving in the same direction and approximately in the same plane as the inner planets, have been perturbed as to their axial motions. It seems difficult to believe that the comparative uniformity observable in the motions of the inner members of the group did not originally extend to the outer members; but there must either have originally been such a want of uniformity, or a change must have been brought about in the axial motions of Uranus and Neptune either by a sudden cataclysm, such as a blow from a comet, or by some slowly-acting force the existence of which we at present have no suspicion of.

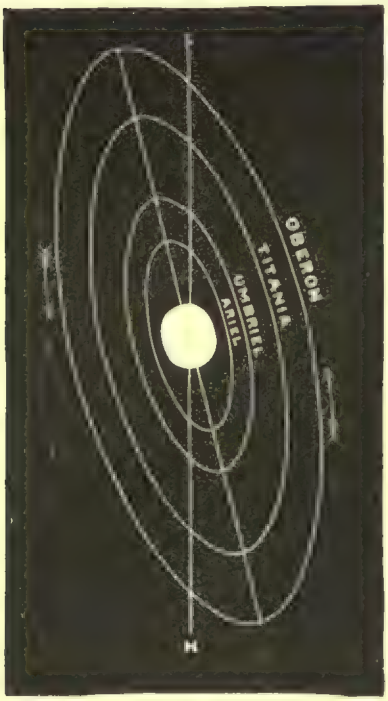


FIG. 423.—The apparent orbits of the satellites of Uranus.

(1381.) The two outermost satellites, Oberon and Titania, are the most easily seen, but even they are very minute and difficult objects. Ward, of Belfast, has caught sight of them with a $4\frac{3}{10}$-inch achromatic, Huggins with an 8-inch achromatic, and Sadler with a $6\frac{1}{2}$-inch mirror. Webb could only see one of them with his $9\frac{1}{3}$-inch speculum. Ariel and Umbriel, the two innermost of the satellites, were discovered by Lassell on October 24, 1851.[1] He describes them as very faint objects, 'probably much less than half the brightness of the conspicuous ones.' The following elements are given by Newcomb, from observations made with the Washington 26-inch achromatic in 1874–75:—

ARIEL.—	Periodical revolution, taking account of precession	$2^d{\cdot}520383$
	Radius of orbit (supposed circular)	$13''{\cdot}78$
	Longitude at noon, December 31, 1871	$15^\circ{\cdot}90$
UMBRIEL.—	Periodic time	$4^d{\cdot}144181$
	Radius of orbit (supposed circular)	$19''{\cdot}20$
	Longitude at epoch	$130^\circ{\cdot}59$
TITANIA.—	Periodic time	$8^d{\cdot}705897$
	Radius of orbit (supposed circular)	$31''{\cdot}48$
	Longitude at epoch	$224^\circ{\cdot}0$
OBERON.—	Periodic time	$13^d{\cdot}463269$
	Radius of orbit (supposed circular)	$42''{\cdot}10$
	Longitude at epoch	$148^\circ{\cdot}9$

[1] *Monthly Notices*, xi. p. 248.

(1382.) The body of the planet shines with a greenish light of sufficient intensity to give a faint spectrum, which Secchi, Vogel, and various other observers thought presented abnormal characters. They speak of the spectrum as fluted by dark absorption bands ; and some observers also describe bright lines or bands. As recently as May 1889 Mr. Albert Taylor, observing with Mr. Common's 5-foot reflector, thought that he detected ten dark bands in the spectrum and fourteen [1] bright bands and flutings, many of which were seen with sufficient distinctness to permit of their positions being measured. Huggins has, however, succeeded in obtaining a photograph of the spectrum of the planet extending from F to N in the ultra-violet. The plate was obtained with two hours' exposure, and on the same plate a solar spectrum has been photographed for comparison. All the principal Fraunhofer lines are to be seen in the photographed spectrum of the planet, but no bright lines or other dark lines or flutings are to be distinguished. It therefore seems that the major part of the light derived from the planet's disc is reflected solar light, and that any light of a different character which may be radiated by the planet is not sufficiently intense to have left its trace on the photographic plate with two hours' exposure.

[1] *Monthly Notices* xlix. 406.

CHAPTER XVI.

THE PLANET NEPTUNE.

(1383.) The discovery of Neptune, one of the most interesting and even impressive episodes in the history of astronomy, has never, so far as I know, been properly dealt with in treatises on what is called popular science. Of course, a thorough and sufficient treatment of the subject in its strictly scientific and mathematical aspect would be utterly unsuitable in popular treatises, and one class of writers on popular science (professors in colleges and well-trained workers in Government observatories) have shown their sense of this by avoiding all attempts to explain this subject, and others of kindred abstruseness, in their strictly scientific aspect. On the other hand the greater number of those who have undertaken to write treatises on popular astronomy are quite unable to discuss a subject of this kind scientifically, and therefore naturally fail in their attempts to expound it popularly. So far as I know, only Sir John Herschel, in his 'Outlines of Astronomy,' has dealt with the discovery of Neptune with sufficient knowledge on the one hand to have mastered the subject independently, and on the other with adequate opportunities to understand what was necessary in order to make the subject intelligible to the general reader, combined with the necessary skill, practice, and experience, to do the work of popularisation properly. [Sir Edmund Beckett (now Lord Grimthorpe) could doubtless have done the work as well as any if it had fallen within the scope of his 'Astronomy without Mathematics;' but, dealt with as thoroughly as some of the other subjects considered in that powerful work, it would have been a very difficult study, unsuited for his readers.] Sir John Herschel, unfortunately, endeavoured to do too much. He treats the subject so thoroughly that what he says would form a small treatise, and in so masterly a manner that advanced mathematical students at college would find that treatise most profitable reading. But the articles (760 to 776, with certain preceding articles on the general subject of planetary perturbations) in which this subject is dealt with are certainly not suited for popular reading. That they have been very little studied by the general reader of popular trea-

tises on astronomy is shown by the circumstance that popular writers on astronomy have clearly not cared to study that admirable portion of Sir John Herschel's work. The only writer among them who refers to it is Mr. Ledger in his excellent little book on 'The Sun: its Planets, and their Satellites'; yet, although Mr. Ledger shows readiness and skill in dealing with geometrical matters in that work, he falls into most of the customary errors about Neptune's perturbing action on Uranus, and, indeed, only makes confusion worse confounded by his reference to Herschel's explanation.

Let us begin with a picture which (or its equivalent) has done duty in a number of astronomical treatises, while it has been partly misunderstood even in so far as it is correct, and it is partly misleading. I do not know to whom popular astronomy may owe the particular form of this picture, shown in fig. 424. It appears in Mr. Ledger's book, in Chambers' 'Astronomy,' and, for aught I know, elsewhere. In Nichol's 'Cyclopædia of the Physical Sciences' there are two drawings (p. 618, fig. 6), similarly misleading drawings as interpreted (see particularly the explanation of the subsidiary drawing, fig. 7). In Lardner's 'Handbook of Astronomy' there is another representation, with arrows, as in fig. 424, explained in the same incorrect way apparently by Mr. Dunkin (at any rate in the edition revised by him).

FIG. 424.—Diagram ordinarily given to illustrate the perturbation of Uranus by Neptune.

Presuming, for want of precise information, that fig. 424 was originally drawn to illustrate Mr. Chambers' useful 'Descriptive Astronomy,' I take his words in explanation of its incorrect teachings. 'The accompanying diagram,' he says, 'shows the paths of Uranus and Neptune from 1781 to 1840, and will illustrate the perturbing action of the latter on the former. From 1781 to 1822, it will be evident from the direction of the arrow that Neptune tended to draw Uranus in advance of its place as computed independently of exterior perturbations' [meaning doubtless Neptune's]. 'In 1822 the two planets were in heliocentric conjunction, and the only effect of Neptune's influence was to draw Uranus farther from the Sun, without altering its longi-

tude. From 1822 to 1840, the effect of Neptune was to destroy the excess of longitude accumulated from 1781,' with further remarks relating to the observed effects of Neptune's action, not explanatory thereof.[1] Nichol, similarly, assumes that the line joining Uranus and Neptune indicates the direction of Neptune's perturbing action. So does Dunkin (or Lardner) in explaining the picture corresponding to fig. 424. Mr. Ledger recognises that there is something wrong about the explanation. For he says that the attraction of the external planet might have acted (although the real effect is considerably more complicated) somewhat after the manner indicated by the arrows in the diagram, and in a note he refers to Sir J. Herschel's beautiful and lucid geometrical explanation. But he speaks of Herschel's method as dealing with the direct or tangential effect of Neptune upon the velocity of Uranus, *which is all that* the figure (fig. 424) *indicates*, and Mr. Lynn, who 'carefully revised the proof sheets,' appears to have made no suggestion in regard to this statement, to which I referred when I spoke of Mr. Ledger's reference to Herschel's explanation as making confusion worse confounded.

In reality the arrowed straight lines shown in fig. 424, that is, lines joining the two planets in simultaneous positions, do not represent, either in magnitude or in direction, Neptune's perturbing action on Uranus—neither the whole, nor the tangential, nor the radial—any more than a side of a triangle represents the area. It is not that 'the real effect is more complicated,' or that the arrowed lines only imperfectly suggest the effect of the disturbing action; these lines do not represent that action in any sense or degree. Looking at the direction of the arrows, as Mr. Chambers invites them, the readers of a book in which such a picture as fig. 424 is used would form quite erroneous ideas of the direction as well as of the amount of Neptune's perturbing action, alike radial (or rather normal), tangential, and resultant.

This will appear if we consider the real details of the problem.

In fig. 425 let P_1 P_2 P_3 P_4 represent the orbit of a planet around the sun at S, while Q represents an exterior planet; and let us consider what the disturbing effect of Q's action will be on a planet situated in various positions on the orbit P_1 P_2 P_3 P_4. We suppose this orbit circular, having at present no occasion to consider the effects of ellipticity. Draw a straight line Q P P′ cutting the orbit P_1 P_2 P_3 P_4 in two points P and P′, and consider the dis-

[1] In the last edition of Mr. Chambers' *Descriptive Astronomy* he adds a paragraph stating that Professor Adams has kindly furnished him with an explanatory comment on his diagram, in which he observes that 'The arrows rightly represent the direction of the force with which Neptune acts on Uranus taken singly, but the diagram does *not* represent the direction of the *disturbing* force which Neptune exerts on Uranus relatively to the Sun, and this latter force is what we must take into account in computing the planetary perturbations. To find the *disturbing* force, we must take the force of Neptune on the *Sun*, reverse its direction, and then compound this with the direct force of Neptune on Uranus.—A. C. R.

turbing actions of Q on the inferior planet first at P and then at P′. According to the incorrect method I have mentioned above, we have only to put an arrow on the line P′ P Q, and to say it is obvious that Q pulls the planet at P′ in the direction P′ Q, manifestly accelerating its motion round S (in the direction shown by the arrow in the orbit) and it is equally obvious that Q pulls the planet at P in the same direction P Q, accelerating its motion there also. That there must be something wrong about this, however, will be manifest when we note that as a mere matter of fact a planet situated at P′ would be retarded by the action of an exterior planet situated as at Q, while a planet at P would be accelerated by Q's action, though the same straight line, according to this exceedingly simple explanation, indicates the direction of the perturbing action in both cases.

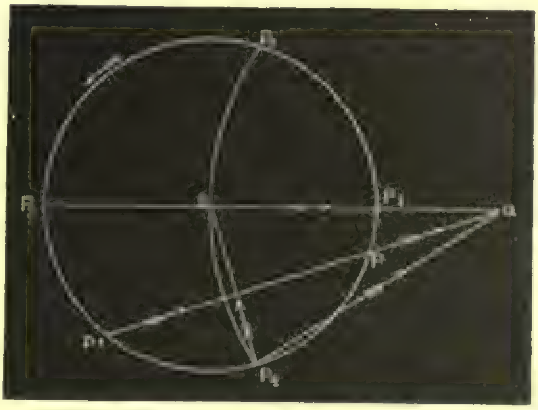


Fig. 425. Diagram to illustrate the perturbing action of an outer planet on an inner one.

The mistake lies in overlooking the fact that it is not the simple pull of the exterior planet on the inferior that we have to consider, but the relative pull which the exterior planet exerts on the inferior as compared with the pull it exerts on the sun. For instance, suppose the disturbed planet were at P_1, where a circular arc P_2 S P_4 around Q as centre and passing through S, cuts the orbit $P_1P_2P_3P_4$. In what way is the planet's motion around S affected? The sun and planet are equally pulled by the disturbing body at Q, the difference of the masses in no way concerns us here—every portion of the sun, equal to the mass of the planet at P_4, is pulled as strongly in direction S Q as the planet at P_4 is pulled in direction P_4 Q. In obedience to this equal pull S would begin to move along S Q, and the planet along P_4 Q with equal velocities. The line joining them would remain parallel to S P_4, but would grow shorter as they advanced towards Q. This is the same as saying that when the planet is at P_4 there is no tendency to increase the velocity, but there is a measurable *radial* or *normal* force, acting towards S. Thus the perturbing force which the explanation above cited indicates as acting in the direction P_4 Q and definitely describes as accelerating the motion of the planet round S, acts really in the direction P_4 S, and does not accelerate the disturbed planet at all.

Take next the positions P and P′, and here, as we shall find that there is in each case a tangential force, let us enter on a proper discussion of the amount as well as the direction of the perturbing force. For the problem

we have really to examine depends on the accelerative or retardative action exerted by Neptune on Uranus in various relative positions, and although radial disturbing forces indirectly affect the motion of the disturbed planet, and in the long run more effectually (or at least for longer lasting periods) than the tangential forces, yet this indirect action is not that whose effects led to the recognition of the unknown outer planet in the particular case we are dealing with.

Let us represent the force exerted by Q (so let us call the outer planet) on the sun—a force actually constant in the case of circular orbits, and nearly constant with all the giant planets—by the line S Q. Then the force exerted by Q on the inner planet will be much greater when that planet is at P, and much less when that planet is at P′, for it varies inversely as the square of the distance. We should have to produce Q P P′ till that line was nearly four times as long as Q S to get a line exceeding Q S in the same degree that the square of Q S exceeds the square of P Q (for as we have drawn it S Q is about twice as long as P Q). If the line Q P were thus produced, and its extremity joined to S, the straight line so drawn would represent in direction and magnitude the disturbing force acting on the inner planet at P. Through P draw a straight line P*ln*, parallel to the line thus supposed to be drawn to S, but not shown in the figure, nor is the whole of the line P*ln* shown, because there is no room. This line P*ln*, carried on till it is nearly four times as long as S Q, will represent the disturbing force at P; and if we resolve this force tangentially in the direction P*ty*, and radially in the direction P*r*, we get two lines, one along P*t*, representing the considerable force acting tangentially to *accelerate* the interior planet, the other also considerable force acting radially *outwards*.

(1384.) In the same manner it can be shown that when the inner planet is at any point along the arc P_4 P_1, the disturbing force exerted by the outer planet at Q is accelerative, except at P_4, where the tangential component vanishes, as we have already seen, and at P_1 where it also obviously vanishes, the pull both on S and on the planet being there radial, so that, of course, the resultant force is radial likewise. We are not here concerned with the radial force; but may note in passing that as the total disturbing force at P_1 is directed to a point on S Q which passes from S to Q as the planet moves from P_4 to P_1, it follows necessarily that, when the planet is at some point on the arc P_4 P_1, the direction of the disturbing force will be tangential, or the radial force will vanish. This will be somewhere near p. From this point the radial force which before had been directed inwards will be directed outwards, it will attain its maximum when the planet is at P_1, and will attain a maximum value —when the planet is not far from P—and will thence diminish till it vanishes

when the planet is at P_1. We must not fall into the mistake into which Mr. Lockyer naturally enough fell in dealing with a kindred problem, of supposing that where the accelerative force attains its maximum, the advance due to acceleration is greatest. On the contrary, this advance, which necessarily increases so long as there is any acceleration at all, attains its maximum when the acceleration vanishes, that is to say when the planet is at P_1, in heliocentric conjunction with the disturbing body.

Now turn to the case where the disturbed planet is at P', a point in the arc $P_3 P_4$. Here we can complete our construction more conveniently,

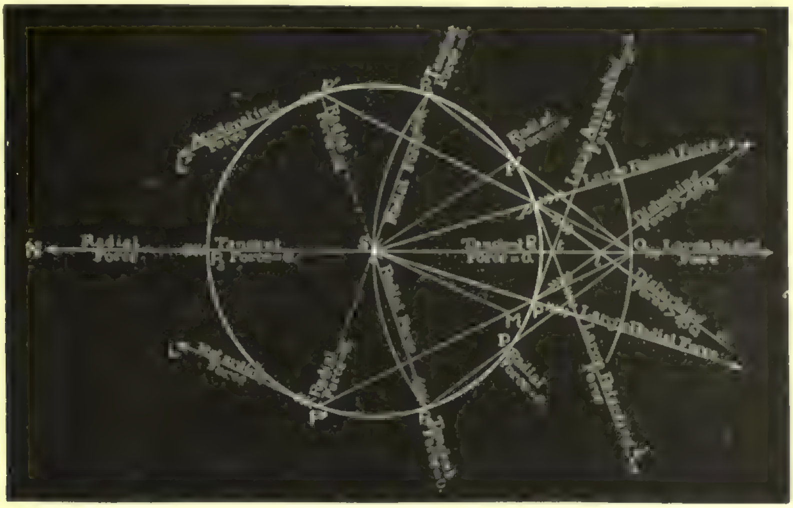


FIG. 426.—Diagram illustrating the accelerating and retarding effect of an outer attracting body which is supposed not to move.

because, since the force exerted on the planet at P' is less than the force exerted on the sun at S, we can indicate the magnitude of this force by a portion of Q P' instead of having to produce this line to a considerable distance. We take M Q such that M Q is to S Q as the square of S Q to the square of Q P',[1] and join S M; then P' L drawn parallel and equal to M S P' L represents the disturbing force on the planet at P', the arrow showing the direction in which the force acts (very different from the direction P' Q indicated by Messrs. Chambers, Nichol, Dunkin and the rest). This force is wholly tangential and retardative, the radial force vanishing at P'. (I selected

[1] It is very easy to obtain M Q by a geometrical construction; but it is better to measure off the lengths Q S, Q P', and expressing these lengths numerically, square the resulting numbers and represent the resulting ratio geometrically. This may be done very quickly and conveniently by the use of tables of logarithms. Suppose the length of Q S to be 100, and that of Q P' to be 141·7; then we find the logarithm of 1417, double (to get the ratio of the squares), subtract from unity to get the ratio inverted, and find the logarithm obtained to be that of 498, showing that M Q must be 49·8 or very nearly half of Q S. (We can start by taking Q S = 10 units of length and dividing each by 10.) But indeed it is unnecessary to explain details of the sort to the student who cares enough for such points to follow the reasoning in the text.

P' with this intent, to prevent the figure from becoming too complicated; the prolongation of $P_2 S$ determines the point P' where the radial force vanishes.)

By a similar construction, resolving the disturbing force at other points where it is not wholly tangential as at P', it can be shown that at any point along the arc $P_3 P_4$ the disturbing force is retardative, except at P_3, where it manifestly vanishes (both the pull on S and the pull on the planet being radial, and therefore their resultant radial also) and at P_4, where it has been already shown to vanish. The tangential retardative force attains its maximum when the planet is not far from P', where, however, this force is much less than the accelerative force at points in the middle of the arc $P_4 P_1$. As for the radial force which vanishes, as we have seen, at P', this force has its maximum value when the planet is at P_3, while it acts outwards and is represented by a line $P_3 N$ nearly equal to $P_3 S$ in length (about $\frac{1}{58}$th shorter). Between P' and P_4 the radial force acts towards S, and at P_4 is represented in magnitude—as our former reasoning really showed, though we had not, at the time, adopted any scale for representing the perturbing forces—by the line $P_4 S$.

Applying similar reasoning to the arc $P_1 P_2 P_3$, we find the tangential force accelerative when the planet is on the arc $P_1 P_2$, retardative when the planet is on the arc $P_2 P_3$, and evanescent at P_2.[1]

(1385.) We have seen that in describing a revolution around S on the orbit $P_1 P_2 P_3 P_4$ under the disturbing influence of an external planet supposed (quite impossibly) to be stationary at Q all the time, the planet will be powerfully accelerated while on the arc $P_4 P_1$, and as powerfully retarded while on the arc $P_1 P_2$, less powerfully accelerated, but for a longer time, while on the arc $P_2 P_3$, and the acceleration on this arc will be exactly balanced by an equal retardation while the planet is on the arc $P_3 P_4$. At $P_1 P_2 P_3 P_4$ there is neither acceleration nor retardation. The total advance due to acceleration attains its maximum at P_1 and P_3, where acceleration ceases, while the total falling behind, due to retardation, attains its maximum at P_2 and P_4 where retardation ceases. Hence somewhere on the arc $P_1 P_2$ the planet has its mean position in longitude, and again somewhere on the arc $P_2 P_3$, somewhere on the arc $P_3 P_4$, and somewhere on the arc $P_4 P_1$. At some point in $P_1 P_2$ the balance of acceleration over retardation acquired in the arc $P_4 P_1$ is worked off (not the whole acceleration, which requires the whole retardation along

[1] In the chapter the 'great "inequality" of Jupiter and Saturn,' in the first edition of my first book, *Saturn and its System*, I have discussed in a somewhat different way the relations dealt with in the text, in dealing with others of greater complexity. But experience has taught me that such explanations are not much cared for by the general reader, and I have been obliged to eliminate the whole chapter from the second edition of that work, lest it should scare off readers as it did from the first.

P_1 P_2 to work it off, but such acceleration beyond the retardation at P_4 as has accrued while the planet was traversing the arc P_4 P_1). And in like manner at some point in P_3 P_4, the balance of advance acquired while the planet was traversing the arc P_2 P_3 is worked off. It will readily be seen that the planet is perturbed most effectively when travelling between those points on P_4 P_1 and P_1 P_2 where it has its mean position; for here, while traversing a relatively small part of its circuit, it is exposed to active acceleration followed by as active retardation.

(1386.) But noting that the tangential force was that which gave the effective part of the evidence which led to the discovery of Neptune, let us set out separately in a figure the actual tangential perturbing forces on a planet travelling around as P in fig. 427, round the orbit P_1 P_2 P_3 P_4 corresponding to the orbit of Uranus, under the disturbing action of a planet at Q, corresponding to the mean distance of Neptune. Proceeding as already explained, we obtain such lines as the lines P Y, P′ Y′, py, $p'y'$ representing the disturbing forces at different points round the path P_1 P_2 P_3 P_1. When we have drawn a sufficient number of such lines, we find their extremities,

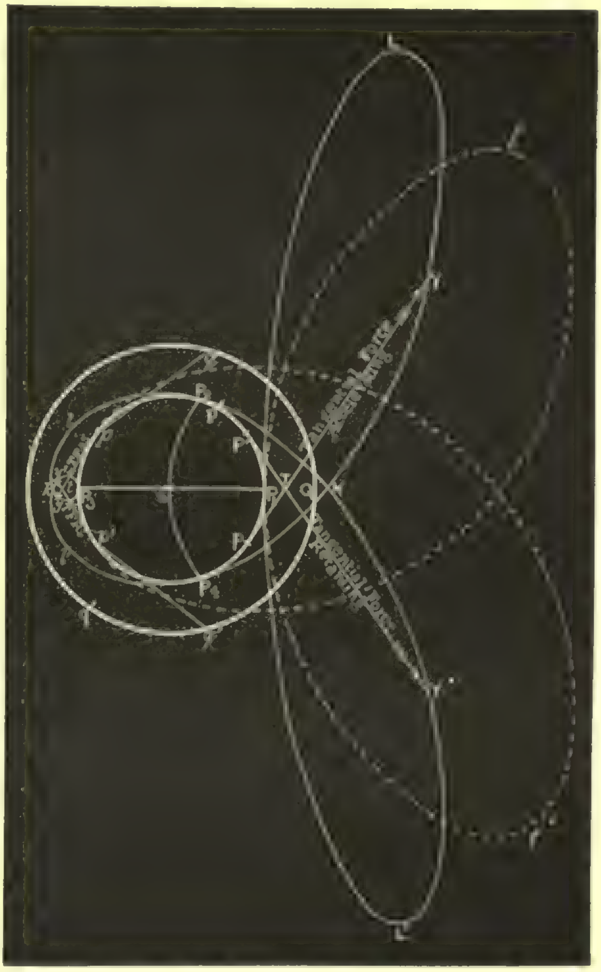


Fig. 427. Diagram showing magnitude of tangential accelerating and retarding forces. Continuous curve when the outer body is stationary, dotted curve when the outer body moves like a planet.

Y_1 Y'_1 $y_1 y'_1$ &c., indicating a curve such as P_4 L P_1 L′ $P_2 l P_3 l'$ P_4, perfectly symmetrical with respect to the line P_3 P_1 produced through k and K. This curve indicates the rapid growth of the accelerating tangential force from nothing at P_4 to a maximum between P_4 and P_1, and its decadence thence to P_1, where again it vanishes; we see it growing again rapidly beyond P_1, but now acting as a retarding force, attaining a maximum before the disturbed planet

reaches P' and thence diminishing until the planet is at P_2, where the tangential force vanishes. Beyond P_2 the tangential force is again accelerative, and attains its maximum when the planet is not far from p, thence diminishing till the disturbed planet is at P_3. After that, the tangential force is retardative ; attains its maximum when the disturbed planet is not far from p' and vanishes when the planet, having completed one circuit, is again at P_4.

Our reasoning is not affected when we take into account the motion of the outer planet. The arcs along which there is acceleration and retardation are all considerably increased when, as in the case of Neptune and Uranus, the motion of the outer planet is not very small compared with that of the inner. But the same general relations hold, and if the accelerative and retardative actions do not alternate so quickly, their effects are even more clearly to be recognised, because while as great at corresponding distances as in the imaginary case before considered, they are longer continued.

For instance, throwing in $Q\,q$ a portion of the path of the disturbing planet Q, we find by carrying this planet along at its proper relative rate while P is travelling round its orbit, the two being supposed to start from heliocentric conjunctions at P, Q, that instead of the tangential force vanishing when the interior planet has reached P_2 (such that $P_2\,Q$ is equal to $S\,Q$) it does not vanish till the interior planet is at p, the exterior planet being at q and $p\,q$ being equal to $S\,q$. In like manner the tangential force had vanished when the two planets were at p', q', respectively (such that $p'\,q'$ is equal to $S\,q'$). It follows that the disturbed planet, instead of being accelerated along such an arc as $P_4\;P_1$ and retarded along such an arc as $P_1\,P_2$, is actively accelerated along the arc $p'\,P_1$ and retarded along the arc $P_1\,p$; and as the accelerations and retardations at points of these longer arcs are as great as the accelerations and retardations at corresponding points of the shorter ones in the imagined case of the external planet being at rest, we see that the total acceleration and retardation produced by a planet moving as Neptune actually does must be much greater than those which would be produced if that planet were stationary all the time as at Q. The student will have no difficulty in indicating the accelerating and retarding forces at different parts of the arcs $p'P_1$ and $P_1\,p$ respectively, either independently or by dividing each arc into a number of equal parts, and each of the arcs $P_4\;P_1$ and $P_1\;P_2$ into as many, then drawing tangents at the former points of division in the same direction as and equal in length to, those from the latter points whose extremities had given the curves $P_4\,L\,P_1$ and $P_1\,L'\,P_2$. The extremities of the tangents thus drawn will give the loops $p'l\,P_1$ and $P_1 l' p$; and the greater span of these loops on the inner orbit (as compared with the loops $P_4\,L\,P_1$ and $P_1\,L'\,P_2$) will serve to give a good idea of the more effective acceleration produced by an exterior

planet as it actually moves than in the imaginary case first conveniently dealt with.

Carrying the external planet onwards from q, we find it coming into heliocentric opposition with the interior planet when the two bodies are, say, in the positions p' and q'; and the loop $P_2 l P_3$, if drawn in the way already employed for the larger loops, would run from p over to p', and thence on (forming a much smaller loop at p' than the one at P_3) to touch $P_1 P_2 P_3 P_4$ again near p'. The student will find it an instructive and pleasant exercise to draw in this representation of the actual tangential forces exerted on the exterior planet at and on either side of heliocentric opposition. But as the perturbations of these are much less important than those near heliocentric conjunction, I have not thought it worth while to give a separate picture showing how they grow from zero to a maximum, then wane, die out and vanish again. To have introduced the curve into fig. 427, would have been confusing.[1]

(1387.) We see that the inferior planet in this case is much more perturbed by the tangential force when the two planets are in heliocentric conjunction than at any other part of their synodic circuits. The radial force, if represented on the same scale, would have presented curves seeming even more striking than the great loops of fig. 427, and the effects of the radial disturbance are, in reality, more important than those of the tangential force, which carry the inferior planet alternately in advance of and behind the place it would have if not affected by tangential accelerations and retardations. For the action of the radial force is of the same sort (outwards) throughout the greater part of the whole relative circuit of the inferior planet, and especially over the arcs near the places of heliocentric conjunction and opposition. Thus, the planet is constantly (on the average of each circuit) relieved of a portion—slight but measurable—of its tendency towards the central sun; and its period is thus

[1] I would recommend the student to make several tracings of the orbits $P_1 P_2 P_3 P_4$ and $Q q q'$, with the loops or without them, in the latter case obtaining the loops by suitable constructions, both for the simpler case of the extreme planet being at rest and for the actual case in which that planet travels round. From the curves thus obtained he should make other tracings, and in these four equidistant points along the path of the internal planet draw a number of tangents limited by the loops. He will thus get an excellent idea not only of the meaning of the loops, which, I trust, I have made clear in the text; but of the law according to which the tangential forces alternate from acceleration to retardation, and pass from 0 to their maxima, and thence to 0 again. He can now, as he is a student of the Newtonian way of representing forces and their effects, represent graphically by lines and areas the extra velocities acquired under the influence of these alternate accelerating and retarding forces from their beginning as 0 to their return to evanescence, and the total gain in longitude accruing in the accelerating arcs and lost in the retarding arcs. This is a study which will present no difficulties to the geometrician; and I could fain give an example of the work here; but unfortunately geometricians interested enough in such astronomical and dynamical problems, so treated after the Newtonian manner, are few; and I should incur the wrath of far the larger proportion of my readers were I to devote any considerable portion of space to the necessary constructions and reasoning.

slightly longer than it would be but for the external planet. Moreover, as in the case of Jupiter and Saturn, disturbances of the radial sort, if renewed (owing to any approach to commensurability in the periods) at the same parts of the orbit of either planet, will for long periods together affect the movement of each, and therefore of the inferior planet whose fortunes we are at present specially considering. But it would require many centuries to observe the effects of Neptune's perturbative action, of this slowly acting and indirect but long-lasting character. And inasmuch as Neptune was discovered by virtue of the quickly acting and direct effects of the tangential disturbing force, we must give attention specially to these alternating actions.

It will be manifest that the time when the tangential perturbations will be most likely to point to an external planet will be that during which the inferior planet is approaching and passing away from the place of heliocentric conjunction. Indeed, when we consider in the case of Uranus and Neptune the relative minuteness of the disturbing force, even at its maximum, we find that only in this part of the motion of Uranus could the effects of the tangential action be recognised at all. Somewhat anticipating, I note that the *total* disturbing force of Neptune when Uranus is in conjunction with him is only about $\frac{1}{7500}$ of the sun's attraction on Uranus; and the maximum tangential force is only a part of the total disturbing force when this force is much less than a maximum. In fact the maximum tangential force is only about $\frac{1}{16000}$ part of the sun's attraction on Uranus.[1]

The nature then of the problem of determining the place of an unknown external planet from its direct disturbing action alternately accelerating and retarding the motion of a known internal planet can be readily recognised. Under ordinary conditions, and as a matter of fact, in the case of Neptune nothing could be hoped for in this way except from the evidence to be obtained at and near the time when the two planets are in conjunction. The marked acceleration of the motion of the interior planet and the accumulated gain in longitude when the planets are approaching conjunction, followed by as marked a retardation and return of the interior planet to and behind its mean longitude, these are the signs of the existence of an external planet on which astronomers must depend, and for which they must look out if they wish early to discover such a body.

[1] I have not made any special calculation of the varying values of the tangential force; but fig. 427 indicates it with sufficient exactness; and from that it will be easily seen that the maximum tangential disturbing force is to the attraction of Neptune on a unit of the sun's mass (this force being represented by Q S) as about 10 to 3. Hence, putting the mass of Neptune at $\frac{1}{19000}$ of the sun's—which is a near enough value for such a calculation as this—and the distances of Neptune and Uranus from the sun at 30 and 19 respectively, we find the maximum tangential perturbing force bearing to the sun's attraction on Uranus the ratio compounded of the ratios 10 to 3, 1 to 19000, and $(19)^2$ to $(30)^2$, or the ratio of 19 to $1{,}000 \times 300$, or about 15,800 to 1.

(1388.) Now let us see what astronomers actually did in the case of Uranus, not at first with any idea of discovering an external planet—nay, with somewhat remarkable obliviousness of the possibility that, as Uranus had been found to travel outside the path of the remotest formerly known planet Saturn, another planet might be travelling, as yet undiscovered, outside the orbit of Uranus.

Quite early in the present century those whose duty it was to deal with the motions of the planets began to recognise that the orbit assigned to Uranus during the ten or twelve years following the discovery of the planet in 1781 required correction. (The reader may here, and in considering what follows herewith, study fig. 428, in which the mean movements of Uranus and Neptune are represented.)

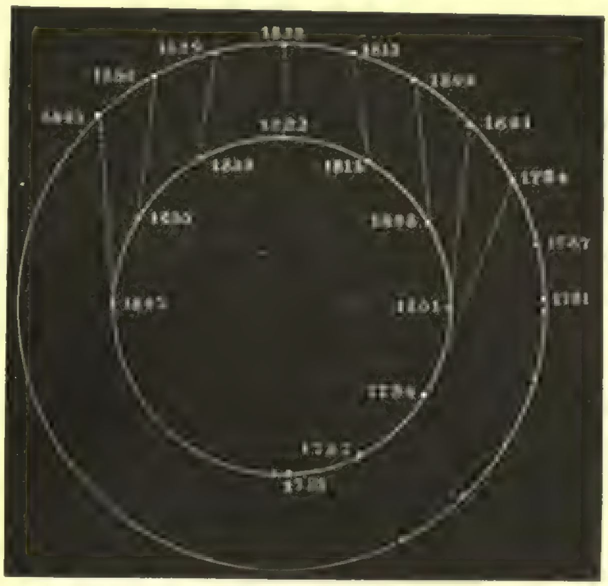


FIG. 428. Illustrating the mean motions of Uranus and Neptune their orbits being represented as concentric circles.

Suppose the period of Uranus determined at the end of last century to correspond with the motion of the planet in the circular path shown in fig. 428, and at the mean rate there represented (as shown by the dates), let the uniform advance of Uranus in its mean orbit be represented by the equal distances between the vertical lines, in fig. 429 the spaces along the line O X corresponding to 30° of advance in mean longitude, and therefore to about seven years each in time, since the period of Uranus is about 84 years and 30° one-twelfth of the complete circuit. Now let π_1, a_1, π_2, a_2, &c. along the line A B indicate the passages of Uranus through the perihelion and aphelion of his orbit corresponding to the dates 1714 (perihelion), 1756 (aphelion), 1798, 1840, &c. And let the straight line A B indicate by its parallelism to the line O X the uniform mean motion of a planet travelling in a circle around the sun at the centre in the same mean period as Uranus. Then the gain of the real Uranus on such a planet, supposing they started together from the perihelion π_1, would be represented by the rise of the curve U b b' above A B from π_1 to b, owing to the more rapid motion of Uranus in the perihelion half of its orbit. The real and mean planet would be together again in longitude when the former

was passing the aphelion α_1, after which, as the real Uranus would be falling behind, the curve U b b' passes below A B. And so on, the curve indicating the gain and loss of the real on the mean, Uranus passing alternately above and below A B, as shown in fig. 429, crossing it at the points π_1, α_1, π_2, α_2, &c.

Now the departures of the observed Uranus from the path of Uranus as calculated at the beginning of the century were such as are shown by the dots of fig. 429; only it must be understood that the displacement of these dots (in a vertical direction) from the undulating line U b b' are shown on a much larger scale than the displacement of the undulating line above and below A B, for the gain and loss of Uranus as compared with the mean planet are so much larger than the gain and loss due to perturbation, that these last must be shown on a much magnified scale to become apparent in any diagram in which the former are not shown very large indeed, which would not here be convenient.

(1389.) The displacements thus indicated up to 1805 or thereabouts, since they vanish near b and b', where the planet, according to its calculated orbit,

Fig. 429.—Diagram showing the displacement of Uranus relatively to a planet moving uniformly in a circular orbit.

would have been at its mean distance, cannot be attributed to an exterior planet. They showed that there was an error in the determination of the elements of the elliptical orbit. Clearly altering the estimate of the eccentricity of Uranus would not correct these deviations, for it would leave the points π_1, α_1, π_2, α_2, &c. unchanged, only altering the rise and fall of the undulating curve U b b' above and below the line A B. But it is equally clear that if the dots were put at their true vertical distances from the curve U b b' we should get another undulating line whose intersections with A B would fall slightly to the left of the points π_1, α_1, π_2, α_2, &c.; but as these vertical displacements indicate the amount by which the curve U b b' is to be *corrected*, we must measure off corresponding displacements below when a dot is above, and above when a dot falls below, and so we obtain an undulating curve whose points of intersection lie slightly to the right of π_1, α_1, π_2, α_2, &c. In other words, we can correct the periodic part of the displacement of Uranus in

longitude up to the first few years of the present century by shifting forward the perihelion and aphelion points. The amount of the change in the position of the perihelion was not more than some 3° in longitude.

This being supposed done, and a diagram like fig. 429 constructed with due reference to the changed position of the perihelion, the displacements indicated by the dots now set down in the same way as before are much reduced, and the periodic disturbance almost entirely vanishes, so long, at least, as we limit our attention to the time between 1690 and 1805 or thereabouts. Another though less marked peculiarity, however, is now noticed, viz. the circumstance, which can be recognised in fig. 429, that Uranus was in advance of his calculated place (as corrected by the change just indicated) before the year 1780, and slightly in advance of his calculated place after that time, his gradual loss with reference to his calculated place being tolerably uniform throughout the whole time from 1690 to 1805.

This manifestly showed that the mean motion of Uranus was slightly less than that which had been heretofore assigned him, or that the period of revolution thus far assigned was slightly short of the true value. It might at first sight appear that herein should at once have been found the suggestion of a disturbing exterior planet. For since the normal perturbing force produced by a disturbing exterior planet is in the main outwards, the effect must be to increase the period of revolution. If the distance of a planet were determined by direct observation of the planet, this inference would be just; but, as a matter of fact, the distance of the planet is inferred from the planet's orbital motion. So that it could not be inferred from the observed motion of Uranus that the planet was moving in a longer period than its observed mean distance required, but only that the observations of the planet's motion had not thus hitherto been so correctly made and dealt with as to indicate the true mean distance. The change was so slight that we can only regard it as corresponding to those progressive improvements in the estimates of the planetary elements which the continued progress of observation is bound to bring about. (The estimated mean distance of Uranus in 1835, before this correction was considered, though not before it *might* have been introduced, was 19·18239, the Earth's mean distance being called 1; the present estimate of the mean distance of Uranus is 19·18338.)

But from about the year 1805 the motion of Uranus, as fig. 429 shows (by the way in which the intersection of the curve of dots with U $b\,b'$ is thrown to the right or forwards of the place of mean distance b, instead of lying as before to the left or behind that place), was significantly accelerated. This acceleration continued until about the years 1821–25, and was then followed by an even more remarkable retardation. The actual acceleration

and retardation can be recognised from fig. 430, which shows by the displacement of the dots above and below the line A B how much Uranus was in advance of or behind his longitude, as calculated from the corrected orbit supposed to have been determined from the observations made up to 1805 or 6. It must be mentioned that the orbit was not thus corrected from the observations of those earlier years. Figs. 429 and 430 represent in reality the results of Leverrier's calculations made after 1840. But there is nothing in this part of Leverrier's work which might not, at least so far as the broad features of his results are concerned, have been obtained by Bouvard—the correction of the place of the perihelion and the mean distance in 1820, and the recognition of the nature of the acceleration and retardation before and after the conjunction of Uranus with the then unknown Neptune, certainly as early as 1830. As a matter of fact, Bouvard did not obtain the first-named corrections, and, until the place of the perihelion is corrected, the fact that Uranus was getting ahead of his calculated place rapidly before and up to 1822, attaining then his greatest advance, is not obvious (as fig. 429 shows). But even so he was able to recognise, and did clearly recognise, how rapidly Uranus was falling behind his calculated place after 1822, and this, noticed as it was by him, should have set him and all thoughtful astronomers of that day on an immediate comparison of the observed and calculated places of Uranus, such as Leverrier made nearly twenty years later, in which case, though doubtless they could not have obtained quite such close results as he obtained with his fuller knowledge of the perturbing action of Jupiter, Saturn, &c., they could have yet obtained results which, pictured as in fig. 430, would have taught to all intents and purposes the same lesson. Indeed, Leverrier was obliged after Neptune's discovery to admit that he had dealt with the observations of Uranus far more closely than their accuracy warranted. A much less precise treatment would have given him all the really trustworthy information they contained.[1]

Fig. 430. –Actual place of Uranus as compared with orbit calculated from observations up to 1805.

It must be noticed here that Bouvard's treatment of the perturbations

[1] It will suffice to show how little precise accuracy avails in such a case as this, where the most exact observations were necessarily (owing to the minuteness of the displacements in question) rough relatively, to mention that the displacements indicated in figs. 429 and 430 are really from Leverrier's estimated displacements of Uranus in geocentric, not in heliocentric longitude. Yet they give the desired suggestions as to the accelerating and retarding influences exerted on Uranus as significantly as if they indicated the true accelerations and retardations of Uranus in his orbit around the sun.

of Uranus up to 1820 was not calculated to help in suggesting the place of an external disturbing body. He practically gave up the problem by simply determining a path for Uranus from the then recent observations, 'leaving it,' as he said, 'to future time to determine whether the difficulty of reconciling the two series arose from inaccuracy in the older observations, or whether it depended on some extraneous and unperceived influence which may have acted on the planet.' Had he boldly confronted the problem as it stood even in 1820, and far more had he but a few years later dealt with it in resolute fashion, he could have pointed to the position of the disturbing planet much more effectively by a comparatively rough method of investigation—applied at that most favourable time—than Adams and Leverrier were able to do later, when with more observations to direct them they nevertheless had inferior opportunities for determining the actual position of the disturbing body. What Bouvard should have done was clearly to take the whole series of observations from 1690 to 1820, and, finding they could not all be represented by one orbit, to take that orbit which covered the greatest range of the planet's motion in longitude, remembering, as he could hardly help doing, that a planet disturbed by an exterior body shows the effects of disturbance markedly only over a small portion of its orbit. Had he done this, he would have been able to reconcile fairly all the observations from 1690 to 1810, leaving those from 1810 to 1820 to represent part (he could not as yet have told how much) of the more marked effects of perturbation which accrue when the disturbed planet is approaching conjunction with the disturbing body. In other words, Bouvard, had he adopted this bolder course, could unquestionably have announced as far back as 1820 that in all probability Uranus was approaching conjunction with an external planet. He could confidently have invited telescopists to sweep the ecliptical region in the direction wherein Uranus lay for a disturbing planet lying beyond. As he would have had every reason to say further that probably the external planet would show a disc when a power of from 200 to 500 was employed, there can be no doubt, seeing that the disc of Neptune is well shown with a power of 300, that within a few weeks, if not days, of the first observations in the neighbourhood of Uranus in opposition Neptune would have been found. For sweeping in advance of Uranus along the ecliptic, with ever-growing confidence (converted into certainty by 1822) that the planet was approaching heliocentric conjunction with an external disturbing planet, the observers would even in 1820 have had less than five degrees to sweep over in longitude before finding Neptune; while in 1822 Neptune was close to Uranus in opposition, and the two planets might even have been seen in one and the same telescopic field.

All this could have been surely and confidently achieved by the simple recognition of upward slope of the perturbation curve shown in fig. 430, had not Bouvard smoothed off that slope by assuming a path for Uranus which the laws of dynamics, as applied to planetary perturbations, assure us (and should have assured him) could not possibly be the planet's path, let the cause of the disturbance of Uranus be what it might. This opportunity, however, was missed, the best time for detecting the outer planet lost, and the problem carelessly left to a future and less favourable time, with very fair chance of losing Neptune altogether until the next heliocentric conjunction of the planets in the year 2000.

The motion of Uranus naturally did not conform itself to the impossible orbit by which Bouvard attempted to explain the most perturbed part of the motion of Uranus in company with only later observations of the planet. Year by year Uranus indicated a greater and greater falling away from the course which that orbit had assigned. The places, calculated from Bouvard's tables, lie along the dotted line shown in fig. 430; and it is seen that, although he used observations up to 1820, the deviation of the planet from his track is not only greater between 1820 and 1840, but sharper—that is, grows at a greater rate—than the deviation from the track obtained by combining old and recent observations up to 1810. By 1830 the displacement of Uranus from his calculated place (Bouvard's tables) amounted to 18″, in 1835 to 32″, in 1838 to 53″, in 1840 to 87″, in 1841 to 72″. From 1840 onwards they rapidly diminish, as the action of Neptune had become accelerative, and by 1845 Uranus was in the position which Bouvard's tables assign to the planet. But the retardation of the planet, though corrected by subsequent acceleration, was thereby only rendered the more significant.

It is worthy of notice that the maximum displacement of Uranus from the place calculated by Bouvard was but about $1\frac{1}{2}'$ of arc—about a twentieth part of the diameter of the moon, and about a tenth of the distance separating the middle star of the Great Bear's tail from the companion star by the middle Horse, which, as seen by the naked eye, looks so close. The displacement from the place which would have been calculated had the whole observed track of Uranus from 1690 to 1820 been employed would have been only half even of this. Yet to the astronomer this is a most marked degree of perturbation.

But for Bouvard's ingenious attempt to cut the Gordian knot by cutting loose the older observations the significance of the perturbations indicated in fig. 430 could not possibly have been overlooked. Long before 1830 it would have been recognised that the exterior planet had accomplished his full accele-

rative work, and had passed into the region where his work is as effectually retardative. That this was not recognised, even as the matter actually stood, is strange enough. It would seem as though most of the official astronomers took it for granted, despite his own words above quoted, that Bouvard had done the best that could be done to explain the observations up to 1820, and so came to regard the discrepancies observed after that time as newly accruing, instead of being the retardative compensation for an accelerative action which had already in 1820 nearly reached its maximum amount.

(1390.) Be this as it may, certain it is that none noticed the actual nature of the evidence thus masked through Bouvard's error of judgment. Dr. Hussey, in 1834, was moved by the observed discrepancies to suggest to the Astronomer Royal, Airy, the idea of sweeping for the exterior planet, if some mathematician would give him some notion of the place where he should look. But in accordance with the usual custom in such cases, official astronomy discouraged unofficial attempts to do the sort of work which the public expect to have accomplished in national observatories.[1] Hussey had talked the matter over with Bouvard, who agreed with him about the probable existence of an external planet, but intimated that he himself had been in correspondence with the mathematician Hansen, who had discouraged him by remarking that not one external planet but two must be at work to produce the observed perturbations.[2]

We can attach no value to the various guesses thrown out by several students of astronomy between 1825 and 1840. They serve to accentuate the remissness of astronomers, and especially official astronomers, in failing

[1] Airy's answer included the remarkable statement that 'he did not think the irregularity of Uranus was in such a state as to give the smallest hope of making out the nature of any external action on the planet'—if, which he doubted, there was any! This was in 1834, when even amateur telescopists recognised the discrepancies between the movements of Uranus and those calculated from Bouvard's tables. The Astronomer Royal did not, it appeared, know what Bouvard had done; for he seemed to think everything would be cleared up by simply assuming the earlier observations wrong—which Bouvard had already done before these recent discrepancies were noticed.

[2] One can easily understand Hansen's mistake on this point. Assuming, as he probably did (for want of due inquiry), that Bouvard's tables, which left the earlier observations hopelessly in error, indicated the nearest approach which could be made to the interpretation of the motions of Uranus, Hansen would naturally (and on that assumption justly) conclude that an exterior planet must exist which had produced those discrepancies between 1690 and 1780; the remarkable discrepancies observed after 1820 also clearly indicated the action of an external planet, but this planet could not possibly be the same which had produced the earlier disturbances. Hence, on this erroneous assumption as to the qualities of Bouvard's tables, two external disturbing planets must be sought for. If the recorded facts did not establish the circumstance that Bouvard's work was unfortunately conceived, this mistake of Hansen's would suffice to prove it. For a single disturbing planet would have been suggested by the discrepancies between the observed places up to 1830 and the places calculated from the orbit deduced before Bouvard made his attempt to explain the acceleration of Uranus from 1800 to 1820 by an orbit not reconcilable with the observations made before 1780. Moreover, the conjunction of that single planet with Uranus somewhere between 1820 and 1825 would have been not only suggested but clearly indicated, had not Bouvard's new tables too ingeniously explained away the acceleration up to about 1824–25.

quickly to recognise that Bouvard's tables were unequal to the task of explaining recent observations any better than the old ones he had thrown overboard. Bessel in 1842 actually entered on the examination of the problem, instructing Flemming, a subordinate, to reduce all the recent observations with great care. But Bessel shortly fell ill and died. Flemming also died soon after he had completed his calculations. Thus nothing came of this, the first resolute attempt to discover the position of the external planet.

Even now, though every year since 1820 the chance of finding the external planet had been diminishing, Neptune might easily have been found by a search conducted on such simple principles as Newton employed in dealing with the moon's motions. If the task of discovering Neptune had become by 1845 a difficult one—which is not in reality the case—it was only through systematic neglect of opportunities offered during nearly a quarter of a century.

Consider the discrepancies shown in fig. 429, noting that from 1805 onwards they were such as Bouvard knew of up to 1820, and afterwards to the day of his death in 1843, and as he must have represented practically, as shown in fig. 430, had he not unfortunately given up all attempt to reconcile the old observations. Clearly the heliocentric conjunction of Uranus with an external planet, which can alone explain the access of acceleration after 1805, must have occurred somewhere between the years 1820 and 1825, since the total acceleration attains its maximum between those years. The greater rapidity of the descending slope manifestly corresponds with the fact that throughout the years 1819 to 1840 Uranus was approaching aphelion, or nearing the orbit of the external planet, assumed not to have its aphelion in the same direction. The marked nature of the difference may be interpreted in more ways than one. If the orbit of the external planet is of small eccentricity, then Uranus in approaching aphelion must be approaching that exterior orbit ; but in this case the distance of the exterior planet must not be assumed too large, because then the approach of Uranus to that orbit in drawing near to aphelion would not diminish his relative distance from the orbit of Neptune effectually enough. In other words, Neptune's mean distance must be assumed small enough to make the eccentricity of Uranus relatively not too small, to explain the manifest increase in the action of Neptune after passing heliocentric opposition. *Or*, we may explain the peculiarity by assuming that while Neptune has such a mean distance as the law of Bode suggests (close on twice the mean distance of Uranus), the eccentricity of the orbit of Neptune is considerable, and situate nearly in the same direction from the sun as the aphelion of the orbit of Uranus. A natural mean course would have been to assume a mean distance somewhat less than

that given by Bode's law, and a perihelion situate nearly in the same heliocentric longitude as the aphelion of the orbit of Uranus. This could not have been suggested before 1830 or thereabouts, though in the meantime search could have been made quite effectively without any such suggestion, even more effectively in 1826, 1827, and 1828 than in 1830, 1831, and 1832.

Suppose that in 1826, taking time pretty fairly by the forelock, it had been assumed that the external planet, in heliocentric conjunction with Uranus between 1820 and 1825, was travelling at a mean distance equal to 38 (Earth's distance 1), or nearly twice that of Uranus. Let us see within what limits the search for the external planet would have had to be conducted. The mean angular velocity of such a planet round the sun would be about 1·54° *per annum* (the period of revolution close on 234 years). Uranus in the years 1820–1830 travelled with a mean velocity of 4·14° per annum. Hence the gain of Uranus in longitude on the imagined exterior planet would be 2·60° annually. If heliocentric conjunction took place in 1820, Neptune in 1826 would have been 15·6° behind Uranus; if such conjunction had taken place in 1825, Neptune would have been only 2·6° behind. Hence at the opposition of Neptune in 1826, the search, as determined by evidence actually gathered then, and limited by the natural, though entirely erroneous, assumption as to Neptune's distance, would have ranged from 2·6° to 15·6° behind Uranus on the ecliptic (or over 13° in longitude), and within $1\frac{1}{2}$° north and south of the ecliptic. Also with the assumed distance of the disturbing planet, a mass and probable volume must have been assumed which would have led astronomers to look for a disc readily distinguishable with a power of 300 from the point-like appearance of a star.

As a matter of fact, Neptune in 1826 was 6·2° behind Uranus in longitude, or within less than 3° of the place thus indicated. He would have been found, supposing the survey began in the middle of the arc of 13° and worked towards the ends, within a few days at the outside from the commencement of the search.

Next suppose the search only began in 1835, by which time the period of heliocentric conjunction could have been better determined, and the relative nearness of the disturbing planet inferred, as shown above. It is hardly necessary to make any exact calculation for this case, or rather for the three cases between which we have to choose. The motion of about a degree and a half per annum is increased in much the same proportion whether (1) we choose a smaller orbit, or (2), assuming the orbit of Neptune to be considerably eccentric, regard the perihelion as nearly in the same heliocentric longitude as the aphelion of Uranus, or (3) combine both considerations. In any one of these three cases we find our assumed Neptune travelling in this part of

his orbit at the rate of about 2° per annum. Hence, if we assume the heliocentric conjunction of Uranus and Neptune to have occurred in 1821, we determine for Neptune in 1835 a place 14 times 2·14° (that is, 4·14° − 2°), or 29·9°, from Uranus ; whereas, if the conjunction is supposed to have taken place in 1824, we find for Neptune a place 11 times 2·14°, or 23·54°, behind Uranus. We thus have a range of 29·96° − 23·54°, or only 6·42°, in longitude over which to look for Neptune. As a matter of fact, Neptune in 1835 was 24·6° behind Uranus, or only 2·15° from the middle of the arc of the ecliptic thus indicated. He would have been discovered in a few hours by his disc if sought for by sweeping on either side of the middle of the 6½° range thus indicated in 1835.

At any time from 1820 to 1850, but probably best about the year 1830, Neptune might have been looked for with certainty of discovery in a few days at the outside by thus determining by the simple Newtonian way of dealing with the observed displacements a certain arc, first on either side of Uranus in opposition, but after 1825 within measurable limits behind—that is, west of—Uranus, over which to search with sufficient telescopic power for a planet with a disc. If at the beginning of the period only imperfect guesses could be made as to the mean distance of the disturbing planet, that was compensated by the assurance that he could not long have passed heliocentric conjunction with Uranus. Later the marked peculiarities recognised on comparing the size and shape of the curves shown in fig. 430 must have forced on inquirers the conclusion that Neptune must be nearer than had first been supposed, and, though after 1840 the arc over which search would have to be made would have grown longer on account of the planet's retreat from the favourable position of heliocentric conjunction, yet the growing conviction that the disturbing body must be nearer, and consequently losing less year by year, as compared with Uranus, than had been supposed, would still keep the arc requiring survey within manageable limits of length. Perhaps five hours' search at the opposition of Uranus in 1830 would have ensured the discovery of Neptune ; in 1850 the search, if guided only by such simple considerations as I have dealt with here, might have lasted during ten or twelve weeks before the opposition of Uranus (since Neptune would by this time be considerably to the west of Uranus). But during the whole quarter of a century the problem in the simplest form which it presents, as dealt with by Newton's geometrical method of considering perturbations, was altogether manageable, whether we consider the discussion of its details or the work of search and survey suggested by the results of such discussion.

(1391.) It was not, however, until 1844 that the work of which several students of astronomy had thought was actually entered upon. In 1844

Professor Challis applied to the Astronomer Royal for some Greenwich observations of Uranus, as a young friend of his, Mr. J. C. Adams, of St. John's College, Cambridge, was at work on the theory of Uranus. In September 1845 Professor Challis wrote again, saying Mr. Adams had completed his calculations. A month later Mr. Adams left at Greenwich a paper giving the result of his calculations, and stating where the exterior planet should be looked for.

This was a startling announcement for the man who only ten years before had deprecated the whole inquiry as idle and unnecessary, and he replied with what he intended as a test question: Could Mr. Adams account for the radial perturbation of Uranus? It was absolutely certain that the motion of Uranus in longitude was the really critical and determining perturbation, so far as the observations which Mr. Adams had dealt with were concerned. His work might not directly have accounted for changes in the distance of Uranus from the sun.

Unluckily the Astronomer Royal's perturbation produced a strongly retardative effect on his young teacher. It has been said that Mr. Adams ought not to have felt discouraged. But it must be remembered that the Astronomer Royal had twice his years and a hundred times his name. Besides, an official or professional career in astronomy might have been fatally obstructed by obstinate persistence in being better informed than the most influential official astronomer in the country. Let the reason have been what it may, Mr. Adams obviously was discouraged. He *had* dealt with the radial perturbations of Uranus, and could most fully have answered the Astronomer Royal's question. This is certain; but it is equally certain that he never did.

In the meantime M. Leverrier announced to the French Academy, in June 1846, that he had gone through a series of calculations for determining the path and movements of an exterior planet, and had found for such a planet a position corresponding quite closely—to the Astronomer Royal's amazement—with Mr. Adams's result.

Professor Challis, who had earlier been informed that the Astronomer Royal considered a search for an exterior planet would not have any chance of success unless a range of 30 degrees were swept over, began now to trust so far in Mr. Adams's work as to look for the planet where the young astronomer had pointed. But he did this in so half-hearted a way that, while he actually twice saw Neptune, he did not recognise the planet as such, either by its disc (which with the Northumberland telescope should have been obvious) or by its motion. Some wretched comet which he thought it his more important duty to watch prevented him from making the reductions which would have

shown him that the exterior planet had twice been recorded in his notes of observations. So much routine industry and so little of the common sense of genius is there in too many official observatories!

Dr. Galle, of Berlin, so soon as M. Leverrier pointed out the probable place of the external planet, went to work in a different fashion. To use Lord Grimthorpe's quaint words, Galle 'not only found the planet, but found that he had found it,' on September 23, 1846.

The work of Leverrier and Adams may be described as simply the analytical treatment of the problem whose geometrical nature has been indicated in the preceding pages.

(1392.) Suppose a certain path as $A_1A_2A_3$ (fig. 431) assigned to the exterior planet, with an assumed mean distance, eccentricity, mass, and epoch. Let the effect of starting this exterior planet from A_1, when Uranus was at U_1, be considered first in the geometrical way. We know from the laws of Kepler (with sufficient exactness for this rough preliminary work) how to determine the places A_2, A_3, A_4, &c., occupied by the exterior planet when Uranus is at U_2, U_3, U_4, &c., respectively. Then by the construction illustrated in art. (1386), we can determine the relative magnitude of the radial and tangential forces at all these stages, and as many intermediate simultaneous positions of the two planets as may be deemed necessary. Nor is there the least difficulty in determining geometrically in Newton's manner the amount of tangential acceleration or retardation and of radial disturbance produced by these disturbing forces, assumed constant during the intervals between the successive simultaneous positions dealt with. We thus obtain, in an hour or two, perhaps, a good though rough means of estimating the perturbations which would arise did an external planet move in the path $A_1A_2A_3$, &c., in the way supposed, having the assumed mass. We can also readily recognise the effect of changing the mass or changing the position of the exterior planet in its assumed orbit. Two or three hours would suffice to indicate all the possibilities of an orbit thus selected tentatively, because the observed accelerations and retardations of Uranus would limit us to such movements of the external planet as would bring it into heliocentric conjunction with Uranus somewhere between the years 1820 and 1825 inclusive.

Guided partly by the results of such a first experiment, which, of course, would be far from satisfying the conditions of our problem, we should then try another orbit, as, for example, the path $L_1L_2L_3$, &c., with which we should deal in a similar way, though probably in a shorter time.

So might we try the effects of many selected orbits with various mean distances, eccentricities, perihelion points, epochs, masses, &c., until gradually we began to recognise with tolerable clearness what the conditions of the

problem required. I think it likely also that this geometrical process would impress rather strongly on any mathematically-minded man who tried it the unlikelihood that refined analytical methods could be employed with advantage on a problem of this nature. I fancy that a Newton, after a few preliminary experiments such as I have imagined, would have been as likely to apply the delicate methods of modern analysis to this problem as a mechanician would be to employ needle-point augers for work needing a centre-bit.

Of course, analysis lends itself to an inquiry of this kind very effectively up to a certain point. If we have surveyed a space, whether with a theodolite and delicate measuring instruments or in a comparatively rough manner, we can always derive more exact inferences from the trigonometrical treatment of our survey than from the construction of triangles geometrically formed with the measured sides and angles. The analytical investigation of the inverse problem of planetary perturbations, though altogether more complex than that of any surveying problem, has similar advantages over any geometrical treatment. In particular we can obtain the actual effects of perturbative action for particular assumed orbits; we can obtain general results whose varying values for varying assumptions of the elements of the disturbing planet are indicated by one and the same set of formulæ. Then there are methods when our equations of condition have been duly formed for deducing the most probable solutions, which, though they have their geometrical analogies, are practically outside geometrical work. There is a certain satisfaction in applying these delicate processes even to observations and assumptions which we know to be by comparison exceedingly rough, just as an ingenious surveyor might conceivably take pleasure in examining with a microscope to thousandths of an inch measurements which he knew to be probably in error by tenths or even by full inches.

At any rate, such were the processes applied by Messrs. Adams and Leverrier to the problem they had taken in hand. How masterly their treatment of the analytical formulæ involved none can appreciate but those who have examined their work with sufficient knowledge of the principles involved to recognise its excellence. How suitable or unsuitable these delicate methods were to the problem in its actual condition may be judged by the circumstance that, while Adams found the observations from 1840 to 1845 compelling him to change his estimate of the exterior planet's distance from 37·2474 to 33·33 (the Earth's mean distance being 1), Leverrier, who had already employed those observations, was forced by his analytical work to the conviction that the mean distance of the exterior planet could not possibly be less than 35. (The actual mean distance of Neptune is 30·05.)

The paths $A_1A_2A_3$ and $L_1L_2L_3$ (fig. 431), used for convenience to illustrate hypothetical paths for Neptune, are in reality those in which Adams and Leverrier respectively were led by their analytical researches to conclude that Neptune was moving, while Uranus was moving along the path $U_1U_2U_3$. The real Neptune moved along the track $N_1N_2N_3$. The dated lines connecting U_1N_1, U_2N_2, &c., serve to show precisely how the two planets were situated at the times when the perturbations indicated in fig. 430 were produced ; and by comparing the lengths of the connecting lines at equal time distances before and after the heliocentric conjunction of the two planets, one can see why the retardative action after conjunction was (as fig. 430 shows) so much more marked than the accelerative action before conjunction. It will be observed that both Adams and Leverrier assigned movements to their several planets accounting fairly for this peculiarity. (It is also worthy of notice how the positions of the two false planets approach each other towards the year 1846 ; in heliocentric longitude Neptune was in actual agreement with Leverrier's planet about the year 1841, and would have been in agreement with Adams's planet about 1850.) On the whole, considering his opportunities—for Leverrier had command of ten times as many observations of Uranus—Adams did his work more satisfactorily than Leverrier. His reduction of the mean distance of Neptune to 33·33, three weeks before the planet was discovered, indicated a much greater mastery of the problem than Leverrier had obtained.

The actual paths assigned by Adams and Leverrier to their respective planets may be compared with the path of the real Neptune, either from the following elements or more simply by examining fig. 431 :—

	Neptune	Leverrier	Adams
Epoch of elements	Jan. 1, 1847	Jan. 1, 1847	Oct. 6, 1846
Mean longitude	328° 32′·7	318° 47′·4	323° 2′
Mean distance	30·054	36·1539	37·2474
Eccentricity	·00899	0·107610	0·120615
Longitude of perihelion	46° 9′	284° 45′·8	299° 11′
Mass (sun's as 1)	0·00005160	0·00010727	0·00015003

I have not thought it necessary to refer to the question of the relative claims of Messrs. Adams and Leverrier, deeming such matters beneath the dignity of science. The facts simply are that the planet was first recognised as a result of Leverrier's announcement ; earlier observed, but not recognised, as a result of Adams's earlier announcement ; and that what Airy described as 'quite within probability' was absolutely certain—that, had proper attention been given to Mr. Adams's letter of October 1845, the planet would have been discovered in November 1845, or seven months before Leverrier announced his results.

Of the attempts made on the other side of the Atlantic to show that the discovery of the planet was in reality accidental, because the orbits assigned to the supposed planet by both Adams and Leverrier differed so remarkably from the actual orbit, the less said the better. A study of fig. 431 will show that both Adams and Leverrier deduced a planet whose perturbing work during the time over which their survey extended corresponded well with the actual perturbing work of Neptune, and sufficed to account for the greater part of the observed perturbations, themselves small quantities. Leverrier's mistake in setting 35 as the least admissible mean distance was certainly unfortunate (it resulted wholly from a weakness of the analytical method), yet does not affect the value of his actual result. Adams was clearly on the way to a rapid reduction of the mean distance. Both dealt with the problem admirably, and, though the American mathematicians who decried their work were able analysts, it is manifest from their discussion of the work of Adams and Leverrier that they had not clearly grasped the nature of the problem which the European mathematicians had attacked.[1]

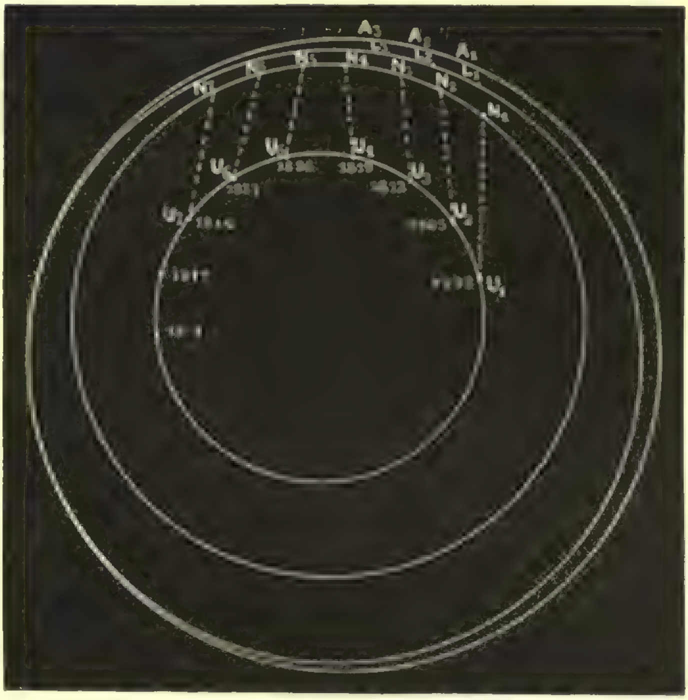


FIG. 431.—Orbits of Uranus and Neptune drawn to scale: A_1 A_2 A_3 orbit of Neptune as assumed by Adams. L_1 L_2 L_3 orbit of Neptune as assumed by Leverrier. N_1 N_2 N_3 actual orbit of Neptune. U_1 U_2 U_3 actual orbit of Uranus.

The success achieved by both Adams and Leverrier is not affected at all by a discrepancy which in reality only serves to indicate the difficulty of the analytical problem they had to deal with. That a Newton would have dealt with it in a more summary if in a more rough-and-ready manner, and would more quickly have achieved success, may be admitted, and is, in fact, im-

[1] I refer particularly to the remarks of the American mathematicians on the effects of the commensurability of the periods of Uranus and Neptune. These effects which, like those indicated in the great inequality of Jupiter and Saturn, become very marked in long periods of time, have absolutely no bearing whatever on the problem with which Messrs. Adams and Leverrier undertook to deal.

plicitly indicated by the real roughness of the result they obtained by methods uselessly delicate. Science may regret her failure to achieve success until at least a dozen years after success might readily have been obtained. Some sense of dissatisfaction at the supineness of so many most capable astronomers, provided with abundant means, opportunity, and leisure for just such work, may be suggested by the story of the late discovery of Neptune. But neither Leverrier nor Adams could earlier have dealt satisfactorily with the work. Adams was little more than a lad when he undertook it ; Leverrier had been engaged for years in most laborious work of astronomical calculation, of which the study of the perturbations of Uranus was but an incident. It need hardly be added in conclusion that the discovery of Neptune solely from the investigation of the perturbations produced by his attraction on the planet Uranus was a triumphant demonstration of the truth of the law of gravity. To Newton alone this part—and it is far the greater part—of the credit of the discovery must unhesitatingly be assigned.

(1393.) Neptune is not visible to the naked eye ; it shines as a star of about the $8\frac{1}{2}$ magnitude ; with sufficient telescopic power it shows a greenish disc of about $2''\cdot6$ in diameter. No markings have been seen upon its surface, and nothing is known of its rotation period. The actual diameter of the planet is given as 34,500 miles, with a probable error amounting to perhaps as much as 1,000 miles either way. Its volume is nearly 82 times as great as that of the Earth. Its mass, as determined by the motion of its satellite, is about 18 times that of the Earth, and its density about ·22, or a little more than a fifth of that of the Earth. The albedo of the planet is a trifle lower than that of Saturn, and its spectrum has been reported to be like that of Uranus crossed by dark bands ; but the light of the planet is so faint that much reliance must not be placed on such observations.

Neptune has one satellite, discovered by Lassell within a few weeks of the discovery of the planet. It is too small to show a disc, but, judging from its brightness, it is probably a little larger than our own moon, and its distance from its primary (223,000 miles) does not differ greatly from that of the moon from the Earth. But, owing to the greater mass of Neptune, it accomplishes its journey round the planet in $5^d\ 21^h\ 2^m\cdot7$. Its orbit is inclined $34^\circ\ 53'$ to the ecliptic, but the satellite moves backwards—that is, in a contrary direction to the motion of its primary round the sun—as is the case with the satellites of Uranus (see 1380, fig. 423).

CHAPTER XVII.

THE STARS.

(1394.) Vast as are the distances we have been considering, they dwindle into insignificance compared with the space which separates the sun's domain from the nearest body which shines as a star.[1] One of the first objections raised against the hypothesis of Copernicus as to the Earth's motion round the sun was, that the stars do not appear to move as they should do if the Earth moved. It was readily seen, even by objectors with very little geometrical knowledge, that the Earth's motion round the sun involved such an enormous displacement of the spectator's place during the course of the year that the stars ought to appear to shift their places. The nearer stars should be seen to move with respect to more distant stars laying behind them, as the Earth passed from one side of its orbit to the other; but no such movement could be detected. The genius of Copernicus had foreseen the objection, and he had suggested the true answer. He attributed the absence of all sensible motion to the immense distances of the stars compared

[1] There may be dark bodies much nearer to us than the nearest star, but their presence has not as yet been detected by any recognised perturbation of the motions of the outer planets. The value of this negative evidence has perhaps been somewhat overrated by some writers. The presence of a dark attracting body comparable with the sun in magnitude within a distance of us 1,000 times as great as that between the Earth and the sun would probably only be detected by reason of the perturbations caused if it were in the neighbourhood of the plane in which the planets move. And the presence of such a mass at a distance from the sun of 5,000 times the Earth's distance would probably remain undetected even if it were in the plane of the ecliptic, for the magnitude of the perturbations depends on the difference of the action of the perturbing mass on the sun and on the planet perturbed. Even in the case of Neptune, with a mean distance 30 times the Earth's distance, the difference of the accelerations produced by the disturbing mass on the planet and on the sun when they are most favourably placed would be less than one-eightieth of the whole acceleration, and the consequent perturbation of the planet would be less than a thousandth part of that caused by the action of Neptune upon Uranus when at its nearest.

A dark body as large as the sun at a distance of only 600 times the Earth's distance from the sun would probably be overlooked, for it would only have a diameter of 3″, that is, it would present a disc only a trifle larger than that of Neptune, and would be illuminated by sunlight of about one-four hundredth of the intensity of that which falls upon the planet Neptune. Assuming the dark body to have a similar albedo to that of Neptune, it would shine by the reflected light of the sun as a star of about the fifteenth magnitude of Argelander's scale.

with the diameter of the Earth's orbit. But such an answer required for its comprehension too great an effort of the imagination for his opponents.

Even Tycho Brahé was staggered and unable to accept the otherwise attractive Copernican theory of the Universe, by reason of the strain which it put upon his imaginative powers. Tycho Brahé had, by a long series of observations with instruments more perfect than had hitherto been used, determined the places of the larger stars with a probable error of about a minute of arc, and he had satisfied himself that he could not detect with his instruments any annual displacement of the stars. On the other hand, he estimated that stars of the first magnitude had an apparent diameter of two or three minutes ;[1] and he saw that if the Copernican system were true, this necessarily involved stars of the first magnitude exceeding in dimensions the whole amplitude of the Earth's orbit—a conclusion which appeared to him to be palpably absurd and sufficient to justify the rejection of the Copernican hypothesis.

(1395.) On the invention of the telescope it was seen that the large star discs which Tycho Brahé and other naked-eye observers had assumed to correspond to the real magnitudes of the stars were optical illusions. Galileo satisfied himself that the apparent diameter of α Lyræ did not exceed five seconds of arc, and Horrocks proved that their diameter could only be a very small fraction of a second of arc. He observed, with his friend Crabtree, an occultation of the Pleiades by the moon on the evening of March 19, 1639 ; they both noted that the stars were obliterated almost, if not quite, instantaneously by the moon ; and Horrocks remarked that, as the moon moves in the heavens through only about half a second of arc in a second of time, the real diameter of the stars must be a very small part of a second of arc.

Galileo was fully alive to the importance of this objection to the Copernican hypothesis, and felt convinced that some relative motion or parallax [2] of the stars would be observed. He proposed in the third of his dialogues on the systems of the world that pairs of stars lying close together should be observed—a proposal which gets rid of complications more numerous than Galileo was aware of, and reduces the problem to one of careful micrometrical measurement. The same idea suggested itself to several subsequent astronomers, but it was not till this century that the parallax of a star [3] was

[1] Tycho Brahé, in his work on the new star of 1572, gives his estimates of the diameters of the six magnitudes visible to him, and makes deductions as to their actual magnitude (*De affixarum stellarum veris magnitudinibus. Progymnasmata*, p. 481).

[2] The parallax of a star is the angle which the mean radius of the Earth's orbit about the sun would subtend as seen from the star's place. The greatest shift of the star's place due to the change of the observer's place as he is carried round the sun is equal to about double the parallax.

[3] The parallax of α Lyræ from observations made in 1835–38 by W. Struve, and the parallax of 61 Cygni from observations made in 1837–38 by W. Bessel.

actually determined by this obviously simple method. The proposal, however, proved fruitful in another and unexpected manner. Sir William Herschel, in order to carry out his plan of sounding the heavens, set himself to collect suitable double stars which were, at that time, supposed to be merely optically double, because they laid near to the same line of sight. Though he did not succeed in finding any parallactic displacement, he was rewarded by the discovery of evidence in several cases of an actual physical connection between the stars, proving them to be binary systems in motion round a common centre of gravity.

The early history of the attempts to detect a parallax or annual shift of the places of the fixed stars is admirably set out in the classical memoir of Dr. C. A. F. Peters, published in the volume of 'Mémoires' of the St. Petersburg Academy for 1853. It brings the history of the research up to the year 1842. There is also an excellent short *résumé* of the earlier attempts to obtain evidence of an annual parallax in Prof. Robert Grant's 'History of Physical Astronomy.' No trustworthy evidence of stellar parallax seems to have been obtained till after the commencement of this century.

The discovery of parallax is one of the few triumphs of the deductive method. Most of the discoveries of astronomy and physical science are fruits of the inductive method; that is, phenomena have been observed and theories have been framed to account for them. Probably the discovery of Neptune from the perturbations of Uranus, and the conical refraction of light by biaxial crystals predicted from the equation to the wave surface, are the two other most notable cases in which important additions have been made to our knowledge by the deductive method.

When the facts to be observed lie in the border-land which limits our powers of perception, the inductive method is much the safest to pursue, for the new truths that are acquired by it are founded upon facts which have forced themselves upon our attention as being contrary to what might be expected. The mental process followed in making a discovery by the inductive method is generally at first to doubt the observed fact, then to become convinced of its existence, and then to hit upon the true explanation.

On the other hand, the deductive method has not infrequently led to supposed confirmations of theory which have afterwards been found to be doubtful, and to other supposed confirmations which have subsequently been found to be explainable in quite a different manner. Thus the early history of the search for stellar parallax is a most interesting history of astronomical mares' nests.

One observer after another discovered evidence of an annual shift in star

places, which was either not confirmed by others or was shown to be due to some other cause than parallax. One of the first of such discoveries was made by Christopher Rothmann, Astronomer to the Landgrave William of Hesse, who found a difference of 1·5 minute of arc between the latitude of the Observatory of Cassel as determined in the summer and in the winter. He also thought that he had shown that the distances between certain stars varied as much as two minutes of arc in the course of the year ; and he relied on these observations as proving the Earth's motion round the sun. But Tycho Brahé, with superior instruments, was unable to confirm these observations, and he attributed the differences between Rothmann's[1] summer and winter observations to variations in the instruments employed due to the differences of temperature.

(1396) The ingenious Robert Hooke thought to get rid of difficulties due to changes of refraction at different periods of the year by selecting for observation the star γ Draconis, which passed within a few minutes of the zenith of his observatory at Gresham College, in the City of London. From four zenith distances of the star as observed by him in the months of July, August, and October of the year 1669, he found a shift of 22 to 24 seconds in the place of the star,[2] which he concluded corresponded to an annual parallax of between 27″ and 30″. Twenty-six years later. Molyneux and Bradley, with a view of verifying the interesting result announced by Hooke, undertook a similar series of observations with a zenith sector. They soon found that the star had an apparent motion at right angles to the direction which should have resulted if the effect had been occasioned by parallax. The star continued to advance until it attained a distance of 20″ from its original position ; it then turned, and, in the course of a year, returned again to its original position. At first Bradley thought that the phenomenon might be due to a nutation of the Earth's axis ; but he undertook a series of observations of other stars which soon showed that the changes could not be due to an annual shift in the direction of the Earth's axis, or to an annual shift, as he had thought might be possible, in the direction of the plumb line. By a happy inspiration he ultimately recognised that the observed phenomena might be completely accounted for by the aberration of light caused by the Earth's motion in its orbit, as compared with the velocity of propagation of the light emanating from the star.

[1] See Dr. C. A. F. Peters's *Mémoire* above referred to, p. 6.

[2] *An Attempt to Prove the Motion of the Earth from Observations made by Robert Hooke*, 4to. London, 1674. This curious volume contains a picture of Hooke's zenith sector. An object glass of 36 feet focal length was fixed in a square tube of wood passing through the roof of his house, and the image of the star was observed from the basement, through a hole in the floor of the upper story, and through the tube in the roof Hooke states his belief that this was the first occasion on which a star had been seen with a telescope in the daytime.

Thus another important truth was inductively discovered whilst searching for confirmation of a truth which had been deductively surmised, but the evidence of which was still far beyond the range of accuracy attainable with the instruments then used. A star is displaced by aberration in the direction of the tangent to the Earth's orbit, but it is displaced by parallax in the direction of the radius vector towards the sun. The displacement caused by aberration is very large compared with that caused by parallax. The Earth, as it were, drives the star before it in the aberrational orbit, while parallax slightly shifts the star towards the sun's place. Thus the aberrational orbit of a star situated in the pole of the ecliptic is an ellipse of similar eccentricity to the Earth's orbit, and about 41″ in diameter, while a star in the ecliptic is caused by aberration to oscillate backwards and forwards in a straight line 41″ long. These apparent motions have to be eliminated before the effect of parallax can be detected by comparing observations of the absolute places of a star. As the parallactic orbit is in no instance 2″ in diameter, the importance of properly eliminating the displacement due to aberration will be evident.[1] It is curious that till 1838 all the determinations of stellar parallax depended upon comparisons of the absolute places of stars, which involve not only the elimination of the apparent shift in the star's place due to aberration, but also corrections for the shifts due to nutation, precession, and the proper motion of the star, as well as sometimes a correction for irregularities discovered in the proper motion.

(1397.) Since the beginning of this century (and notably in the half century which has nearly elapsed since 1842, the epoch at which Dr. Peters's survey of parallax work ceases) the determinations of stellar parallax have not been quite as contradictory as they previously were. The diagram given as Plate XXVI. is intended to bring before the mind, in a graphic form, the results of parallax investigations which have been published in the ninety years since the beginning of this century. It has been prepared from a very complete list of the results of parallax investigations collected by Mr. Sadler and published in the February number of 'Knowledge' for 1890. Our own sun is supposed to be at the centre of the plate. A distance of about half an inch (one-tenth of the radius of the outer circle) represents the distance of a star with a parallax of one second of arc. A distance of about one inch from the centre corresponds to a parallax of half a second, two inches to a quarter of a second, three inches to

[1] The amount of the aberrational displacement depends upon the velocity of the Earth and the velocity of light: according to the latest determination of Newcomb and Michelson, the velocity of light is 186,330 miles ± 20 miles. The mean velocity of the Earth in its orbit, if we assume solar parallax to be 8″·8, is 18·50 miles. This makes the constant of aberration 20″·480, a quantity which will vary with every change in the adopted velocity of light, and in the adopted value of the Earth's mean distance from the

0·16 of a second, four inches to 0·125 of a second, and the stars on the outer circle are those that have a parallax of one-tenth of a second or less. Where more than one value of the parallax has been obtained, a line is drawn through the star joining the various determinations, and the star disc is placed in a position intended to indicate different weights for the various determinations, some of which are evidently much more trustworthy than others.[1]

(1398.) It will be seen that, with the exception of α Centauri and 61 Cygni, there are very few stars with respect to which there is any marked concurrence between the determinations of parallax obtained by different observers, and even in the case of α Centauri and 61 Cygni the determinations are far from grouping themselves within the narrow limits which we might be led to expect from the estimates of the different observers of their own probable error.

This is probably due to the fact that observers have been endeavouring

[1] In order to illustrate the meaning of the diagram it may be well to extract from Mr. Sadler's catalogue the various parallax determinations which have been published of α Centauri and 61 Cygni:

α CENTAURI. 14h. 32·1m. -60° 23′ (0·7 magnitude). HENDERSON (1832-1833), mural circle of 4-inches aperture, transit of 5-inches aperture. Absolute. (Mem. R. A. S. vol. xi. p. 68) + 1·14″ ± 0·11″. HENDERSON-MACLEAR (1839 1840), two mural circles of 4-inches and 5-inches aperture. Absolute. (Mem. R. A. S. vol. xii. p. 370) + 0·913″ ± 0·064″. PETERS-MACLEAR, a rediscussion of Maclear's results. Absolute. (Peters. Recueil de Mém. des Astr. de Poulkova, vol. i. p. 63) + 0·976″ ± 0·064″; with other corrections + 0·49″ (*loc. cit.*). MŒSTA (1860–1864), transit circle of 6-inches aperture. Absolute. (A.N. No. 1688) + 0·880″ ± 0·068″. MŒSTA (1860–1864), a fresh determination. Absolute. (A.N. No. 2349) + 0·521″ ± 0·066″. ELKIN-MACLEAR (1880*), rediscussion of Maclear's results. Absolute. (Über die Parallaxe von α Centauri) + 0·512″ ± 0·080″. GILL (1881–1882), heliometer of 4¼-inches aperture. Relative. (Mem. R. A. S. vol. xlviii. part i.) + 0·76″ ± 0·013″. ELKIN (1881–1883), same instrument. Relative. (*Loc. cit.*) + 0·676″ ± 0·027″.

61 CYGNI. 21h. 2·0m. + 38° 13′ (5 magnitude). ARAGO and MATHIEU (1812), repeating circle of (?) inches aperture (no details are given). Absolute. (Annuaire du Bureau des Longitudes, 1834, p. 281) + 0·55″. Baron von LINDENAU (1812–1814), transit instrument of 3¾-inches aperture. Absolute. (Recueil de Mém. des Astr. de Poulkova, vol. i. p. 48), reduced anew by Peters, + 0·47″ ± 0·51″. Von Lindenau himself found no trace of parallax. BESSEL (1815–1816), transit instrument of 2¾-inches aperture. Absolute. (Peters, *loc. cit.*) – 0·88″ ± 0·19″. BESSEL and SCHLÜTER (1837–1840), heliometer of 6¼-inches aperture. Relative. (Peters, *op. cit.* p. 60) + 0·3483″ ± 0·009″. (Bessel from the first series (1837–1838) found + 0·3136″ ± 0·014″.) PETERS (1842 1843), vertical circle of 6-inches aperture. Absolute. (Peters, *op. cit.* p. 136) + 0·349″ ± 0·080″. JOHNSON (1852–1853), heliometer of 7½-inches aperture. Relative. (Radcliffe Observations, vol. xiv. p. xxxix.) + 0·402″ ± 0·016″. O. STRUVE (1853), refractor of 15-inches aperture. Relative. (St. Pétersbourg Acad. Mém. vol. vii. p. 51) + 0·506″ ± 0·028″. (Absolute. + 0·493″.) C. A. F. PETERS (1854*), rediscussion of Bessel's and Schlüter's results. Relative. (A. N. No. 866) + 0·360″ ± 0·012″. AUWERS (1860–1862), heliometer of 6¼-inches aperture. Relative. (M. N. vol. xxiii. p. 75) + 0·566″ ± 0·016″. BELOPOLSKY-WAGNER (1862–1870), meridian circle of 6-inches aperture. Absolute. (A. N. No. 2888) mean + 0·525″ ± 0·093″. SCHWEIZER-SOCOLOFF (1863–1866), refractor of 10¾-inches aperture. Relative. (Annales de l'Observatoire de Moscou, vol. viii. part ii. p. 90) + 0·4330″ ± 0·2091″. Sir R. BALL (1877–1878), refractor of 11¾-inches aperture. Relative. (Dunsink Observations, part iii. p. 27) + 0·465″ ± 0·049″. Sir R. BALL (1878), as above. Relative. (Dunsink Observations, part v. p. 166) + 0·468″ ± 0·032″. GLASENAPP (1880*), rediscussion of Peters's results. Absolute. (Refraktsionnya Koklony) + 0·430″ ± 0·049″. HALL (1880–1886), refractor of 26-inches aperture. Relative. (Washington Observations, 1883. App. ii.) + 0·270″ ± 0·010″. PRITCHARD (1886–1887), by means of photography with a reflector of 13-inches aperture. Relative. (M.N. vol. xlvii. p. 445) mean + 0·432″ + 0·437″ (1886–1887), same method and instrument. (Oxford University Observations, part iii.)

to measure quantities much smaller than they can see. According to the estimate of the clear-sighted Dawes, it requires a very perfect instrument and a very perfect night to permit an observer with a telescope of 4·56 inches aperture to divide a double star with a separation of a second of arc. Theoretically, it would require a telescope of 45½ inches aperture (*i.e.* considerably larger than the Lick telescope) to separate a double star with a distance of a tenth of a second between its components. Practically, the limit of actual division of star discs for such large telescopes has not advanced beyond a fifth of a second of arc. But many parallax observers using comparatively small instruments have been relying on their measures to much smaller fractions of a second. Thus Gill gives the probable error of his determination of the parallax of α Centauri with a 4¼-inch heliometer as $\pm 0{\cdot}013''$, or about an eightieth part of a second of arc, with a telescope which, according to Dawes, would not divide a double star with a distance of two seconds. For the two halves of the object glass of such a heliometer would, for dividing purposes, be at most equivalent to two 2⅛-inch telescopes.

The mistake made by such observers is in assuming that the law of probable error will hold with regard to their estimates of quantities smaller than they can see as accurately as it does with regard to quantities of which they can estimate the magnitude with some reasonable degree of probability. It has been amply proved by Quetelet, Francis Galton, and others, that all physical quantities which vary in magnitude, such as the heights of individuals in a nation, or the weights of seeds, arrange themselves with respect to the mean height or mean weight according to the law of probable error; but no one has shown that a man's guesses will arrange themselves according to a curve of probable error. In fact, a little consideration will show that it is very improbable that they should be capable of arrangement in so regular a manner.

The ordinary method of treating multiple observation gives a probable error for the result which, theoretically, goes on decreasing as the number of the observations is increased; but it seems probable that the method can only be relied on while we are dealing with quantities large enough to affect the senses.

An examination of the diagram indicates that there is only a concurrence between the parallax determinations of different observers which about corresponds to the magnitudes their instruments enable them to perceive. With the exceptions of Prof. Hall's measures with the Washington 26-inch telescope, all parallax work has been done with comparatively small instruments. Of the two stars whose distances seem to be best determined, 61 Cygni and α Centauri, there is a difference between the results of Hall and Auwers for

61 Cygni of more than three-tenths of a second, which corresponds to an annual shift upon the heavens of more than six-tenths of a second; and in the case of α Centauri there is a difference between the results of Henderson and Elkin-Maclear of more than half a second, which would correspond to a shift of more than a second upon the heavens.

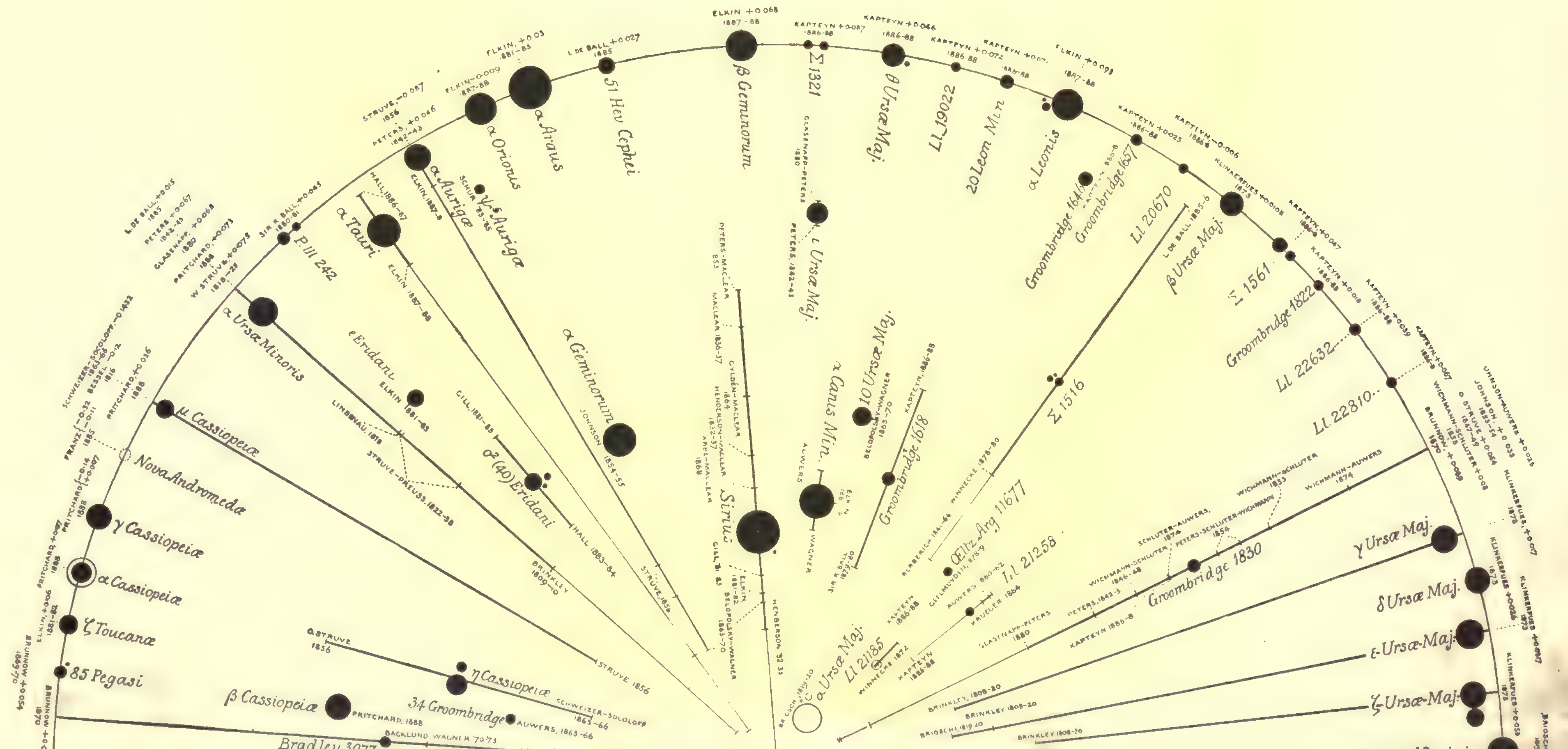
In the case of stars at greater distances, such discordances in the value of the parallax correspond to still greater differences in the distances deduced. So that, with the means at present at our disposal, we only seem to be able to measure (with a degree of accuracy which, in the ordinary affairs of life, would be thought rough) the distances of stars with a parallax greater than a third of a second. But the great distance of even the nearest stars seems to be amply proved. A parallax of three-quarters of a second of arc corresponds to a distance of about two hundred and seventy-four thousand times the Earth's distance from the sun. Assuming the parallax of α Centauri to be three-quarters of a second, its light would occupy four and a third years in traversing the vast space which separates us from it.[1]

(1399.) Some modern astronomers, as well as the earlier philosophers who adopted Plato's views with regard to the geometrical perfection and arrangement of the Universe, have imagined that the stars are arranged throughout space according to some simple uniform plan. But that there is no rigid geometrical arrangement in the stellar Universe must be evident to any thoughtful person who takes note of the different brightness of various parts of the Milky Way, as well as of the irregular distribution of the brighter stars over the heavens. There seems, however, to be some general law in the arrangement of the stars, as appears from the roughly symmetrical manner in which the heavens are divided into two nearly equal parts by the girdle of stars known as the Milky Way, and by the evident aggregation as pointed out by the elder Struve of the brighter stars towards the plane in which the Milky Way lies.

If we suppose a sphere described about the sun with a radius equal to the distance of the nearest star, we might place upon the surface of such a sphere twelve stars all at equal distances from one another and from the sun. A sphere of double the radius would have four times the surface of the first sphere, and forty-eight stars might be arranged upon it so as to be separated from one another by distances all equal to the radius of the first sphere. On a sphere of three times the radius nine times twelve stars might be similarly arranged, and so on, the number of stars increasing as the surface of the

[1] On using Newcomb and Michelson's determination of the velocity of light, 186,330 miles per second, the distance of a star expressed in light years can be found by dividing 3·262 years by the number or fraction expressing parallax of the star in seconds.

PLATE XXVI.


β Geminorum
Σ 1321
θ Ursæ Maj.
Ll 19022
20 Leon Min.
α Leonis
Groombridge 1646
Groombridge 1657
Ll 20670
β Ursæ Maj.
Σ 1561
Groombridge 1822
Ll 22632
Ll 22810
γ Ursæ Maj.
δ Ursæ Maj.
ε Ursæ Maj.
ζ Ursæ Maj.
Groombridge 1830
Ll 21258
Œltz. Arg 11677
Ll 21185
α Ursæ Maj.
Σ 1516
Groombridge 1618
10 Ursæ Maj.
α Canis Min.
ι Ursæ Maj.
Sirius
α Geminorum
σ² (40) Eridani
ε Eridani
51 Hev Cephei
α Araus
α Orionis
ψ⁵ Aurigæ
α Aurigæ
α Tauri
P. III 242
α Ursæ Minoris
μ Cassiopeiæ
Nova Andromedæ
γ Cassiopeiæ
α Cassiopeiæ
ζ Toucanæ
85 Pegasi
β Cassiopeiæ
η Cassiopeiæ
34 Groombridge
Bradley 3077

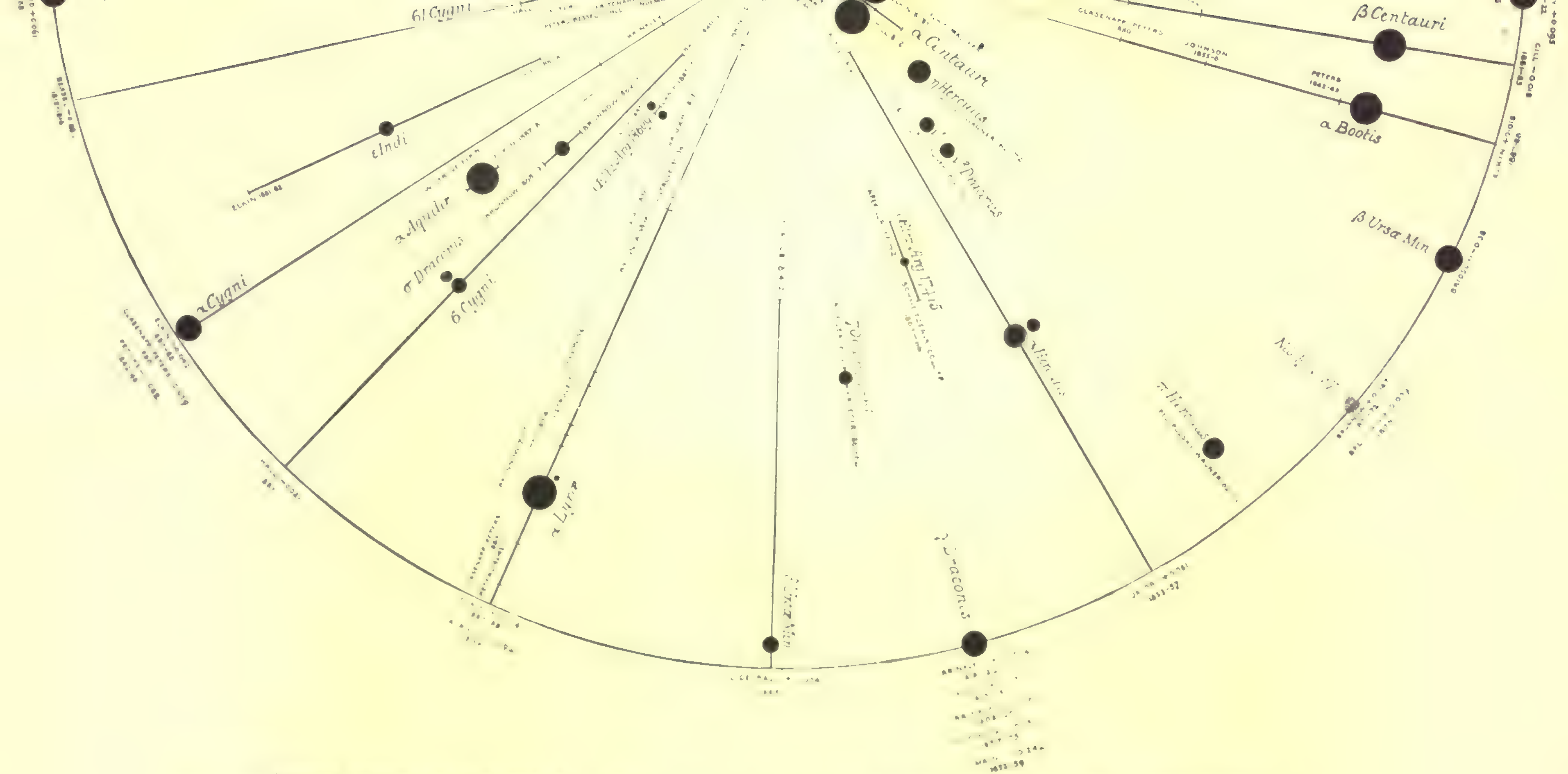


DIAGRAM SHOWING THE DISTANCES OF ALL STARS WHOSE PARALLAXES HAVE BEEN DETERMINED DURING THE PRESENT CENTURY.

When more than one parallax has been obtained a line is drawn through the star joining the various determinations, and the star disc is placed in a position intended to indicate its most probable distance having regard to the weight of the various determinations. Three or four obviously erroneous determinations have been omitted from the diagram, but they will be found in Mr. Sadler's *Catalogue of Parallaxes* referred to in the text. The outer circle corresponds to a parallax of one tenth of a second of arc. A distance of one tenth the radius of the outer circle corresponds to a parallax of a second of arc. One fifth the radius to a parallax of half a second; one half the radius to a parallax of a fifth of a second. For stars actually on the outer circle a parallax of a tenth of a second or less, and in some cases a negative parallax, has been found. The stars are arranged as nearly as possible in order of right ascension.

sphere increases. In other words, as we increase the radius the number of stars that could be placed at equal distances on any sphere would be proportional to the square of the radius of the sphere; and if all these stars were equal in brightness, and there were no loss of light in transmission from the surface of the sphere to its centre, each sphere of stars would give the same amount of light to an object at the centre. For the diameter of each star as seen from the centre of the sphere will decrease as the radius of the sphere increases, and the apparent size of the disc from which light is derived will decrease in inverse proportion to the square of the radius. In other words, the light derived from any star will decrease as the square of its distance increases, and the decrease of light would in such a uniform universe be exactly counterbalanced by the increase in the number of stars at the same distance from the centre. If we assume that the light derived from the stars on one of such spheres is equal to one thousand millionth of the light we derive from the sun, and that there is no absorption of light in space, then the light derived from the stars on two thousand million such concentric spheres would be equivalent to twice the light we receive from the sun; and, if there were no eclipsing of the light of more distant stars by nearer ones, the hemisphere of stars above the horizon at night would shine with a light equivalent to the light of the sun, a supposition which is evidently quite inconsistent with observed facts.[1]

[1] The total light received from the stars visible to the naked eye in the Northern Hemisphere is, according to Plummer (*Monthly Notices*, xxxvii. 437), equivalent to 7,349 standard stars of the 6th magnitude, while the total light of all the stars in Argelander's *Durchmusterung*, which includes stars down to the $9\frac{1}{2}$ magnitude, but probably does not contain the whole of them, is equivalent to 23,337 standard stars of the 6th magnitude. Thus we receive twice as much light from the stars between the $6\frac{1}{2}$ and the $9\frac{1}{2}$ magnitude as from all the stars visible to the naked eye down to the $6\frac{1}{2}$ magnitude. As these results are probably approximately true for any hemisphere of the heavens, it follows that (if we disregard the light of all stars too faint to be included in the *Durchmusterung*) the illuminating power of all the stars above the horizon at any one time is about $\frac{1}{80}$th part of the illumination due to the full moon. We have not at present at our disposal statistics that will enable us to determine the total illumination derived from the stars of the 10th and still smaller magnitudes. But we may assert with confidence that the total light of the stars on a dark night does not amount to one-tenth of the light derived from the full moon—that is, according to Zöllner's estimate, to $\frac{1}{6,180,000}$ of the light derived from the sun. Young estimates the total light derived from stars of all magnitudes in the northern heavens as equivalent to that of 1,500 stars of the 1st magnitude or 150,000 stars of the 6th magnitude.

It is evident that the light derived from the smaller magnitudes of stars cannot maintain the same ratio to the light derived from the magnitudes immediately above them, as in the higher parts of the scale. From the 1st to the 9th magnitude the total light from each magnitude, considered as a separate class, increases rapidly, with one exception, viz. in passing from the 3rd to the 4th magnitude; thus, according to a table derived by Plummer from Littrow's analysis of Argelander's *Survey of the Northern Heavens*, the total light of the stars of the several classes may be divided thus:

Class	[illegible]	[illegible]	[illegible]
1	1·0 to 1·9	10	646·6
2	2·0 „ 2·9	37	957·0
3	3·0 „ 3·9	130	1344·9
4	4·0 „ 4·9	312	1291·1
5	5·0 „ 5·9	1001	1656·9
6	6·0 „ 6·9	4386	2904·0
7	7·0 „ 7·9	13823	3669·9
8	8·0 „ 8·9	58095	6154·4
9	9·0 „ 9·5	237131	12069·3

The rise from the 8th to the 9th magnitude is

Olbers carried this speculation still further, and showed that, if the universe of stars is infinite, as we are naturally inclined to suppose, no line could be drawn extending from the Earth into distant regions of space which would not encounter some star. Supposing all the stars were equally bright with our sun—however minute their superficial dimensions might appear by reason of their distance—the brilliance of their discs as compared area for area would not be affected, and the whole celestial sphere would, if there were no absorption of light in space, shine as a luminous envelope equally bright with the surface of our sun. We should, in fact, only be able to discover the sun with difficulty by his spots, and the moon and planets would only be perceived by us as jet black discs upon a bright ground as brilliant as the suns.

(1400.) If we reject as abhorrent to our minds the supposition that the universe is not infinite, we are thrown back on one of two alternatives—either the ether which transmits the light of the stars to us is not perfectly elastic, or a large proportion of the light of the stars is obliterated by dark bodies. As will be seen later on, we possess evidence of the existence of some dark bodies, comparable in size with the aggregations of matter which shine as stars, and we also know of the existence of smaller dark bodies such as comets and meteors, which are evidently very numerous in the space immediately surrounding the solar system. If the light of the uniform bright background were only cut down by dark bodies comparable in size with the brilliant objects which shine as stars, we should need to assume that the number of such dark stars very greatly exceeds the number of bright stars, in order to account for the faint illumination of the heavens. An equal number of dark and bright stars intermixed in space would only serve to reduce the general illumination of the heavens, on the above suppositions, to one-half of the average brightness of the surface of the bright stars; and a million times as many dark stars as bright would, if the dark masses were all comparable in size with the bright ones, not account for the faintness of the illumination of the midnight sky as we see it. But a much smaller amount of matter distributed through space in comparatively small masses, such as flights of stones or clouds of dust, would serve entirely to obliterate the light of a distant background.

(1401.) It should be noted that a want of perfect elasticity in the ether

the most rapid of all. This may possibly be due to Argelander's having entered many stars of the 10th and 11th magnitudes just visible in his telescope as lying between the 9·0 and the 9·5 magnitudes; in the higher magnitudes the ratio must again drop off, and that very rapidly, or the total amount of the light derived from the stars would be greater than it is. The small amount of light given by the stars may either be accounted for by supposing, as men were once content to suppose, that the solar system occupies a central position in a limited universe of stars, or by assuming that the light of distant stars is absorbed or cut out by the interposition of dark bodies.

would probably reduce the light of the stars equally in all directions ; whereas dark masses might be so arranged in space as to produce a greater diminution in the light of the background in one direction than in another. We have evidence of a grouping of the bright masses which shine as stars, and

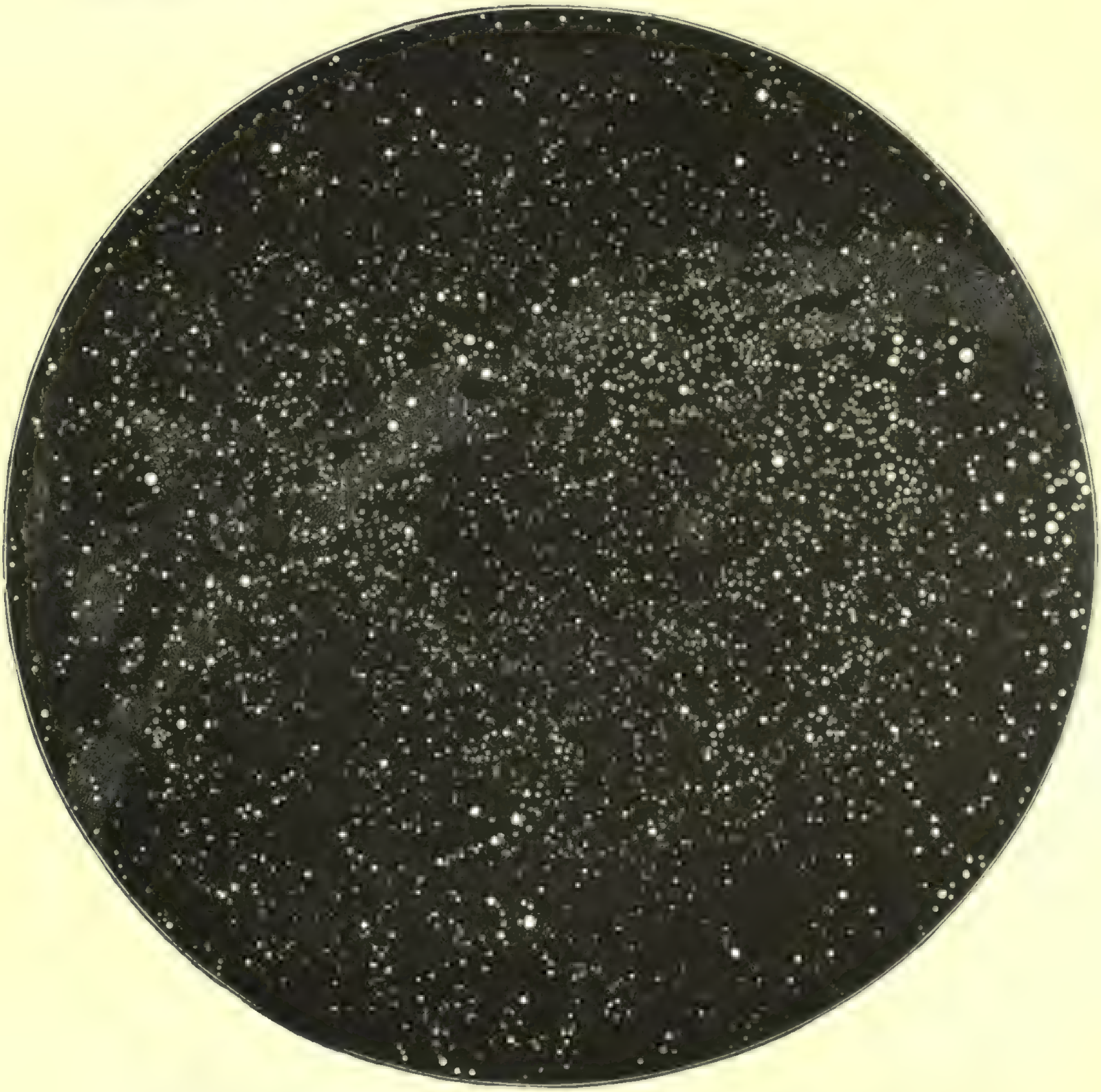


FIG. 432.—Mr. Proctor's Isographic Projection of the Stars visible to the naked eye in the Southern Hemisphere.

also of a different grouping of the fainter masses we know as nebulæ, and an arrangement in particular parts of space of non-luminous matter does not seem on general grounds improbable ; but no aggregation of dark masses in the poles of the Milky Way that can rationally be conceived of could cut down the light of a uniform background so as to produce the gradations of

nebulous light evidently connected with streams and clusters of stars which we observe in the Milky Way.

(1402.) The intimate connection that exists between the distribution of the stars visible to the naked eye and the smaller stars which apparently

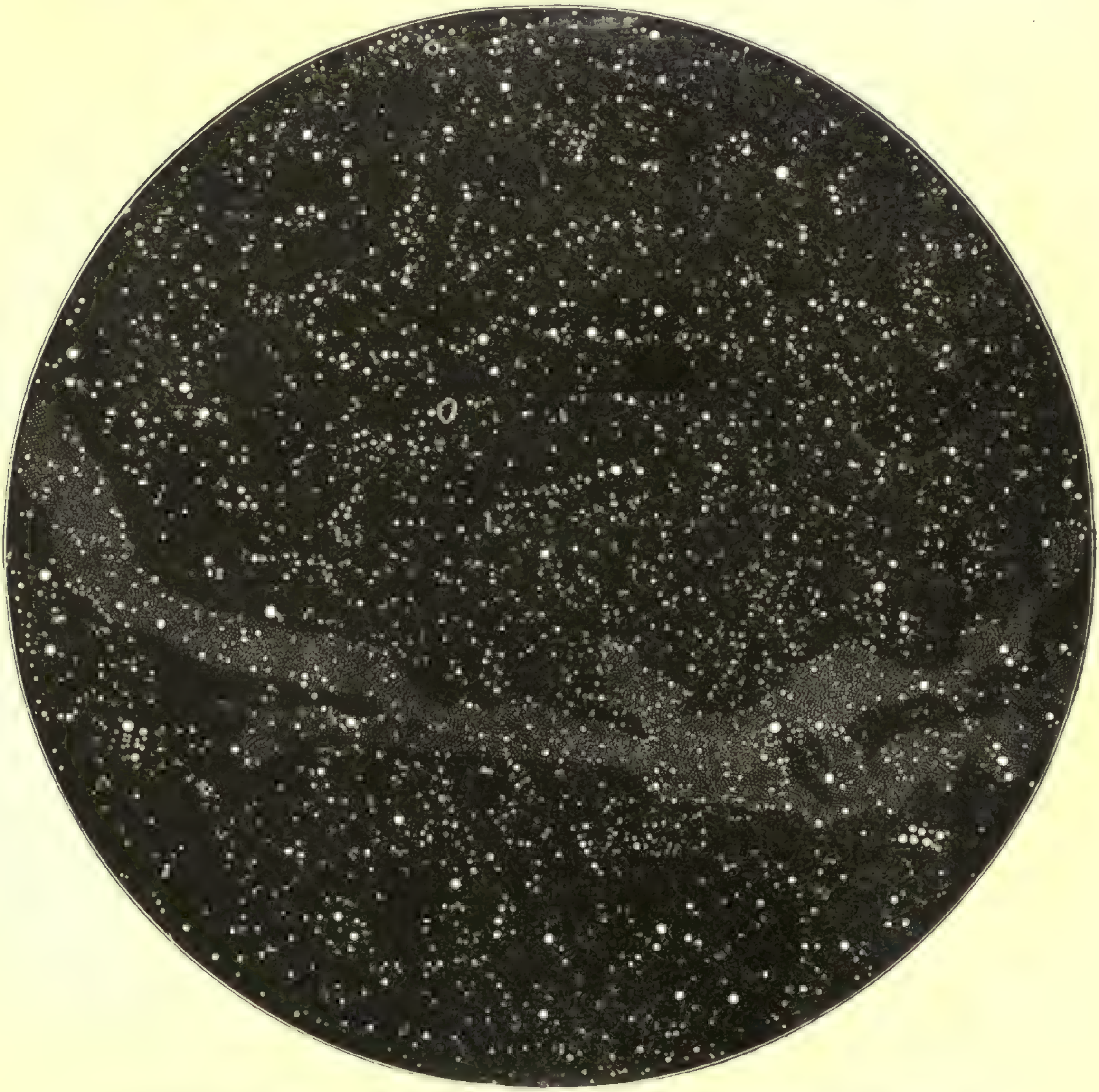


Fig. 433.—Mr. Proctor's Isographic Projection of the Stars visible to the naked eye in the Northern Hemisphere.

cause the nebulous light of the galaxy was first pointed out as regards the details of the Milky Way by the late Mr. Proctor in a series of papers published in the 'Monthly Notices' of the Astronomical Society between October 1870 and November 1871. The importance of the work, and its bearing

on our theory of the universe, can hardly be over-estimated, for it proves that many of the larger stars are intimately associated with the Milky Way, and that the stars which form the background of the galaxy are really, as well as apparently, of smaller dimensions than the brighter stars which are associated with them.

Fig. 434.—Mr. Proctor's Equal-Surface Chart of 324,000 Stars (from Argelander's series of 40 large charts).

Having laid down all the stars visible to the naked eye on two charts, one of the Northern and the other of the Southern Hemisphere, on what is known as the isographic projection because equal areas of the heavens are represented by equal surfaces on the chart, he counted the number of naked eye stars visible upon various regions of which he estimated the area by cutting up the maps and weighing the paper.[1] Figs. 432 and 433 are copies

[1] *Monthly Notices*, xxxi. p. 29.

reduced in size of Mr. Proctor's isographic charts. In the Northern Hemisphere there is a very rich region covering Cygnus, Cepheus, Ursa Minor, and Lacerta. It is numbered 2 in the accompanying table. In the Southern Hemisphere there is a corresponding rich region covering the keel of Argo. It is numbered 12 in the table. Nearly half the northern heavens (the more central portion) is exceptionally rich in stars, while even in a more marked degree one half of the southern heavens (almost centrally surrounding the Greater Magellanic Cloud) exhibits exceptional richness. These are numbered 3 and 11 in the table. The remaining portions of the two hemispheres form the parts numbered 4 and 10 in the table. Certain parts of this outer and poorer region are exceptionally poor in lucid stars. They abut on the richest parts of this rich region, and the contrast is thus rendered more striking. They are numbered 5, 6, 8, and 9 in the accompanying table.

	Name of Region	Area. Area of Hemisphere as 1	Number of Lucid Stars	Richness. Average = 5850
Northern	1 Milky Way	$\frac{1}{11}$	497	9940
	2 Richest region	$\frac{3}{22}$	622	9050
	3 Central region	$\frac{5}{11}$	1420	6248
	4 Outer region	$\frac{6}{11}$	1070	3923
	5 Poor region I.	$\frac{3}{22}$	201	2948
	6 Poor region II.	$\frac{3}{22}$	175	2567
	7 Gaps in Milky Way	$\frac{1}{62}$	20	1240
Southern	8 Poor region I.	$\frac{3}{22}$	161	2361
	9 Poor region II.	$\frac{3}{22}$	216	3198
	10 Outer region	$\frac{1}{2}$	893	3572
	11 Central region	$\frac{1}{2}$	2467	9868
	12 Richest region	$\frac{3}{22}$	895	13126
	13 Milky Way	$\frac{1}{10}$	618	13596

The numbers in the last column indicate the total number of stars which would be visible to the naked eye, if the whole heavens were covered as richly or as sparsely as the corresponding region.

This simple numerical investigation of Mr. Proctor's conclusively disposed of the theory that the stars from which we derive the milky light of the galaxy lie at vast distances beyond the stars visible to the naked eye; for the numbers we are dealing with preclude the supposition that the coincidence of distribution can be a mere result of chance.[1]

[1] Mr. Proctor remarks that it may be mathematically demonstrated that even less marked features, such as the exceptional richness of the Southern Hemisphere, cannot be ascribed to chance distribution thus:

Assuming the northern heavens to contain 2,490 lucid stars, the southern 3,360, or 5,850 in all (we need not inquire whether these numbers are exactly correct, because the laws of probability include chance errors as well as chance distribution): the probability that a star placed at random will lie on one or other hemisphere equals $\frac{1}{2}$. Hence the antecedent probability that out of 5,850 stars so distributed, as many as 3,360 will be found in either hemisphere, is represented by a fraction whose numerator is the sum of all the coefficients in the expansion of $(x+y)^{5850}$, in which either x or y is raised to a power of not less than 3,360;

In 1871 Mr. Proctor laboriously[1] laid down on a single equal surface projection chart 324,000 stars ranging down to below the 9th magnitude from Argelander's series of forty large charts, and he made it clear that there is a detailed connection which becomes more striking as we proceed downwards, so as to include stars of the $9\frac{1}{2}$ magnitude, between the distribution of the stars and the complex branching and denser portions of the Milky Way. Fig. 434 is a reproduction on a smaller scale of Mr. Proctor's original chart, which was twenty-five inches in diameter. It includes nearly all the stars between the Pole and 2° of south declination down to the 9th magnitude, and also a good many between the 9th and 10th, and would include stars down to the 11th magnitude of Herschel's scale; some of the stars in the rich regions of the Milky Way are omitted, as Argelander had not always time to note them down when they came into the field very thickly,[2] but this only adds to the strength of Mr. Proctor's case. Each space in Mr. Proctor's map was, he says, filled in, not according to the mere

and its denominator the sum of all the coefficients, or $(2)^{5850}$. The sum of the favourable coefficients is

$$2\left[1+\frac{5850}{1}+\frac{5850.5849}{1.2}+\&c.\ldots\right.$$
$$\left.+\frac{5850.5849\ldots3361}{1.\quad 2\ldots2490}\right]$$

and the sum of the unfavourable coefficients is

$$=\frac{5850.5849\ldots2926}{1.\quad 2\ldots2925}$$
$$+\ 2\left[\frac{5850.5849\ldots3360}{1.\quad 2\ldots2491}+\frac{5850.5849\ldots3359}{1.\quad 2\ldots2492}\right.$$
$$\left.+\&c.\ldots+\frac{5850.5849\ldots2927}{1.\quad 2\ldots2924}\right].$$

The total number of favourable terms is 4982, that of the unfavourable 869, or more than one-sixth of the former. Hence by comparing the largest unfavourable term with six times the largest favourable term, we shall get a ratio obviously less than that we require. (It needs only a consideration of the law according to which the coefficients increase to see this.) Hence the odds against the occurrence of the observed arrangement of the stars, as respects the Northern and Southern Hemispheres, would be, if chance-distribution were alone in question,

$$>\frac{1}{6}\left\{\frac{\dfrac{5850.5849\ldots2926}{1.\quad 2\ldots2925}}{\dfrac{5850.5849\ldots3361}{1.\quad 2\ldots2490}}\right\}$$

$$>\frac{1}{6}\left\{\frac{3360.3359\ldots2926}{2491.2492\ldots2925}\right\}$$

and obviously therefore (*à fortiori*)

$$>\frac{1}{6}\left\{\frac{2926.3013.3100.3187.3274}{2925.2838.2751.2664.2577}\right\}^{87}$$

Now the value of this expression is easily calculated to be

66,996,090,000,000,000,000,000.

Hence the antecedent probability of the observed event is less than

$$\frac{1}{66,996,090,000,000,000,000,000}.$$

The denominator would have had 132 figures had we dealt with the probability of the existence of two such regions as those numbered 3 and 11 in the above table.

As respects this last relation it may readily be shown that if a sphere having a radius exceeding many million times the distance of the furthest object revealed by the Rosse Telescope, were filled with atoms severally minuter many million-fold than the minutest object which the microscope will reveal, the chance that a specified atom would be selected at random from that inconceivably vast universe of atoms would be many million times larger than the antecedent probability of the observed relation, supposing chance-distribution alone in question. Yet that relation is by no means the most remarkable exhibited in the above Table.

[1] *Monthly Notices*, vols. xxxi. p. 175 and xxxii. p. 1.

[2] Argelander's great catalogue was made with a small telescope which remained fixed in position while each zone was observed; the Right Ascension being determined by the time of transit, and the Declination by the position of the telescope and the place of transit in the field.

number of the stars, but very carefully by an eye-draught from the corresponding space in Argelander's charts ; and it amply shows the way in which these larger stars are clustered together on the denser parts of the Milky Way, as well as the striking manner in which the gaps or vacuities in the Milky Way are avoided.

(1403.) Mr. J. E. Gore has carried Mr. Proctor's investigation further by counting the number of stars of the 1st, 2nd, 3rd, and 4th magnitudes, which lie upon the Milky Way,[1] and he has shown that the tendency to aggregation may even be recognised in the distribution of stars of the 1st magnitude. Thus, of 32 stars brighter than the 2·0 magnitude, 12 lie upon the Milky Way, viz.: Vega, Capella, Altair, α Orionis, Procyon, α Cygni, α Persei, Sirius (near boundary), α and β Centauri, and α and β Crucis. As the area of the Milky Way does not exceed one-seventh of the whole sphere (Proctor assumed one tenth), the percentage of stars of any magnitude which should lie on the Milky Way, if they were distributed at random over the sphere, should be only 14·3 instead of 37·5, as is the case with stars greater than the 2nd magnitude. According to Mr. Gore, 33 stars brighter than the 3·0 magnitude lie upon the Milky Way out of a total of 99 or 33·3 per cent. Of those between the 3rd and 4th magnitude, 73 stars lie on the Milky Way out of a total of 262, or 28·2 per cent. Considering the stars north and south of the equator, there are, according to Mr. Gore, 164 stars above the 4·0 magnitude in the Northern Hemisphere, of which 52 are on the Milky Way, or 31·7 per cent., while in the Southern Hemisphere there are 228 stars, of which 66 are on the Milky Way, or a proportion corresponding to 28·9 per cent. A result which seems to have some connection with the fact that the Southern Hemisphere is richer in bright stars than the Northern in the proportion of about 11 to 8.

Mr. Gore has also counted all the stars in Heis's Atlas which lie on the Milky Way, and he finds the number to be 1,186 out of a total of 5,356 objects given by Heis (excluding variable stars, clusters, and nebulæ) in his catalogue, so that, according to Heis, out of all the stars visible to the naked eye in the Northern Hemisphere, there is a proportion of 22·1 per cent. stars on the Milky Way, or more than $1\frac{1}{2}$ time that due to its area.

The rapid way in which the percentage of stars on the Milky Way at first falls with the magnitudes and then rises again is very instructive ; it seems to indicate that while the Milky Way is chiefly made up of small stars, there are some giants amongst them which probably greatly exceed in magnitude the larger stars in the cluster to which we belong.

Major Markwick has made a similar investigation with regard to the

[1] Gore's *Scenery of the Heavens*, p. 264.

stars in the Southern Hemisphere, by counting the stars of the various magnitudes which fall on the Milky Way as shown in Gould's 'Uranometria Argentina;' he finds that out of 228 stars brighter than the 4th magnitude, there are 121 on the Milky Way, or a percentage of 53. And for the total number shown down to the 7th magnitude inclusive there are 3,072 on the Milky Way out of 6,694, or a proportion of 45·8 per cent. From a careful calculation of the area of the Milky Way as shown in these charts, Major Markwick finds that it covers, as shown by Gould, an area equal to one-third of the whole hemisphere, or 33·3 per cent.

(1404.) In pursuance of the general plan of the Old and New Astronomy, we will rapidly pass in review the theories of early astronomers with regard to the constraction of the stellar universe, and show the steps by which the theory at present received was arrived at.

Copernicus, in his treatise 'De Revolutionibus,' says that the universe is spherical; not only because that is the most perfect of all figures, needing no fastening or junction, complete in itself, but because it is the most capacious figure, fittest to enclose and preserve all things; because the most perfect portions of the universe—the sun, moon, and stars—are seen to have that shape; and, lastly, because, as we see in drops of water and other liquids, all things capable of assuming the figure they prefer, select the figure of a sphere.

Tycho Brahé and Kepler both seem to have supposed the Milky Way was formed of some nebulous substance; this is evident from their speculations with regard to the origin of the new stars of 1572 and 1604, which they respectively observed and described.

The invention of the telescope—or rather its application to the examination of the heavens in 1609—showed Galileo that this nebulous stream was thickly studded with stars, and he congratulates himself in the 'Sidereus Nuncius' with having put an end to the ancient controversy as to its nature. He says: 'It is truly a wonderful fact that to the vast number of fixed stars which the eye perceives, an innumerable multitude, before unseen, and exceeding more than tenfold those hitherto known, have been rendered discernible. Nor can it be regarded as a matter of small moment that all disputes respecting the nature of the Milky Way have been brought to a close, and the nature of the zone made manifest not to the intellect only, but to the senses'

Christian Huyghens, in his 'Cosmotheoros,' a book that was passing through the press when he died,[1] held that the sun is one of the stars, and

[1] Huyghens died at the Hague, June 8, 1695. Its final publication seems to have been delayed for three years. This work became very popular. It was published at the Hague in 1698 and an

resembles them in size and structure. The distribution of the stars he regarded as in a general sense uniform, for he held that the same distance which separates the sun from the nearest star separates that star from the next beyond, and that from the next, and so on to infinity.

(1405.) Thomas Wright, of Durham,[1] made the next step towards a complete theory of the universe. The position of the Milky Way was not discussed by Huyghens, and the nebulæ were not referred to. Wright's theory took in the whole of the sidereal universe as known in his day, and by a bold effort of genius he anticipated some of the speculations of Sir William Herschel. Wright examined the structure of the Milky Way with a 'one-foot reflecting telescope'—which, we presume, means a telescope only one foot in length—

English translation entitled *The Celestial Worlds discovered, or conjectures concerning the inhabitants, plants, and productions of the worlds in the planets*, was published the same year in London; there was a second English edition, besides several translations into Continental languages. The book was dedicated to his brother Constantine Huyghens, who was secretary to William of Orange, and came with him to England. At p. 145 he says: 'The stars might still be suns, which is the more probable, because their light is certainly their own; for it is impossible that ever the sun should send, or they reflect it, at such a vast distance. This is the opinion that commonly goes along with Copernicus's System. And the patrons of it do also with reason suppose that these stars are not in the same sphere, as well because there's no argument for it, as that the sun, which is one of them, cannot be brought to this rule. But it's more likely they are scatter'd and dispersed all over the immense spaces of Heaven, and are as far distant from one another, as the nearest of them are from the sun.' It is perhaps not very generally known that the Plumian Professorship of Astronomy at Cambridge was founded by Dr. Plumer as an expression of the pleasure he had derived from the perusal of Huyghens's *Cosmotheoros*. The work was recommended to him by Flamsteed, the first Astronomer Royal.

[1] Thomas Wright is usually spoken of as 'of Durham,' and I have followed the usual custom in giving him this place patronymic; but he seems to have spent the greater part of his life in London. He was 'mathematical instrument maker to the King,' and kept a shop in Fleet Street. De Morgan, in his delightful *Budget of Paradoxes*, says that the celebrated business of Troughton and Simms, also in Fleet Street, is lineally descended from that of Wright, and he quotes Mr. Simms as informing him that the line of descent runs thus: Wright, Cole, John Troughton, Edward Troughton, Troughton and Simms. In another place De Morgan says that Thomas Wright was not made a Fellow of the Royal Society 'because he kept a shop.' A very curious list might be made of the remarkable men who have not been made, or who have refused to be made, Fellows of the Royal Society. De Morgan himself was one of the latter, but I can hardly accept his suggestion as to the reason for not electing Wright; for John Dollond, who was a Spitalfields working weaver, was about this period not only elected a Fellow of the Royal Society, but was given the Copley Medal; while Moor Hall, the twin discoverer of the achromatic telescope, who was a man of means, a Bencher of the Inner Temple, and a county magistrate, was not elected. Thomas Wright was born at Byer's Green, about six miles from the city of Durham, on September 22, 1711. He was the son of a carpenter who was also a small landholder. He was apprenticed to a clockmaker, and then went to sea, and he afterwards struggled for some years as a maker of almanacs, a lecturer, and a teacher of mathematics. At last he seems to have 'risen into note as a teacher of the sciences in noble families.' He evidently enjoyed affluence towards the end of life, for he built himself a handsome house at Byer's Green in 1756–62 and died there February 25, 1786. Wright wrote on navigation, and was at one time offered the professorship of navigation in the Imperial Academy of St. Petersburg. He was a man of artistic tastes and an engraver. De Morgan suggests that the mezzotint plates in his book were by his own hands, notwithstanding the statement on the title-page that the plates are 'by the best masters.' He seems to have been consulted on matters of taste at Durham, for in the Chapter library of Durham there is a design by him for some alterations in the Cathedral, including an ornamental battlement with finials upon the western towers; which design was carried into execution, as is to be seen.

and satisfied himself that the Galaxy really consists of a multitude of minute stars. He does not seem to have known of Galileo's previous discovery, or at all events he does not refer to it. In his book published in 1750, entitled 'An Original Theory, or New Hypothesis of the Universe founded upon the Laws of Nature,' he remarked that, if we judge of the Milky Way only by phenomena, we must regard it as a zone of stars surrounding the heavens ; but this conception of the Milky Way as a perfect ring is not in agreement with the irregular distribution of the stars which are scattered over the rest of the sky, and which seem dispersed promiscuously throughout the space surrounding us on all sides. It seems inconsistent with the harmony observed in 'all the other arrangements of nature' that one scheme of stars should be arranged with perfect symmetry while another is scattered so irregularly.

He considered, however, that this might be explained by the eccentric position of the sun amongst the stars, and cited as a parallel instance the apparent irregularity of the motion of the planets as viewed from the earth, while as viewed from the sun all would appear to move with perfect regularity. He therefore argued that there may be some place in the universe where the arrangement and motions of the stars may appear perfectly uniform and harmonious. If, he said, we suppose the sun to be plunged in a vast stratum of stars of inconsiderable thickness compared with its dimensions in other respects, it is not difficult to see that the actual appearance of the heavens may be reconciled with a harmonious arrangement of the constituent bodies of such a system with respect to some common centre, provided it be admitted at the same time that the stars have all a proper motion. In such a system it is manifest that the distribution of the stars would appear more irregular the farther the place of the spectator was removed from the centre of the stratum towards either of the sides. It is also evident that the stars would appear to be distributed in least abundance in the opposite directions to the thickness of the stratum, the visual line being shortest in these directions ; and that the number of visible stars would increase as the stratum was viewed through a greater depth, until at length, from the continual crowding of the stars behind each other, it would ultimately assume the appearance of a zone of light.

Wright further held that the Milky Way is only one of many systems of stars ; though the other systems, forming with our Galaxy a system of star systems, may not resemble the Galaxy or each other in structure. There may be differences as striking as those which exist between the rings of Saturn and the belts of Jupiter. Some systems of stars may move in perfect spheres, at different inclinations and in different directions ; others again may

revolve like the primary planets in a general level; or more probably in the manner of Saturn's ring.[1]

'That this in all probability may be the case,' he says, 'is in some degree made evident by the many cloudy spots just perceivable by us . . . in which, although visibly luminous spaces, no one star or particular constituent body can possibly be distinguished. These, in all likelihood, may be external creations, bordering upon the known one, too remote for even our telescopes to reach.' Wright imagined that all the stars of the Milky Way travel round some common centre, probably an orb larger and more massive than the rest.[2]

(1406.) Five years later Kant published his remarkable treatise on 'The Natural History and Theory of the Heavens.' His ideas respecting the universe of stars were admittedly suggested by Wright's 'Theory of the Universe,' which he had read in a Hamburg journal of the year 1751, though he is unable to indicate, he says, 'to what extent his system is a reproduction or amplification of Wright's.' As a matter of fact, Kant's ideas resemble very closely those of Wright; indeed, it may be said that Kant's theory only differs from Wright's when he passes beyond the limits of observed facts. All Wright's theory, excepting his opinion respecting a central sun, was based upon observation. Kant's added conception of an infinite progression of systems, or rather order of systems,[3] though imposing, cannot be regarded as involving any real addition to our knowledge.

(1407.) Lambert's theory, published in 1761, five years after Kant's, differed from his, or rather from Wright's, chiefly in the suggestion that the

[1] Another of Thomas Wright's remarkably correct conjectures has reference to Saturn's rings. He says, 'I cannot help being of opinion, that could we view Saturn through a telescope capable of it, we should find the rings no other than an infinite number of lesser planets, inferior to those we call his satellities.'

[2] De Morgan has given a good account of Thomas Wright in the *Philosophical Magazine* for April 1848. See also the *Gentleman's Magazine* for 1793, vol. lxiii. pp. 9, 126, 213.

[3] Kant stated his theory in six theses; the first five of which are included in Wright's theory. They may be shortly stated thus: 1. that the stars are suns; 2. that the action of gravity extends beyond the solar system, so as to include the whole of the Galaxy; 3. that as in the solar system there is a general level near to which the planets travel, so there is a plane of condensation in the heavens, and that the Milky Way is the zodiac of the stellar system, 4. As the planets travel round the sun, so the stars travel round a central orb 10,000 larger than our sun. Kant thought that Sirius was this central orb, because it is opposite to the part of the Milky Way which, as he mistakenly imagined, seems widest in the constellations of the Eagle, the Fox, and the Swan. Hence the sun must lie towards these constellation—and the central orb must lie in the opposite hemisphere. But the Milky Way is, in fact, widest in Scorpio and the neighbouring constellations, a fact unknown to Kant. Struve remarks that this argument is only sound if the Milky Way is regarded as a ring, a theory which Kant does not seem to have entertained. 5. The nebulæ which the telescope does not resolve into stars, are systems resembling the Milky Way. Kant's sixth thesis was that the same sort of relation probably exists amongst the different Milky Ways which is recognised among the different suns of the Milky Way. These other Milky Ways are members of a new system of a yet higher order. We trace here the first terms of a series of worlds and systems, and these first terms of an infinite series enable us to infer the nature of the rest of the series.

Milky Way is a system of star systems, not globular but flat, forming in fact a disc, whose diameter vastly exceeds its thickness. The fact that this system of the third order consists of different systems of the second order is shown, said Lambert, by the irregularity of the Milky Way, by the different richness of its various parts, and by its branching figure. The following part of Lambert's reasoning should be noticed. He remarks that, 'as in the solar system, we observe that the distances between the several planets exceed, incomparably, the dimensions of each planet or scheme of planet and satellites, so the distance between sun and sun exceeds enormously the

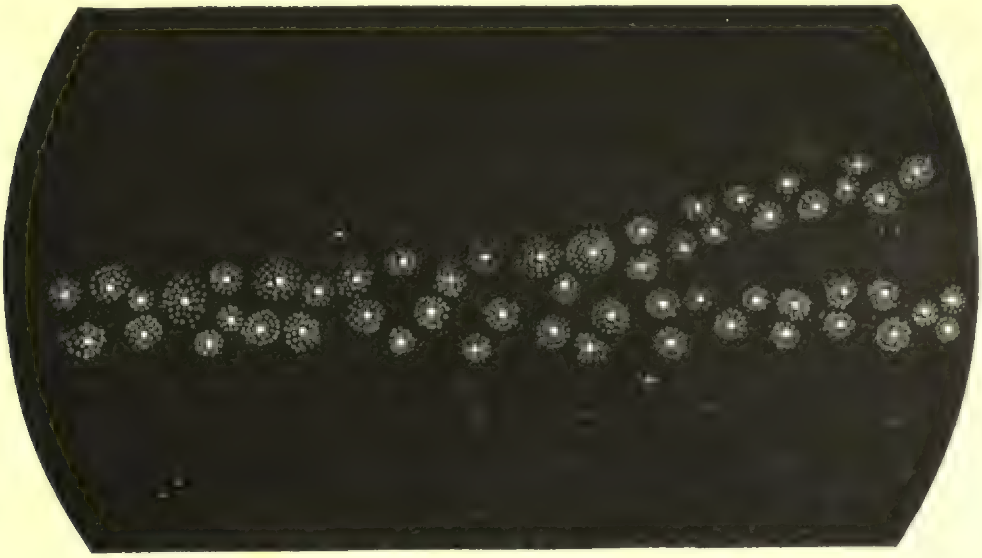


FIG. 435.—Illustrating Lambert's Theory of our Stellar System.

dimensions of the several solar systems. A similar relation probably prevails amongst higher orders—in other words, the dimensions of any system are always much less than the distances separating it from its nearest fellow-system[1] in the system of systems of which it is a member.'

Lambert's theory of the Milky Way is illustrated in fig. 435, only the clusters should be more numerous and the distances between them much greater compared with their several dimensions.

The next important step was made by the Rev. John Michell in a paper published in the 'Phil. Trans.' for 1767; he did not enunciate a complete

[1] Lambert regarded the sun as belonging to a cluster containing above a million and a half of stars, forming a spherical cluster whose diameter exceeds 150 times the distance of Sirius, but this diameter he supposed to be far less than the distance of the nearest cluster of suns. He imagined that each cluster must be ruled by a central sun, otherwise he considered that the motions within the cluster would not have the requisite stability. Though the mass of such a central sun must be very great, he supposed that its luminosity might be faint, or it might even be an opaque body, illuminated by the suns which travel nearest to it.

theory respecting the galaxy, but his inquiries led him into speculations respecting the arrangement of the larger visible stars into groups or clusters. He satisfied himself, by an application of the doctrine of probabilities, that there must be some physical connection between the numerous double and triple stars that had been discovered by Herschel ; and he concluded that they must be under the influence of mutual gravity ; he also applied the doctrine of probabilities to the larger groups, such as the Pleiades and the cluster in the sword handle of Perseus. 'We may conclude,' he says, 'with the highest probability (the odds against the contrary opinion being millions to one) that the stars are really collected together in clusters in some places where they form a kind of system, whilst in others there are either few or none of them.' He then proceeds to inquire whether the sun is a member of such a system, and to speculate on the appearance of the solar group as seen from the Pleiades. He considers two hypotheses respecting the extent of our cluster. According to one of them, the cluster contains about 1,000 suns, and according to the other about 350. Adopting one set of considerations, he infers that our cluster would subtend about two degrees as seen from the Pleiades, with no star bright enough to be seen by the naked eye and only about ten in a two-inch telescope ; while, if the other be adopted, our cluster would subtend about twelve degrees as seen from the Pleiades, some ten stars would be visible to the naked eye, and all stars down to the 4th magnitude belonging to the cluster would be seen in a telescope of two inches aperture.[1] Michell's ideas differed from those of Lambert in that he did not regard the different systems of stars composing the sidereal universe as either regular in shape or similar to each other in figure or constitution. He judged rather from observed facts and attentive reasoning thereon than from preconceived opinions as to the uniformity of creation.

In 1783 Sir William Herschel, having constructed a telescope of $18\frac{7}{10}$ inches aperture, and 20 feet focal length, commenced a systematic survey of the heavens with the intention of investigating the structure of the stellar

[1] Michell fully realised the great differences of brightness that evidently exist amongst the stars : he regarded the nebulæ as clusters of stars seen from a great distance. Whatever the real magnitude and distance of the stars of our system, he says, p. 260 : 'If they were to be seen from a distance at which the whole system would not subtend an angle of more than six or eight minutes, it would appear only as a nebula, no single star being visible with perhaps any telescope that has ever yet been made ; for at this distance the distance between the Earth and the biggest star of this system not subtending an angle of more than about three minutes (that is, about a twelve-hundredth part of the radius), the stars of this system must appear less luminous than they do to ourselves, in the proportion of the square of 1,200 (or 1,440,000) to 1. And supposing the light of Sirius to exceed that of the least visible star in the proportion of 1,200 to 1, the brightest star therefore would still require to have its light increased in the proportion of 1,200 to 1 before it could begin to be distinguishable ; to do this would require a telescope that would take in a pencil of rays of a larger diameter than the pupil of the eye in the proportion of 35 to 1, that is, a pencil of about a foot diameter, exclusive of deductions.

universe. His telescope was mounted so as to confine it to the meridian, and by noting the time when the stars passed the meridian and the position of the telescope during the observation, the places of stars in the heavens could be determined in a rough way that would enable them to be identified again. In 1784 he presented to the Royal Society the preliminary results of his investigations. His method of surveying the heavens, which he termed *star-gaging*, was to count the objects in ten consecutive fields of view, add the numbers together and divide by ten, so as to give the mean of the numbers which he called the *star-gage* for that region of the heavens. He was much struck by the fact that the nebulæ as well as the stars which passed before his view appeared to be arranged in long groups which he termed strata.

'It is very probable,' he wrote, 'that the great stratum, called the Milky Way, is that in which the sun is placed, though perhaps not in the very centre of its thickness. We gather this from the appearance of the Galaxy, which seems to encompass the whole heavens, as it certainly must do if the sun is within the same. For suppose a number of stars arranged between two parallel planes, indefinitely extended every way, but at a given considerable distance from each other, and calling this a sidereal stratum, an eye placed somewhere within it will see all the stars in the direction of the planes of the stratum, projected into a great circle, which will appear lucid on account of the accumulation of the stars; while the rest of the heavens, at the sides, will only seem to be scattered over with constellations more or less crowded, according to the distance of the plane or number of stars contained in the thickness or sides of the stratum. Thus, in the figure an eye at S, within the stratum *a b*, will see the stars in the direction of its length *a b* or height *c d*, with all those in the intermediate situations projected into

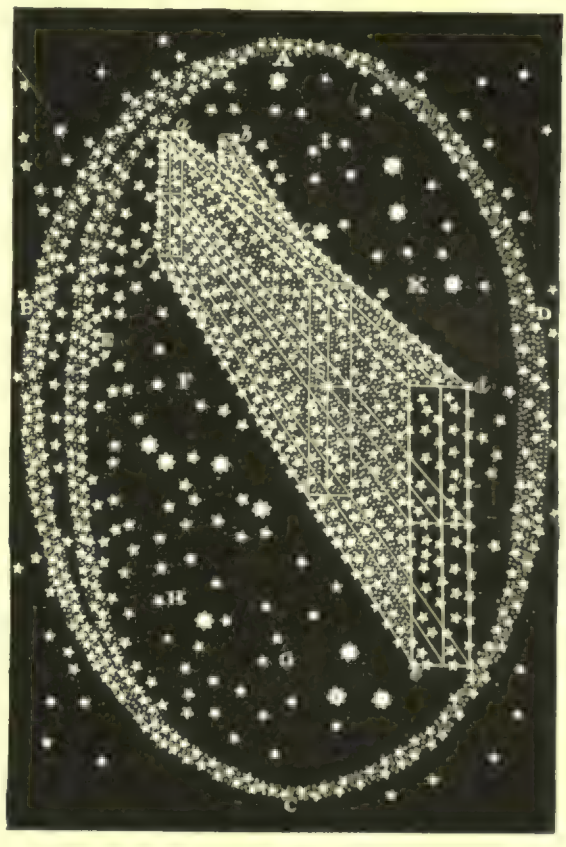


FIG. 436.—Copied from the plate illustrating Sir W. Herschel's paper in the 'Phil. Trans.' for 1784.

the lucid circle ABCD, while those in the sides will be seen scattered over the remaining part of the heavens. If the eye were placed somewhere without the stratum, at no very great distance, the appearance of the stars within it would assume the form of one of the less circles of the sphere, which would be more or less contracted according to the distance of the eye, and if this distance were exceedingly increased, the whole stratum ought at last to be drawn together into a lucid spot of any shape according to the position, length, and height of the stratum.

'Let us now suppose that a branch, or smaller stratum, should run out from the former in a certain direction, and let it also be contained between two parallel planes extended indefinitely onwards, but so that the eye may be placed in the great stratum somewhere before the separation, and not far from the place where the strata are still united. Then will this second stratum not be projected into a bright circle like the former, but will be seen as a lucid branch proceeding from the first, and returning to it again at a certain distance less than a semi-circle.'

Herschel evidently regarded his drawing as only a diagram which would serve to illustrate his theory (or rather Wright's theory), that the great circle of the Milky Way corresponded to a deep stratum of stars, for he says, p. 444, 'What has been instanced in parallel planes may easily be applied to strata irregularly bounded and running in various directions,' and he was fully aware that there are aggregations or clusters of stars in certain directions. But this diagram has been made use of, as if it represented Sir William Herschel's matured views with regard to the structure of the Stellar Universe, and it has caused his theory to be mistakenly described as the 'flat grindstone theory,' and the 'split grindstone theory' of the Universe. Herschel, however, held the erroneous idea that the position of the sun within the cluster of stars to which it belongs, might be determined by a systematic counting of the number of stars seen in projection in different directions and with this end in view, he commenced his series of star-gages. Herschel's method of determining the form of the star cluster to which we belong involves the assumption that the stars within the cluster are uniformly distributed, or their number within any field of view could not be used to determine the distance from the point of observation to the exterior of the cluster.

Herschel refers in this paper to the fact that the distribution of stars in space is not uniform, and he was aware that there was a connection between their arrangement and the distribution of nebulæ over the surface of the heavens. He says, 'I soon found that I generally detected nebulæ in certain directions rather than in others, and that the spaces preceding them were generally quite deprived of their stars so as often to afford many fields with-

out a single star in it; the nebulæ generally appeared some time after amongst stars of a certain considerable size, and but seldom amongst very small stars. When I came to one nebula, I generally found several more in the neighbourhood, and afterwards a considerable time passed before I came to another parcel, and these events being often repeated in different altitudes of my instrument, and some of them at a considerable distance from each other, it occurred to me, that the intermediate spaces between the sweeps might also contain nebulæ, and, finding this to hold good more than once, I ventured to give notice to my assistant at the clock "to prepare, since I expected in a few minutes to come on a stratum of the nebulæ, finding myself already (as I then figuratively expressed it) on nebulous ground." In this I succeeded immediately, so that I now can venture to point out several not far distant places where I shall soon carry my telescope in expectation of meeting with many nebulæ.'

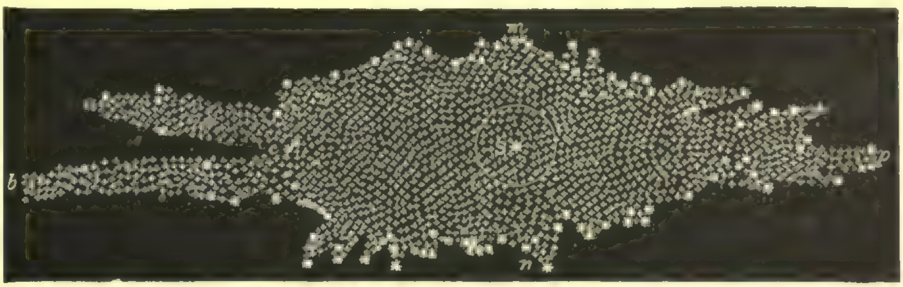


FIG. 437. Section of the Star Cluster to which the Sun belongs, as deduced from Sir W. Herschel's Star-gauges,[1] copied from the plate illustrating Sir W. Herschel's paper in the 'Phil. Trans.' for 1785.

It is evident, then, that even at the date of Herschel's first paper in 1784 he was aware that the stars were not distributed uniformly throughout space, but he adopted the assumption probably because he imagined that, in dealing with large areas, it was approximately true, and it afforded a very convenient basis for calculation.

In 1785 he presented a second paper to the Royal Society on the construction of the heavens, in which he gave a table of star-gauges (altogether about 700, corresponding to more than 3,000 observations), and a further table in which he had calculated the length of the visual ray, or the distance from the eye to the exterior of the cluster, corresponding to the various numbers of stars seen in the field of view at the same time. He says, 'By taking out of this table the "visual rays" which answer to the gauges, and applying lines proportional to them around a point, according to their respective right ascensions and North polar distances, we may delineate a solid by

[1] The woodcut is reduced to about two-fifths of the scale of the diagram accompanying Sir Wm. Herschel's paper in the *Phil. Trans.* of 1785.

means of the ends of these lines, which will give us so many points in its surface. I shall, however, content myself at present with a section only. I have taken one which passes through the poles of our system, and is at right angles to the conjunction of the branches which I have called its length. The name of poles seemed to me not improperly applied to those points which are 90° distant from a circle passing along the Milky Way, and the North Pole is here assumed to be situated in R.A. 186° and P.D. 58°. The section represented is one which makes an angle of 35° with our equator, crossing it in 124½° and 304½°. . . . From this figure, which I hope is not a very inaccurate one, we may see that our nebula is of the third form—that is, *a very extensive branching compound congeries of many millions of stars* which most probably owes its origin to many remarkably large as well as closely scattered small stars that may have drawn together the rest. Now, to have some idea of the wonderful extent of this system, I must observe that this section of it is drawn upon a scale where the distance of Sirius is no more than the $\frac{1}{80}$th part of an inch' [as shown upon our reproduction of Sir W. Herschel's diagram the $\frac{1}{200}$th part of an inch], 'so that probably all the stars which on the finest night we are able to distinguish with the naked eye, may be comprehended within a sphere drawn round the large star near the middle, representing our situation in the nebula of less than half a quarter of an inch radius' [that is the $\frac{1}{20}$th of an inch, as shown in our reproduction of the diagram].

Michell's application of the doctrine of probability, with which Sir William Herschel must have been familiar, had established the fact that the grouping of stars corresponds to a physical reality, and cannot be accounted for as merely an optical illusion. Herschel's language, even at the date of these early papers, clearly shows that he conceived of the existence of streams as well as strata of stars, and that he fully appreciated the fact that there are actual clusters of stars as well as vacant spaces in the heavens, and that he recognised that there is a connection between the distribution of the stars and the distribution of nebulæ. From his paper on the construction of the Heavens, published in the 'Phil. Trans.' for 1811, it is clear that he did not regard the great majority of nebulæ as distant clusters of stars, but as diffused luminous matter, occupying large regions of the heavens, and he was aware that the general distribution of nebulæ over the heavens differed from, though it was evidently intimately connected with, the general distribution of stars. As early as 1802 Sir W. Herschel's language clearly shows that he considered the Milky Way to be a roughly circular stream of stars or region in which the stars are more closely grouped than in the space immediately around the sun. He says: 'The stars we consider as

insulated are also surrounded by a magnificent collection of innumerable stars, called the Milky Way, which must occasion a very powerful balance of opposite attractions, to hold the intermediate stars in a state of rest. For, though our sun and all the stars we see may truly be said to be in the plane of the Milky Way, yet I am convinced by a long inspection and continued examination of it, that the Milky Way itself consists of stars very differently scattered from those which are immediately about us.' Sir William Herschel never seems to have abandoned the idea that our place in the stellar universe may be determined by the method of star-gauging described in his earlier papers, or it would not have been reproduced with filial respect by his son, Sir John Herschel, who extended this method to the Southern Hemisphere.

(1408.) Piazzi, and subsequently Wilhelm Struve, had noted the evident condensation of the brighter naked eye stars towards the region of the Milky Way. W. Struve, in his 'Études d'Astronomie Stellaire,' published in 1847, reviewed the previous speculations with regard to the structure of the Milky Way, and re-discussed them with the help of Bessel's and some of Argelander's catalogues; arriving at a conclusion practically identical with that of Sir William Herschel, although he perceived that if the principle of star-gauging was sound, the stars of the brighter orders should be strewn with general uniformity over the heavens. His method, too, was unsatisfactory, as he only took into account a zone of stars 30° wide, extending 15° North and 15° South of the equator. The zone of stars examined was not at right angles to the zone of the Milky Way, but only makes a comparatively acute angle with it. The stars in his equatorial zone include only the first nine magnitudes, and they were numbered in hours of Right ascension. He found that the hours corresponding to the Milky Way—the seventh and the nineteenth—were the richest. In approaching the Milky Way there was a gradual increase up to the maximum of richness, and then a gradual decrease down to the minimum, whence he drew the general conclusion that the number of stars increases as we approach the Milky Way from any point outside it, and that the sun occupies nearly the centre of a cluster of a disc-like shape, not perfectly regular in form towards its circumference, so that the line of sight passing through the greatest number of stars does not accurately describe a great circle in passing round the heavens, but follows a course which 'presents some inflections.' Wilhelm Struve also thought that he had grounds for assuming that the sun must be situated a little to the north of the central plane of the cluster, in a direction towards the constellation *Virgo*, at a distance equivalent to about the average distance of a star of the 2nd magnitude from the centre of the cluster.

(1409.) In 1852 Prof. Stephen Alexander of Princeton, New Jersey, wrote a series of papers in Gould's 'Astronomical Journal,' 'On the origin of the forms and the present condition of some of the clusters of stars and several of the nebulæ.' He discusses the possible origin of the forms of the nebulæ as then known, and compares the Milky Way to one of the spiral nebulæ. An observer near to the centre of one of these spiral nebulæ would, he says, see a stream of nebulous light surrounding the heavens, somewhat similar to the Milky Way, possibly with branches and loops if the streams of the spiral were not in a plane, thus suggesting that a stream of stars would satisfy the appearances observed by us as well as a stratum of stars turned edgeways towards the observer. But the arguments used were not sufficiently detailed and convincing to influence the generally received ideas as to the construction of the stellar universe.

(1410.) The late Mr. Proctor, in a series of papers commencing with one in the 'Intellectual Observer' for August 1867, showed the intimate con-

Fig. 438.—A perspective view of the Milky Way supposed to be depicted on a crystal globe.

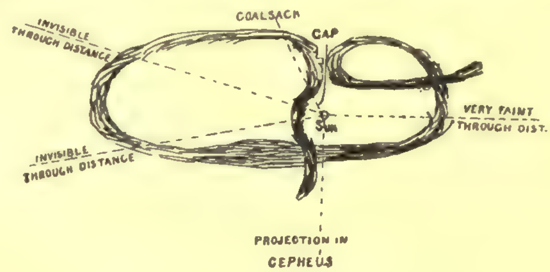


Fig. 439.—Suggested general figure of the Milky Way spiral in space.

nection that exists between the distribution of the larger stars and the gaps and branches of the Milky Way (see Art. 1402), and by a simple application of the theory of probabilities he showed that it is not reasonable to suppose that the curving streamers of milky light we see in the sky correspond to strata or sheets of stars viewed edgeways. If, he said, we estimate the chance as $\frac{1}{10}$th that a single sheet or stratum of stars should be seen edgeways projected into a narrow band, the chance that two such sheets should be so projected would be only $\frac{1}{100}$th. Irregularities of the Milky Way are manifest to everyone, and Sir William as well as Sir John Herschel agree in describing many streams branching from the main stream. 'It will be seen at once that the existence of many such streams goes far to prove the stream form of the galaxy itself.'

In a letter addressed to Sir John Herschel in July 1869 Mr. Proctor gave the following sketches to illustrate his idea of the form of the stream[1] of stars in space which we see projected into the Milky Way; he says, the

[1] This letter was printed in *Knowledge* for November 1, 1885.

well-defined edges of parts of the Milky Way prove that we are outside its streams, for no cluster of objects within which (cluster) the observer is situated can anywhere appear with well-defined edges. The great gap in Argo, he further remarks, is not explicable on either the disc or the flat ring theory; the 'coal-sack,' though more readily explicable on the latter than on the former, remains a difficulty with either, but is easily explicable by the stream theory. The lucid stars near to the Milky Way seem to be intimately associated with it, especially in certain regions. Along that part of the Milky Way which Mr. Proctor considered to correspond to the nearest part of the spiral, the stars have, in many instances, singularly large proper motions. Also, added Mr. Proctor, '61 *Cygni* and *α Centauri*, the nearest known stars, are on this branch.' When the irresolvable nebulæ in your large catalogue (Sir John Herschel's) are isographically distributed, they show a marked preference for extra-galactic space. Their withdrawal from a given great circular zone, if not accidental, indicates their association with the sidereal scheme by some law.

Writing in 1886 Mr. Proctor says: 'I assert that the naked eye appearance of the Milky Way is sufficient evidence on which to ground the belief that there is a distinct ring of matter out yonder in space, and that this ring is not flattened, as Sir John Herschel thought, but is (roughly speaking) of nearly circular section throughout its length.'

Fig. 440 was constructed by Mr. Proctor to illustrate his latest views as to the actual form of the galactic stream. The outer stream (marked fig. 1) represents the galaxy as actually seen in the heavens. The mode of projection must be conceived to be as follows: Suppose that on a celestial globe a band were taken, including the whole of the Milky Way, and that this band is spread as a long straight strip on a plane surface. If then we conceive the band turned into a circular strip, by the uniform contraction of one of its edges we should have such a map as fig. 1.

At first Mr. Proctor says he felt a difficulty in conceiving how, if the galaxy were really a stream of relatively small stars, the interruptions in the Milky Way, its variations in brilliancy, and the lacunæ in the stream, could be accounted for by any single stream, however shaped. But at length he was able to construct a single spiral curve which seemed to him completely to meet all the requirements of the problem. The curve is shown in fig. 2, which is supposed to exhibit the actual figure of the galactic stream in space. It is so situated that the various lines drawn from our sun, supposed to be at S, intersect the various portions of the figure representing the real galactic stream, opposite the regions in which these lines meet the figure of the galaxy on our heavens.

Line 1 passes through a gap between the two loops of the galactic spiral. This seems (wrote Mr. Proctor) a simple explanation of what has hitherto been admitted to be one of the most perplexing features of the Milky Way. Passing to position 2 the line crosses two branches of the curve, and the coal-sack is accounted for by the deviation of one branch (or both branches) slightly from the mean galactic plane. From position 3 the line crosses one

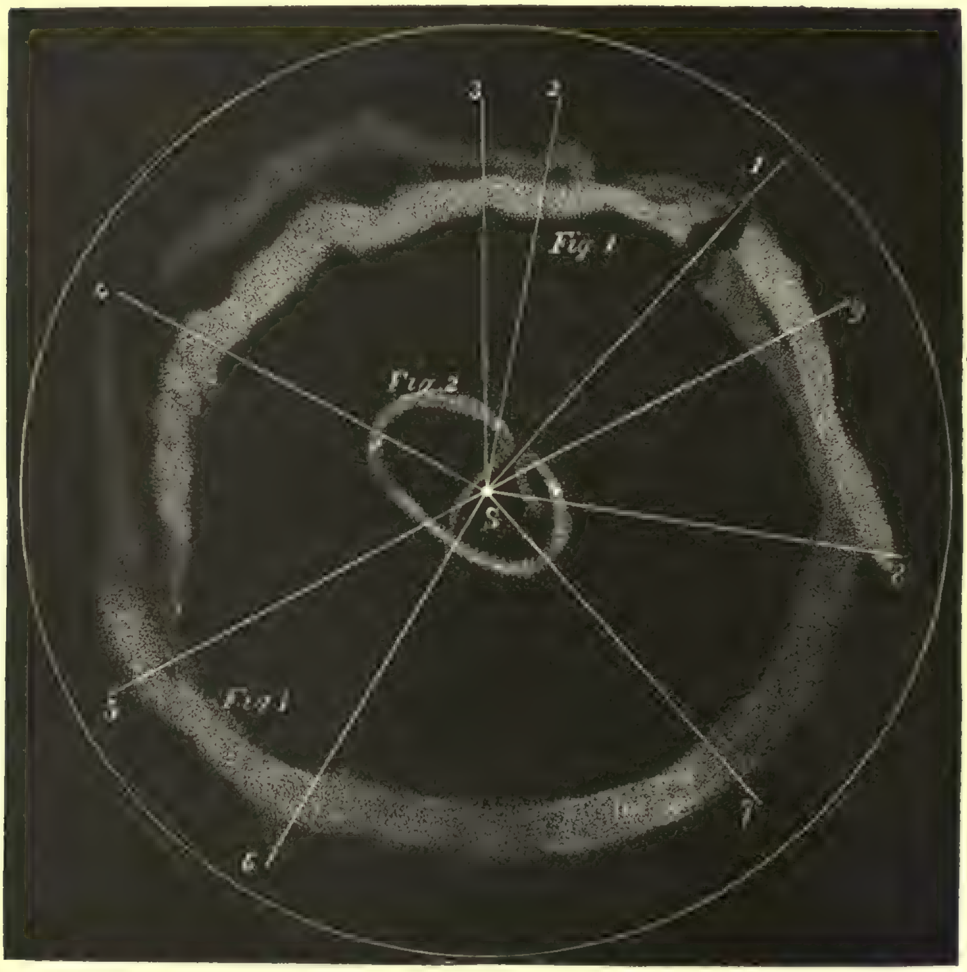


FIG. 440. – (Fig. 1) The Milky Way as seen in the heavens. (Fig. 2) The Spiral Stream which Mr. Proctor assumed represented the actual form of the Milky Way.

branch at a very small distance, the other being much farther off. This corresponds (says Mr. Proctor) closely with the appearance of the two branches, the continuous one being much the brighter, some portions along this part of its length being described by Sir John Herschel as singularly bright. The further branch attains along S 4 so great a distance from the sun as to become invisible. This corresponds with the *mode* of the discon-

tinuity of this part of the Milky Way, for each of the broken divisions *loses itself*, not terminating abruptly like the two fan-shaped terminals opposite the line S 1.

Near this portion of the circuit we are provided with an explanation of what had always been looked upon as a great difficulty. Where the two branches start from the coal-sack in Cygnus (on S 5) the northern branch is much the brighter; but presently the northern branch grows fainter and ultimately vanishes, while the southern grows brighter and brighter. This Mr. Proctor believed was fully accounted for by the figure he assigned to the spiral. The projection at 6 he believed could be accounted for by assuming the end of the spiral to be curved back as shown in the figure. The faintness at 7 was accounted for by the distance of the spiral stream; the projection at 8 by the overlapping end of the stream, which also caused the appearance of the vacuity at 9.

Mr. Proctor is careful to state that[1] 'he did not wish to assert that the actual form of the galaxy in space is that shown in fig. 2.' He adds, 'I yet think it probable that the order of its windings resembles that shown in the figure. . . . I feel convinced, further, that the study of the Milky Way as represented in fig. 1 will at once dispose of the notion that the galaxy can be either a cloven disc or a flat ring, or that the section athwart any branch of it can be otherwise in general than roughly circular.' The latter paragraph seems very fairly to sum up the work which Mr. Proctor accomplished with regard to the structure of the Milky Way. He made it evident that the Milky Way is a stream, or collection of streams, of stars of roughly circular section, and he showed that the distribution of the brighter stars and nebulæ is more intimately connected with the form and curvings of this stream than had hitherto been supposed. Hence he boldly asserted that the brighter stars as well as the nebulæ form part of one system. But he did not, it seems to me, succeed in showing that the particular form of spiral he drew corresponds to the actual form of the Milky Way in space, or even that the stream of the Milky Way is spiral. Mr. Proctor's theory assumes that the apparent doubling of the nebulous stream through half of its course around the heavens is due, not to the branching or division of the stream, but to the projection of two different portions of one gigantic stream, one branch, the northern, being situated at an immense distance behind the other. Mr. J. R. Sutton has very well summed up the arguments[2] which may be used against Mr. Proctor's spiral stream hypothesis. He says Mr. Proctor has assumed that the brightness of the

[1] *Knowledge* for April, 1886, p. 184.

[2] *Illustrated Science Monthly*, vol. ii. pp. 63, 199; and *Knowledge*, March, 1891, p. 42.

various parts of the Milky Way is a rough test of the distance of its parts ; a curious mistake for one so conversant with the laws of brightness. It may be shown that brightness alone is no test of distance. For let a be the apparent area of any small portion of the Milky Way at some distance taken as unity, and containing n stars, λ the average amount of light received from each star, β the average brightness of the area. If this area be removed to a distance, d, the light received from each star will be reduced to $\frac{\lambda}{d^2}$; but the apparent area containing the n stars will be reduced to $\frac{a}{d^2}$, and hence the stars are apparently compressed into a smaller area in the same proportion as their light is reduced ; β, the average brightness of the area, therefore remains unaltered. The apparent breadth of the galaxy would be a much safer test of its distance than the brightness. Although its actual breadth may vary between very wide limits, as a matter of fact the broadest parts of the Milky Way are, on the whole, the faintest ; whereas at its narrowest part, at the 'isthmus' leading into the Southern Cross, it is almost at its maximum brilliancy.

During the course of his long review of the elder Herschel's cloven-flat-disc theory, Mr. Proctor pointed out again and again the remarkable tendency of the brighter stars to congregate along the course of the Milky Way and its branches. This feature is so noticeable in the Southern hemisphere that on a moonlight night, when the fainter lucid stars are invisible, the position of the Milky Way can be traced by the condensation of the brighter stars along its course. There is no antecedent reason why the brighter stars should collect on one portion of the Milky Way rather than on another ; and hence we may assume that, if the fainter parts of the Milky Way are the most distant, they would have, say, 4th or 5th magnitude stars collected upon them, in the same way that the brighter parts have 1st and 2nd magnitude stars collected upon them. But, continues Mr. Sutton, it is a fact that there are stars on the assumed more distant streams, obviously associated with them, as bright as those on the assumed nearer ones.

It thus seems hardly probable that one branch of the galaxy is at a much greater distance than the other, though the two branches are not necessarily closely associated. Gould, after a careful study of the Milky Way during the preparation of the maps which accompany the Argentine Catalogue, was inclined to consider it as the resultant of two or more superposed [1] galaxies. We shall be in a better position to judge as to what convolutions of the stream are due to different branches seen in projection, and what

[1] *Uranometria Argentina*, p. 381.

are due to actual irregularities in the form of the stream or streams when the whole of the galaxy has been photographed. At present the complicated structure exhibited by parts of the main stream which have been photographed, notably the *Sagittarius* region shown in Plate XXV., shows that all the variations in brightness of the main stream cannot be accounted for by the convolutions of a single cylindrical stream seen in projection. It appears rather to be an aggregation of stellar structures of very complicated form.

(1411.) Sir John Herschel,[1] in describing the Milky Way as observed by him at the Cape of Good Hope in the years 1834–8, noticed that the Milky Way is crossed by a zone of large stars which stretches round the heavens, passing through the brilliant constellation of Orion, the bright stars of Canis Major, and almost all the more conspicuous stars of Argo, the Cross, the Centaur, *Lupus* and *Scorpio*. He says: 'A great circle passing through ϵ *Orionis* and α *Crucis* will mark out the axis of the zone in question, whose inclination to the galactic circle is therefore about 20°, and whose appearance would lead us to suspect that our nearest neighbours in the sidereal system (if really such) form part of a subordinate sheet or stratum deviating to that extent from parallelism to the general mass, which, seen projected on the heavens, forms the Milky Way.'

(1412.) Dr. B. A. Gould, in a paper published in 1874,[2] and again in the *Uranometria Argentina*,[3] infers the existence of such a stellar cluster about our sun from the excess of the number of stars brighter than the 4th magnitude above the number which might be inferred from the numbers of 5th, 6th, and 7th magnitude stars on the assumption that stars of all magnitudes are distributed uniformly in space. Considering only the stars above the 4th magnitude, he thinks that there is evidence of the bifurcation of the belt of brighter stars to which Sir John Herschel drew attention, and he sums up the results of his investigation as follows :—

1. There is in the sky a girdle of bright stars, the medial line of which differs but little from a circle, inclined to the galactic circle by a little less than 20°.

2. The grouping of the fixed stars brighter than $4^{m}\cdot1$ is more symmetric, relatively to that medial line, than to the galactic circle; and the abundance of bright stars in any region of the sky is greater as its distance therefrom is less.

3. The known tendency to aggregation of faint stars toward the Milky

[1] Sir John Herschel's *Results of Astronomical Observations at the Cape of Good Hope*, p. 385.

[2] Entitled 'On the Number and Distribution of the Bright Fixed Stars,' first published in the *Proc. Amer. Assoc. for Adv. of Science*, 1874, p. 118.

[3] *Uranometria Argentina*, pp. 348–370.

Way is according to a ratio which increases rapidly as their magnitudes decrease, and the law of which is such that the corresponding aggregation would be scarcely, if at all, perceptible for the bright stars.

4. These facts, together with others, indicate the existence of a small cluster, within which our system is eccentrically situated, but which is itself not far from the middle plane of the galaxy. This cluster appears to be of a flattened shape, somewhat bifid, and to consist of somewhat more than 400 stars of magnitudes from the 1st to the 7th, their average magnitude being about 3·6 or 3·7.

5. The general distribution of the fixed stars according to magnitude does not appear capable of being well represented by any simple algebraic expression. Yet by adopting the data of the preceding paragraph, and supposing the several magnitudes of the stars in the cluster to follow the law of probabilities, we obtain for each class of magnitudes a number which, being subtracted from the observed number in the sky, leaves a system of distribution which may be represented by the expression $\Sigma_m = a\,b^m$, within the limits of errors of observation.[1]

6. The accordance thus obtained holds good for the stars of both hemispheres down to the lowest limits of magnitude for which trustworthy enumerations exist, and this whether we employ the numbers of the *Durchmusterung*, of Argelander's and Heis's Uranometries, or of this present work.

7. The form of the expression $\Sigma_m = a\,b^m$ is that which corresponds to the hypothesis that in general the stars are distributed at approximately equal distances from one another, and are of approximately equal intrinsic brilliancy. It is, however, not requisite for its applicability that their distribution be uniform in all directions, but only that their number be proportional to the volume of the spherical shell within which they are contained.

8. Each of the authorities and each hemisphere affords data from which results essentially the same value for the ratio b, the differences in the data being in every case represented by differences in the coefficient a. The value thus obtained for b corresponds to the light ratio, 0·4028 for descending, or 2·4827 for ascending, magnitudes.

(1413.) Professor E. C. Pickering has made a still more careful investigation as to the number of stars of various brightness down to those of the $6\frac{1}{2}$ magnitude of the photometric scale in the Northern heavens, and he has compared his own results with those of Professor Gould for the Southern heavens, as given in the *Uranometria Argentina*, having first made certain small changes in Gould's magnitudes to reduce them to the accurate photo-

[1] Σ_m denotes the total number of stars down to the magnitude m inclusive.

metric scale used at Harvard. All Professor Pickering's magnitudes have been determined with the meridian photometer,[1] an ingenious instrument of his invention which enables him to compare all the stars in the Northern heavens with the pole star, which he has shown does not vary sensibly in brightness.

Professor Pickering gives the number of stars of each half-magnitude down to the $6\frac{1}{2}$ in his own Photometric Catalogue as follows, Br. signifying brighter than the 0·5 mag. The corresponding results, deduced by Professor Pickering[2] from the *Uranometria Argentina* for the Southern heavens, are given to compare with them.

	Br.	0·5	1·0	1·5	2·0	2·5	3·0	3·5	4·0	4·5	5·0	5·5	6·0	6·5
Photometric Catalogue .	4	2	5	3	24	22	63	95	170	353	650	1090	1118	439
Uranometria Argentina .	2	3	4	5	15	24	41	74	126	234	426	681	1189	2891

These figures, which refer only to stars large enough to be seen with the naked eye, seem to indicate a clustering of stars in the sun's neighbourhood, or that the stars near to us are larger than those at a distance, for the numbers corresponding to the various magnitudes increase less rapidly than the cubical space contained between spherical shells corresponding to the assumed average distance of stars of the various magnitudes. In the following diagram, fig. 441, the dotted line is drawn through points, corresponding to the density of aggregation of stars of the various magnitudes in the Northern hemisphere, as shown in the Photometric Catalogue, and the

[1] The meridian photometer consists of two telescopes side by side pointed nearly east and west. In the front of each object-glass is placed a reflecting mirror at an angle of 45°, so that the rays from the pole star are reflected into one object-glass, and rays from a star on the meridian are reflected into the other. The second telescope can be turned round its optic axis, carrying the mirror in front with it, so that any star on the meridian can be brought into the field of view. The images of the pole star and the star on the meridian are then compared by means of a double-image prism, which brings two contrary polarised images, one of the pole star, and the other of the meridian star, side by side in the field of the eye-piece, and the brightness of the two images is equalised by turning a Nicol prism either in front of or between the lenses of the eyepiece. In the course of about a minute, while the meridian star transits the field, four readings can be taken of the four positions of the Nicol which produce equality of the images, and a very satisfactory determination of the relative magnitude of the two stars can be deduced. In this way Professor Pickering has measured the magnitude of 4,260 stars in the Northern heavens. The measures are given in the Harvard *Photometry*, published in 1884; and he is now occupied with the still larger work of measuring the magnitude of nearly 80,000 stars above the 8th magnitude contained in Argelander's *Durchmusterung*. This method of judging of the equality in brightness of images is much more satisfactory than the method of measuring the light of a star by extinguishing its image by a dark wedge, or by limiting the aperture of the viewing telescope, for the eye is constantly strained when determining the moment when a star is lost sight of by reason of its faintness.

[2] The numbers are taken from tables published in the Harvard *Annals*, Vol. XIV. pp. 479–80. Each half-magnitude is regarded as comprising all stars not differing from it more than two-tenths; thus all stars from 2·8 to 3·2 mag. are given under the heading 3 mag. In drawing the curves in fig. 441 the consecutive half-magnitudes have been grouped together, giving a somewhat smoother curve than would have been obtained if the number of stars of each half-magnitude had been taken separately.

continuous line through similar points for the stars in the Southern hemisphere, as shown in the Argentine Catalogue.

(1414.) According to the photometric scale, a star of any magnitude is about $2\frac{1}{2}$ times as bright as a star of the next magnitude below it. About half a century ago Sir John Herschel noticed that the light given by an average star of the 1st magnitude is about 100 times as great as that received from a 6th magnitude star. This would give $\sqrt[5]{100}$ for the light-ratio used in passing from one magnitude to another of the naked-eye stars—if all the steps were equal. It was known that the magnitudes which had been used by the Herschels, Struve, Argelander, Heis, and others, though they practically agreed with such a scale for the naked-eye stars, diverged from it to a greater

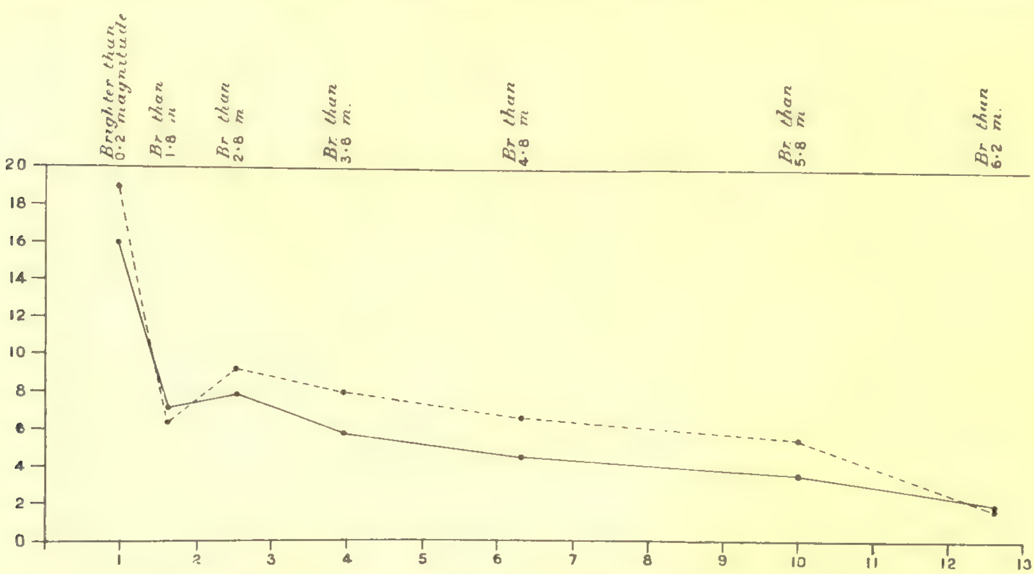


Fig. 441.—The ordinates in the diagram correspond to the thickness or density of distribution of the stars of various magnitudes in space, and the abscissæ to the assumed distances of the various magnitudes. The dotted line for the Northern heavens, and the continuous line for the Southern heavens.

or less extent for the smaller telescopic stars ;[1] and some forty years ago it was proposed by Pogson to reform the systems in use by adopting a scale with a uniform light ratio of $\sqrt[5]{100}$—that is, about 2·512—for all magnitudes. This scale is now pretty universally adopted, and is called the *photometric scale* or *absolute scale* of magnitude.

If a 1st magnitude star such as α^2 Centauri were removed to a distance a little greater than one and a half times its present distance, it would appear to shine as a 2nd magnitude star, for its light would be diminished in

[1] A discussion of some of these divergences is given in an interesting paper by the Rev. W. R. Dawes, published in the *Monthly Notices*, vol. xi. p. 187. Dawes assumed that the light ratio between successive magnitudes was 4 for telescopic stars. Struve made use of a similar ratio; while the Herschelian ratio seems to have been 2.

inverse proportion to the square of its distance ; more exactly, its light would be diminished a whole magnitude if its distance were increased in the ratio 1 to 1·585, two magnitudes if its distance were increased in the ratio 1 to 2·512, and five magnitudes if it were removed to ten times the distance. The number 1·585 may be called the distance-ratio, for it corresponds to the proportional distance to which a star must be removed in order that its light may be reduced one stellar magnitude. The volume included within spherical shells with such increasing radii increases in the proportion 1 to 3·981 ; so that if stars were distributed uniformly in space, and were all of the same actual magnitude and brightness, and there were no absorption or loss of light in transmission through space, we ought to see about four times as many stars of each succeeding magnitude as we proceed downward in the scale.

This law as to the increase in the numbers of stars of the various apparent magnitudes would not hold good except for the smaller magnitudes, if stars of different degrees of intrinsic brightness or actual magnitude were distributed uniformly in space, and there was no absorption of their light in transmission from a distance, but it seems from the considerations mentioned in Arts. 1399, 1400 that such absorption probably exists. Its effect would be to cut down the light of the more distant stars, leaving an apparently undue increase in the numbers of the stars of the higher magnitudes, from which a clustering in the sun's neighbourhood might be inferred. The unduly large numbers of the brighter stars of the first four magnitudes indicated by the curves in fig. 441 can, however, hardly be due to this cause, for when a similar curve is drawn corresponding to the number of stars of the various magnitudes down to the 9th, the curve does not continue to fall as uniformly as it would if the decrease in the numbers indicating the density of the grouping of the apparently smaller stars was due only to the absorption of light in its transmission through space. The numbers given by Littrow in his analysis of Argelander's magnitudes run thus :—

Limiting magnitudes	Number of stars according to Littrow	Calculated numbers on the assumption that the 8th mag. stars correspond to average density	Radius of sphere forming exterior limit of distance (distance of 6th mag star = 1)
1·0 to 1·9	10	5	0·156
2·0 to 2·9	37	15	0·247
3·0 to 3·9	130	60	0·391
4·0 to 4·9	312	238	0·618
5·0 to 5·9	1,001	941	0·977
6·0 to 6·9	4,386	3,718	1·545
7·0 to 7·9	13,823	14,697	2·443
8·0 to 8·9	58,095	58,095	3·863
9·0 to 9·5	237,131	99,631	5·092

The rapid increase shown in the number of stars between the 9th and the 9½ magnitudes is no doubt due to numerous stars fainter than the 9½ magnitude (which were near to the limit of vision with Argelander's telescope) being

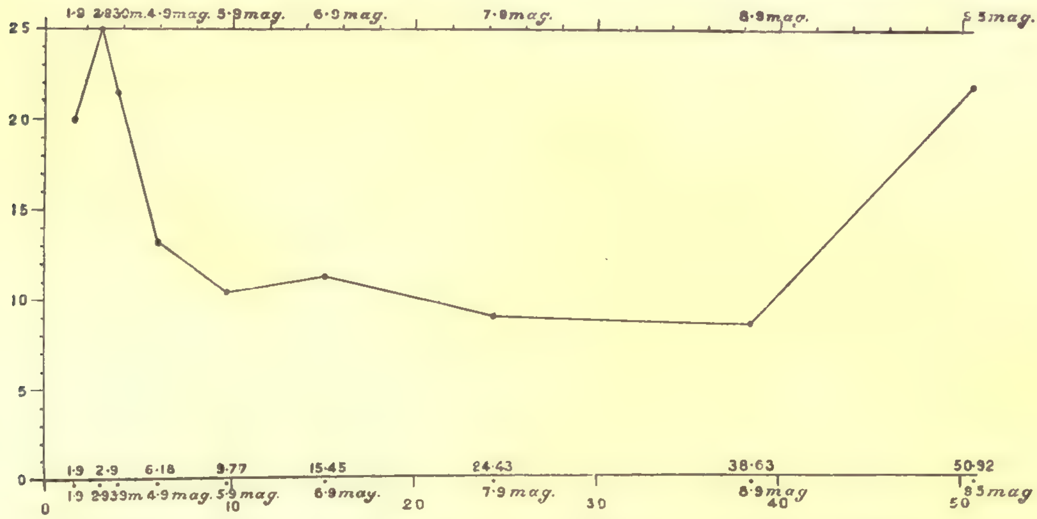


FIG. 442.—The ordinates in the diagram correspond to the thickness or density of distribution of stars of various magnitudes down to 9½ mag., as derived from Littrow's analysis of Argelander's *Durchmusterung*; the abscissæ to the assumed distances.

included in a class to which they do not belong. This curve, as well as the curves in fig. 441, derived from Prof. Pickering's more exact classification,

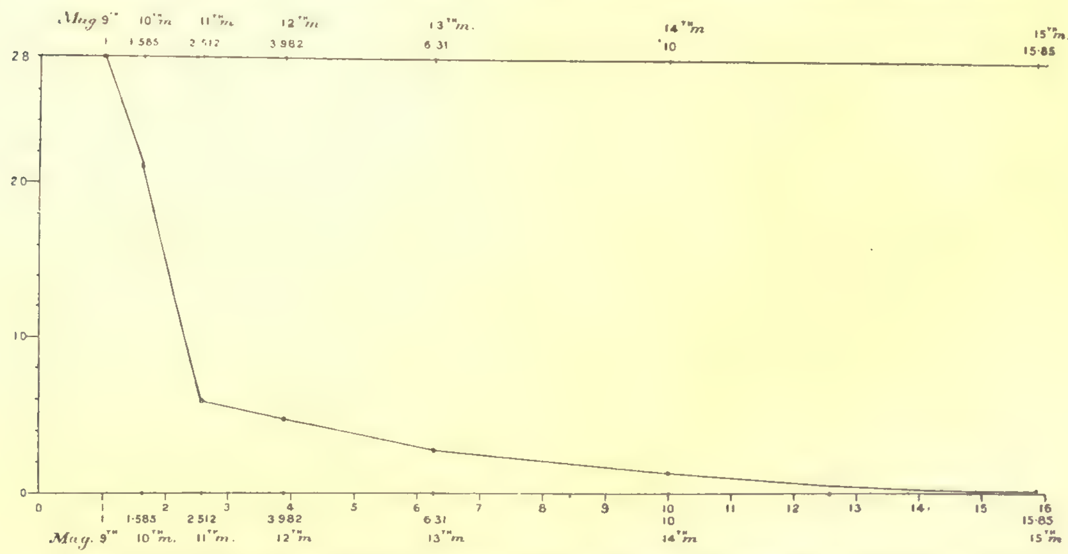


FIG. 443. The ordinates correspond to the density of distribution of the stars between the 9th and 15th magnitudes within 1° of the North pole of the heavens.

seems to show a greater increase in the relative number or thickness of distribution of the apparently larger stars as compared with those of the 6th, 7th, 8th, and 9th magnitudes. The curves in figs. 441, 442 do not correspond

as exactly as might at first sight be expected, because the limits of the classes or groups of stars which have been taken together are not similar in the two cases; but this only shows more clearly that the general law of decrease exhibited corresponds to an actual fact, and is not merely an accidental coincidence depending on the groups of magnitudes selected.

Prof. Pickering, in Vol. XVIII. of the Harvard 'Annals,' has given the magnitudes of 947 small stars, ranging from the 8th to the 15·3 magnitude, within one degree of the pole of the Northern heavens. The numbers of stars of successive magnitudes in this limited area also show that the smaller stars are too few, as compared with the space they would be assumed to occupy, on the theory of uniform distribution and perfect transparency of space. The numbers given by Prof. Pickering[1] are—

Magnitudes	8·0	9·0	9·5	10·0	10·5	11·0	11·5	12·0	12·5	3·0	13·5	14·0	14·5	14·8	14·9	15·0	15·1	15·2	15·3
No. of stars	1	5	2	12	9	15	9	24	50	85	103	143	253	99	19	58	35	13	11

(1415.) It seems from these curves that there is a decrease in the density numbers corresponding to the various star magnitudes, which might be accounted for by a comparatively dense clustering of stars in the immediate neighbourhood of the sun, and then a gradual and progressive thinning out of the cluster at a distance from the sun greater than that corresponding to the average distance of stars of the 5th and 6th magnitudes—a theory of the universe involving a symmetry with regard to the earth's place which is not likely to be accepted nowadays. But there are two other assumptions which will account for the observed facts.

(1) There may be a cluster of some 400 or 500 stars about the sun, as assumed by Prof. Gould, and a comparatively uniform distribution of stars outside the limits of the cluster, the light of the more distant stars being cut down in increasing proportion according to their distance, owing to the imperfect transparency of space, either by reason of meteoric and cometary bodies scattered through space, as well as dark stars and opaque regions of nebulous matter, or the ether itself may not be *perfectly* elastic and transparent.

Or (2) there may be no actual clustering of the stars in the neighbourhood of the sun, but the apparent clustering may be due to the number of actually large stars distributed in the space near the sun as compared with the number of actually small stars at greater distances; and the more gradual

[1] Harvard *Annals*, vol. xviii. p. 202; and see also Miss A. M. Clerke's *System of the Stars*, p. 383, where a somewhat similar diagram is given; but the abscissæ do not correspond to the assumed distances of the various apparent magnitudes. In fig. 443 the consecutive half-magnitudes have been grouped together so as to give a somewhat smoother curve.

diminution of density after the 6th magnitude may be caused by opaque matter in space, or the imperfect transparency of the ether alluded to above.

(1416.) Several facts tend to prove that there are great differences in the actual magnitude as well as in the intrinsic brightness of stars. Thus the great difference in the apparent magnitude of stars which apparently belong to the same cluster, and therefore are probably at nearly the same distance from us, as well as the great difference in light given by stars which have been proved to be associated with one another in binary systems, show that some stars give many thousand [1] times as much light as others, while the phenomena exhibited by the Algol type of binaries prove that stars differ in actual brilliance as well as in diameter.

For simplicity of illustration, let us consider the stars as divided according to their actual magnitude or brilliance into three classes. The first class, A, being of such dimensions and brightness that they would appear as stars of the 1st magnitude if they were separated from us by a distance equal to the distance of *α Centauri.* The second class, B, of such dimensions that they would appear as stars of the 2nd magnitude if at a distance equal to that of *α Centauri*; and the third class, C, of such dimensions that they would appear as stars of the 3rd magnitude if seen from a similar distance, the space within which they are distributed being perfectly transparent. Let us suppose, also, all three classes of stars to be distributed uniformly in space, so that, while there are no stars nearer to us than *α Centauri*, there are a stars of the first class within the space contained between a sphere of radius equal to the distance of *α Centauri* and a sphere of 1·585 times that radius; and that there are b stars of the second class, and c stars of the third class within the same region, and so on uniformly outwards. An observer would on looking at the heavens see a stars which appeared above the 2nd magnitude, and $b + (3{\cdot}981)\ a$, stars between the 2nd and 3rd magnitudes—that is, b stars which would at the distance of *α Centauri* look like 2nd magnitude stars—and about four times as many stars of the first or A class, situated in the second spherical shell or space between the spheres whose radii are respectively 1·585 and 2·512 times the distance of *α Centauri.*

Of stars between the third and fourth magnitudes he would see,

[1] Sirius is given in the Photometric Catalogue as of the −1·4 magnitude, while its small companion has been estimated to be of the 10th magnitude. The larger star is consequently about 11½ magnitudes brighter than its companion—that is, it gives about 40,000 times as much light; while, according to Auwers, it has only about double the mass of its faint companion. The stars of the Pleiades cluster range from about the 3rd magnitude to certainly below the 15·5 magnitude, thus showing a range of more than 12½ magnitudes. In other words, the brighter stars of the group give us 100,000 times as much light as other stars which appear to be associated with them; though it seems probable that all the Pleiades stars do not belong to the same cluster, some few, mostly small stars, having a greater proper motion, which seems to render it likely that a small group of stars is seen in projection on the larger cluster.

$c + (3{\cdot}981)\,b + (3{\cdot}981)^2\,a$; that is, c stars, which would look like stars of the 3rd magnitude if they were all at the distance of *α Centauri*; about four times as many stars of the next larger or B class, situated in the space between the spheres referred to above; and nearly sixteen times as many stars of the A class, situated in the space between the spheres whose radii are respectively equal to 2·512 and 3·982 times the distance of *α Centauri*.

Of stars between the 4th and 5th magnitudes he would see $3{\cdot}981\,[c + (3{\cdot}981)\,b + (3{\cdot}981)^2 a]$, and so on, the number of stars of each successive magnitude being now always about four times as numerous as the number of the magnitude below; that is, after the 4th magnitude the numbers would increase in geometrical progression with a constant ratio 3·981, which it is convenient to speak of as the space ratio for successive magnitudes; while below the 4th magnitude the ratio of numbers would depend on the values a, b, c, representing the thickness of distribution of stars of the various classes. But whatever supposition we make with regard to the proportionate numbers of the stars of the A, B, and C classes in the uniform universe we have supposed, the curve of density, drawn as the curves in figs. 441, 442, 443 are drawn, would appear to show a decrease of density for stars apparently greater than the 4th magnitude. For the ratio of the numbers of stars of successive apparent magnitudes below the 4th would be greater than the space ratio; thus—

$$\frac{\text{No. of stars between the 2nd and 3rd magnitudes}}{\text{No. of stars between the 1st and 2nd magnitudes}} = \frac{b + 3{\cdot}981\,a}{a} = 3{\cdot}981 + \frac{b}{a};$$

$$\frac{\text{No. of stars between the 3rd and 4th magnitudes}}{\text{No. of stars between the 2nd and 3rd magnitudes}} = \frac{c + 3{\cdot}981\,b + (3{\cdot}981)^2\,a}{b + 3{\cdot}981\,a} = 3{\cdot}981 + \frac{c}{b + 3{\cdot}981\,a}$$

With no absorption of light in space, the density curve corresponding to the various magnitudes would in the uniform universe we have supposed at first rise and then pass into a straight line parallel to the abscissa, as shown in fig. a; while in such a uniform universe a want of perfect transparency in space would cause the curve to slope downwards for all the magnitudes, but more rapidly for the lower magnitude stars; and the curve would be less steep for the higher apparent magnitudes, where the inequalities in the actual size of stars as above assumed began to tell, as shown in fig. b.

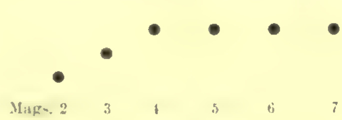


FIG. *a*.

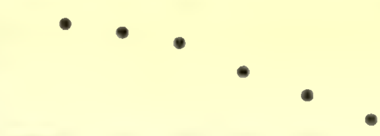


FIG. *b*.

(1417.) We have comparatively few data to go upon in determining the probable relative proportion of actually large and bright stars to actually small and

faint stars. In clusters and in the denser parts of the Milky Way it seems probable that the number of actually small stars greatly exceeds the number of actually large stars. Thus Prof. E. C. Pickering, in Vol. XVIII. of the Harvard 'Annals,' p. 202, gives the following figures as representing the number of stars of the various magnitudes below the 8th in the Pleiades, comprised within a region extending over 5m of Right Ascension and 45′ of Declination.[1]

Magnitudes	8·0	8·5	9·0	9·5	10·0	10·5	11·0	11·5	12·0	12·5	13·0	13·5	14·0	14·5	14·8	14·9	15·0	15·1	15·2
No. of stars	9	1	6	3	8	5	10	25	18	25	22	28	22	42	15	5	18	13	1

In order to obtain a comparatively smooth curve, we will take these numbers and group them into classes which contain all the stars included within a range of one magnitude, and neglect the number of stars of higher magnitude than the 8th, which amount to three between the 3rd and 4th magnitudes, three between the 4th and 5th magnitudes, one between the 5th and 6th magnitudes, five between the 6th and 7th magnitudes, and twelve between the 7th and 8th magnitudes.

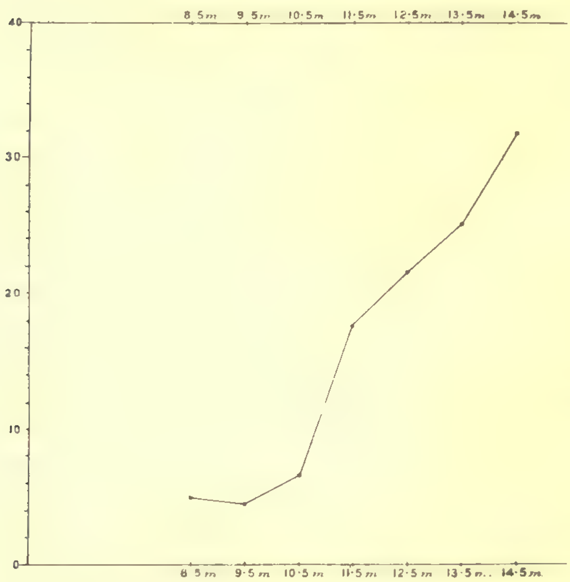


FIG. 444.—The ordinates of the curve represent the number of stars in the Pleiades cluster of various magnitudes from the 8th to the 15th comprised within a limited area about Alcyone.

The curve shows that in the Pleiades group the actually small stars, or those which are comparatively dull and give but little light, are much more numerous than stars of apparently greater magnitude, and that the number of stars included within the limits of successive photometric magnitudes increases pretty steadily down to the 15th magnitude. If we may assume that this law does not only apply to star clusters, and that there is a similar increase in the number of small stars in other regions of space, the curve of densities corresponding to the apparent star magnitudes, seen by an observer in a uniform universe of stars differing only fifteen magnitudes in actual brightness, ought to slope rapidly upwards to the 15th magnitude, instead of sloping downwards as in figs. 441, 442, 443.

[1] The selected region is included between 3 minutes preceding and 2 minutes following the star Alcyone, and in declination between 30′ north and 15′ of the same star.

(1418.) The number of stars of the different apparent magnitudes actually observed gives us two alternatives to choose from: (1) Either we are situated near to the centre of a limited universe, in which the stars grow smaller or dimmer or are more thinly scattered as we recede from the sun; or, (2) If the outer regions of space are occupied by luminous stars similar to those near us, and scattered with any approximation to the density with which they are scattered in the region within forty times the distance of *a Centauri*,[1] there must be absorption of light in space.

(1419.) The great proper motions of many stars, of which we shall presently have to speak as being greater than could be caused by the action of gravity towards any possible star cluster, would lead us to suppose that outer space cannot be thus void of stars. For in the ages with which we have to deal in considering the evolution of the universe, the proper motions of such swift-moving stars would carry them out of the region which man is able to explore with the largest telescopes, a region which is smaller than is generally realised; thus if α^2 *Centauri*[2] were removed to ten times its present distance and space was perfectly transparent, its light would be decreased a hundred-fold, and it would shine as a 6th magnitude star. If it were removed to a hundred times its present distance, it would shine as an 11th magnitude star; and if it were removed to a thousand times its present distance, it would shine as a 16th magnitude star, and would at most only just be visible in the Washington 26-inch telescope. If it were reduced another magnitude, it would not be visible in the Lick telescope. The calculation on which this statement is based neglects the absorption of light by the thick object-glasses of these large telescopes, and neglects the absorption of light in space. Assuming with Prof. Young that for normal eyes with a good telescope the *minimum visible* for a 1-inch aperture is a star of the 9th magnitude,[3] stars of the

[1] That is the assumed average distance of a star of the 9th magnitude.

[2] The larger component of the binary star *a Centauri* is known as α^2 *Centauri*; according to Pickering and Gould its magnitude is 1·0. The smaller component, α^1 *Centauri*, is rated as of the 3·5 magnitude.

[3] The above estimate of Professor C. A. Young is given in his *Text-book of General Astronomy* (sec. 822). Mr. Dawes found that the diameter of the pupil of his eye by starlight was as nearly as possible a quarter of an inch; see *Monthly Notices*, vol. xi. p. 197. I have measured the diameter of the pupil of several persons whom I believed to have keen sight, amongst others the observing eyes of the Rev. T. W. Webb, Mr. Burnham, and the late Dr. H. Draper, and I found that a quarter of an inch about corresponded to the maximum dilation of the pupil in viewing faint objects. A telescope of an inch diameter would consequently collect about sixteen times as much light as would enter the pupil of the unassisted eye, and, according to Dawes, an average star of the 6th magnitude, taken from Argelander's *Uranometria Nova*, about corresponds to the limit for steady visibility on clear nights with the unassisted eye; such a star is of the 6th magnitude in the photometric scale, and gives 15·85 times as much light as a star of the 9th magnitude of the photometric scale. The power used with such a telescope makes some difference, as it increases the contrast between the brightness of the star and the background on which it is seen – the light of the background being dimmed by magnification, while the star in a good defining telescope is but slightly dimmed by moderate

14th magnitude would be just seen with a telescope of ten inches aperture; and it would need a telescope of 15·85 inches aperture to see a star of the 15th photometric magnitude, a telescope of 25·12 inches aperture would be needed to see a star of the 16th magnitude, and a telescope of 39·81 inches to see one of the 17th magnitude The great Lick telescope is 36 inches in diameter, and, considering the loss of light due to the great thickness of its object-glass, it seems probable that stars less than the 16th magnitude of the photometric scale are not visible with it. Consequently, if α^2 *Centauri* were removed to a thousand times its present distance from us, and there were no absorption of light in space, it would appear as a star of the 16th magnitude, and would probably be only just visible with the Lick telescope.

It is difficult to make any estimate as to the loss of light due to opaque bodies or absorption in passing through space from a distance where a star such as α^2 *Centauri* would appear to us as a star of the 16th magnitude without making assumptions as to the uniform distribution of stars which we know to be false. We may, however, make some approximation towards an estimate of such loss by considering the number of stars of the various apparent magnitudes actually observed, compared with those which would be seen in a uniform universe of stars all of the same size arranged as thickly as they would need to be arranged to give the number of stars of the 6th magnitude actually observed.

According to Littrow's analysis of Argelander's magnitudes, there are in the region included in the *Durchmusterung* survey 1,001 stars between the 5th and 6th magnitudes, or more exactly 1,001 stars included within the limits of the 5·0 to the 5·9 magnitudes. This would, according to the space ratio, give 63,159 stars between the 8·0 and 8·9 magnitudes in the Northern hemisphere, which, if they were distributed uniformly over the sky, would give about 38½ stars within a circular area 2° in diameter. Applying the space ratio still further we should have:—

Limits of magnitude .	9·0 9·9	10·0–10·9	11·0 11·9	12·0–12·9	13·0–13·9	14·0–14·9	15·0–15·9
Calculated no. of stars .	153·2	609·9	2,428	9,666	38,480	15,320	60,990

magnification. Thus Dawes found that he could see a star of the 6th magnitude with a telescope having an aperture of only 0·15 inch when a power of 16½ was used; an observation which he found it difficult to reconcile with the other facts known to him. He noted, however, that within certain limits an increase of magnifying power brought out many small stars which were invisible with a lower magnifier. In the case of the 1-inch telescope referred to above, the loss of light by absorption and reflection at the surfaces of the lenses seems to be about balanced by the increase of contrast with the background due to the power employed. The loss of light by absorption in passing through the glass near to the centre of the Lick object-glass must be considerable. The thickness of the object-glass of the Washington 26-inch refractor at its centre is nearly three inches; thus the flint-glass lens is there 0·96 inch thick, while the crown-glass lens is 1·88 inch thick at its centre. Such a thickness more than halves the intensity of the emergent pencil. Exact measures of the absorption of light by such lenses would be of interest.

Instead of which, the numbers of stars observed by Prof. Pickering within a circular area 2° in diameter about the North Pole,[1] as lying within closely corresponding limits of magnitude, are :—

Limits of magnitude	8·8–9·7	9·8–10·7	10·8–11·7	11·8–12·7	12·8–13·7	13·8–14·7	14·8–15·3
No. of stars within 1° of the North Pole, according to Prof. E. C. Pickering	7	21	24	74	188	396	235

Or, instead of over fifteen thousand stars between the 14th and 15th magnitudes, there are less than four hundred, or only about one four-hundredth part of the calculated number; in other words, about the number of stars which, according to the space ratio, would lie between the 9·5 and 10·5 magnitudes. A similar effect would be produced by absorption of light in space in a uniform universe if stars actually situated at a distance corresponding to the $9\frac{1}{2}$ to $10\frac{1}{2}$ magnitudes were dimmed by absorption so as to appear as stars of the 14th to the 15th magnitude.

With such absorption of light in space we should lose sight of α^2 *Centauri* in the Lick telescope if it were removed to about 150 times its present distance. For, without taking absorption into account, its light would be diminished by increase of distance so as to make it appear as a star of the 12th magnitude; and absorption would further reduce its light through at least another $4\frac{1}{2}$ magnitudes, making it appear as a $16\frac{1}{2}$ magnitude star.

(1420.) Whatever the arrangement or distribution of stars may be in space beyond the region of the Milky Way, there is ample evidence that in the part of space girdled by the Milky Way, in which the sun is situated, there is no uniform distribution of stars, but an evident grouping showing a connection between the Milky Way or stream of small stars we see extending symmetrically round the heavens and stars of other magnitudes. There is also an intimate connection between the distribution of nebulæ in the heavens and the stream of the Milky Way. While the stars and star clusters are grouped more thickly about the region of the Milky Way, the smaller nebulæ seem to shun its neighbourhood and are most thickly distributed in the poles of the Milky Way, showing, as Mr. Proctor remarked, not a want of relationship with the stellar scheme, but an intimate 'relation of con-

[1] The photographs of Mr. Isaac Roberts show that this region is moderately rich in small stars, as compared with other regions outside the Milky Way; he has, with an exposure of 105 minutes, photographed 1,270 stars in this area—that is, about one-third more stars than the number observed and measured by Professor Pickering. The photograph, therefore, probably includes stars down to the 17th magnitude. The average number of stars to the square degree upon it is 404. Rather nearer to the Milky Way in *Taurus* at R. A. 4h. 15m. D. + 19°, with an exposure of 3 hours, Mr. Isaac Roberts obtained an average of 550 stars to the square degree. In *Virgo* at 11h. 36m. D. + 3°, with an exposure of 106 minutes, he obtained only 178 stars to the square degree. In *Virgo* at R. A. 14h. 24 m. D. s 5° 28′, and an exposure of 90 minutes, 203 stars, and in *Pegasus*, at R. A. 23h. 9m. D. + 8° 56′, and an exposure of 2 hours, he obtained 391 stars to the square degree.

trariety,' the existence of one class of bodies seeming to depend on the comparative absence of the other.

Sir William Herschel was the first to notice an intimate connection between the distribution of nebulæ and the brighter stars. He noted that the nebulæ were arranged in streams and clusters separated by dark intervals from the regions containing lucid stars,[1] and he also noticed the aggregation of nebulæ in the poles of the Milky Way. Sir William Herschel observed and registered the places of about two thousand nebulæ, and his son, Sir John Herschel, continued the work and extended it to the Southern heavens, giving in his 'General Catalogue,' published in 1864, the places of 5,079 nebulæ.[2]

Mr. Herbert Spencer seems to have been the first to point out that the observed connection between the distribution of nebulæ and lucid stars proves the nebulæ to belong to our sidereal system, and that they cannot be remote galaxies, as had up to that time been the popular belief; though Sir William Herschel had pointed out at the beginning of this century[3] that many of the larger nebulæ cannot be accounted for as remote clusters of stars too distant to be seen as separate points of light, still it was very generally supposed that many of the smaller nebulæ were galaxies more or less alike in nature to that immediately surrounding us, but so inconceivably remote that, as looked at through the largest telescopes, they appeared like small faint spots of light. In a very thoughtful paper on 'The Nebular Hypothesis,' published in July 1858, in the 'Westminster Review,' Mr. Herbert Spencer wrote:

> If there were but one nebula, it would be a curious coincidence were this one nebula so placed in the distant regions of space as to agree in direction with a starless spot in our own sidereal system. If there were but two nebulæ, and both were so placed, the coincidence would be excessively strange. What, then, shall we say on finding that there are thousands of nebulæ so placed? Shall we believe that in thousands of cases these far-removed galaxies happen to agree in their visible positions with the thin places in our own galaxy? Such a belief is impossible.
>
> Still more manifest does the impossibility of it become when we consider the general distribution of nebulæ. Besides again showing itself in the fact that 'the poorest regions in stars are near the richest in nebulæ,' the law above specified applies to the heavens as a whole. In that zone of celestial space where stars are excessively abundant, nebulæ are rare; while in the two opposite celestial spaces that are furthest removed from this zone, nebulæ are abundant. Scarcely any nebulæ lie near the galactic circle (or plane of the Milky Way); and the great mass of them lie round the galactic poles. Can this also be mere coincidence? When to the fact that the

[1] Sir William Herschel seems to have frequently warned his assistant to prepare to write, since he expected in a few minutes to come to a stratum of the nebulæ, finding himself already on nebulous ground.—*Phil. Trans.* vol. lxxiv. p. 449.

[2] The work has been carried still further by Dr. Dreyer, who in his Catalogue published in the Memoirs of the R.A.S. in 1888 gives the places of 7,840 nebulæ.

[3] See Sir William Herschel's paper 'On Astronomical Observations relating to the Construction of the Heavens,' published in the *Phil. Trans.* for 1811, pp. 261–336.

general mass of nebulæ are antithetical in position to the general mass of stars, we add the fact that the local regions of nebulæ are regions where stars are scarce, and the further fact that single nebulæ are habitually found in comparatively starless spots, does not the proof of a physical connection become overwhelming?

The distribution of Sir J. Herschel's nebulæ with respect to the Milky Way is shown graphically in the accompanying maps, which first appeared in

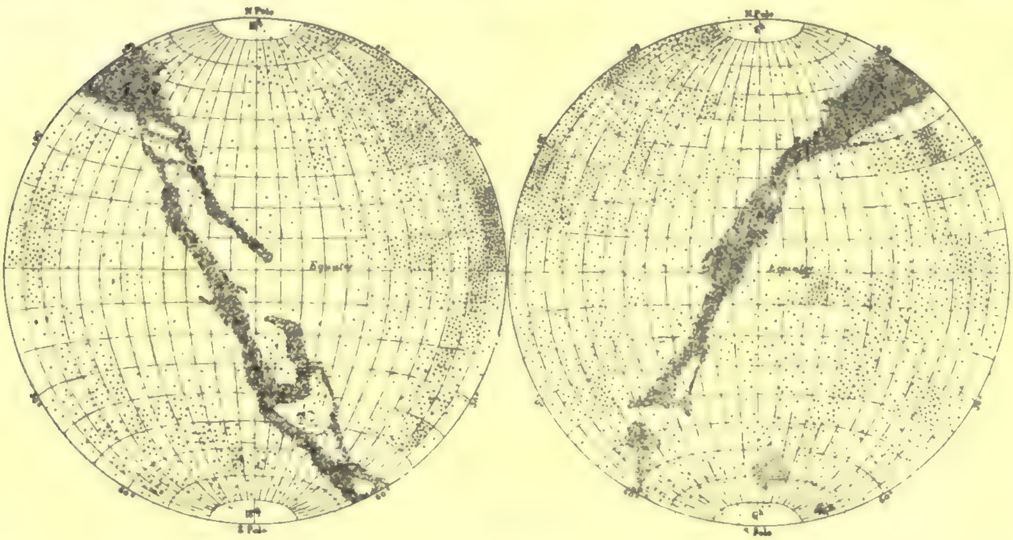


FIG. 445.—Distribution of the Nebulæ—Isographic projection showing the zone of few nebulæ, from Mr. R. A. Proctor's paper in the *Monthly Notices* for October, 1869.

a paper published by Mr. Proctor in the 'Monthly Notices' of the Royal Astr. Society for October 1869.[1] These maps very clearly show that the nebular

[1] Mr. Proctor's paper in the *Monthly Notices* for 1869 was not the first in which the *general* arrangement of the nebular system with respect to the stellar system was pointed out, but it was the first in which the connection of the two systems had been exhibited by means of suitably drawn maps. Besides the essay of Mr. Herbert Spencer referred to above, Prof. Cleveland Abbe published in the *Monthly Notices* for May 1867 an important paper on the Distribution of Nebulæ in space, in which most of the conclusions arrived at by Mr. Proctor had been pointed out by counting and tabulating the number of nebulæ in different areas of the heavens. And Sir John Herschel had himself in the *Cape Observations*, p. 134, remarked, 1. That the distribution of the nebulæ is not like that of the Milky Way, in a zone or band encircling the heavens, or if such a zone can be traced out, it is with so many interruptions, and so faintly marked out through by far the greater part of its circumference, that its existence as such can be hardly more than suspected. 2. That one third of the whole nebulous contents of the heavens are congregated in a broad irregular patch occupying about one-eighth of the whole surface of the sphere. . . . The general conclusion which may be drawn, said Sir John Herschel, is that the nebulous system is distinct from the sidereal, though involving, and perhaps to a certain extent intermixed with the latter. The great nebulous constellation in the northern hemisphere, which he called the region of Virgo, may be regarded as the main body of this system, and subtends at our point of view an angle of 80° or 90°. Supposing its form to approach to the spherical, our distance from its centre must be considerably less than its own diameter. . . . It must not be left out of consideration, and has been distinctly remarked by Sir William Herschel as an element of whatever speculation a closer attention to this subject and a more perfect classification of nebulous objects may lead us to indulge in, that the most condensed portion, and what may fairly be regarded as the principal nucleus, of the region of Virgo, is situated almost precisely in the pole of the Milky Way. Taking that great circle as a horizon, the whole of that stratum forms as it were a canopy occupying the zenith and descending to a con-

system is intimately connected with the stars of the Milky Way, and that the nebulæ and stars of the galaxy form part of a single scheme. If the nebulæ

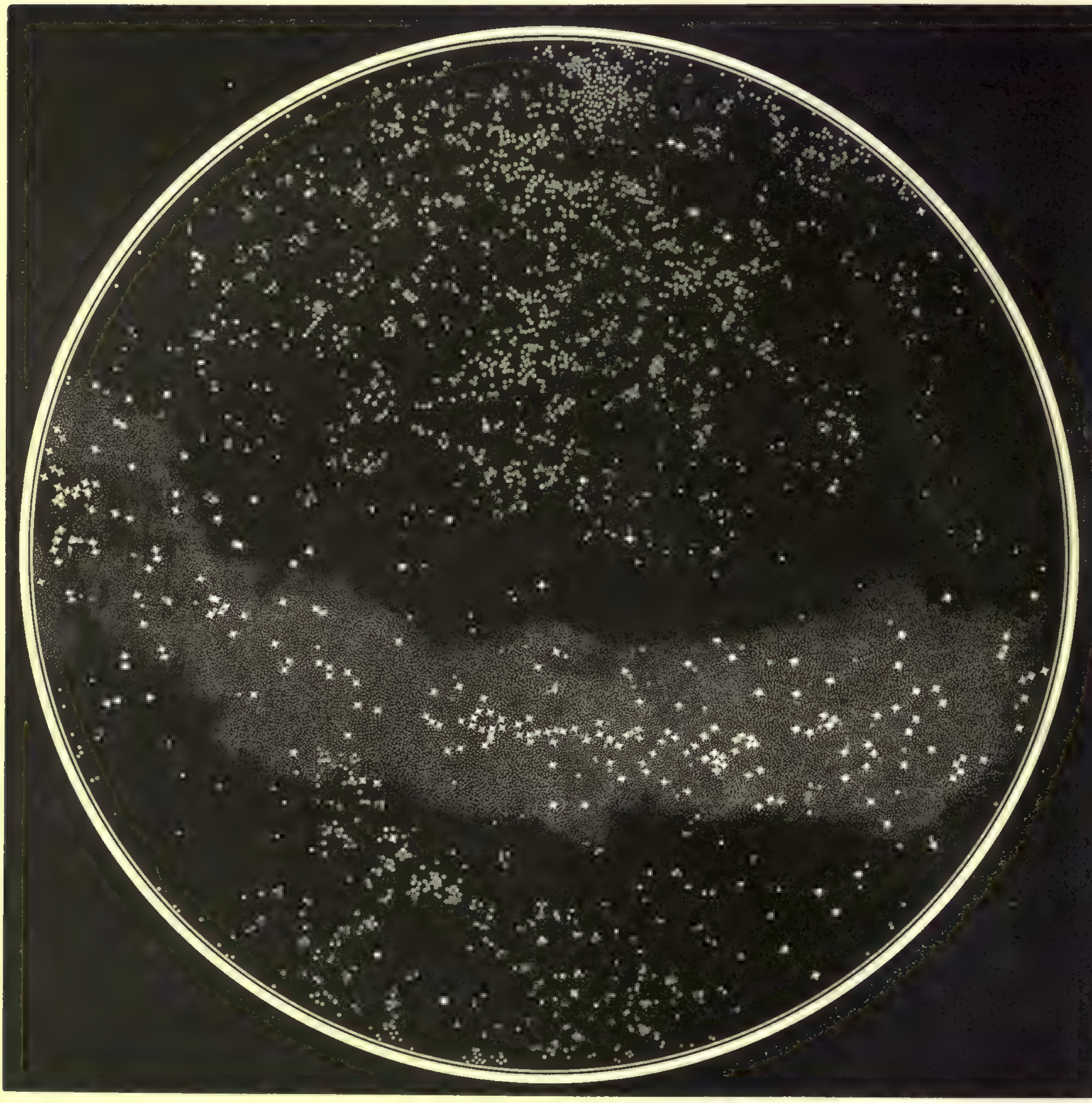


Fig. 446. - The Nebulæ and Clusters in the Northern Hemisphere, plotted on an equal surface-projection by Mr. Sidney Waters, from Sir John Herschel's catalogue. The nebulæ are represented by dots, the clusters by crosses.

siderable distance on all sides, but chiefly on that towards which the North Pole lies. The phenomena on the other side of the Milky Way, though much less characteristic, are not altogether dissimilar, the nebulous region of Pisces and Cetus standing on the whole in pretty nearly the same relation to that circle—the most condensed part of that stratum being elevated at an altitude of between 60° and 70° above its plane. . . .

were systems like our galaxy, we should expect to find no such symmetrical arrangement of the nebulæ into two groups separated by the galactic stream.

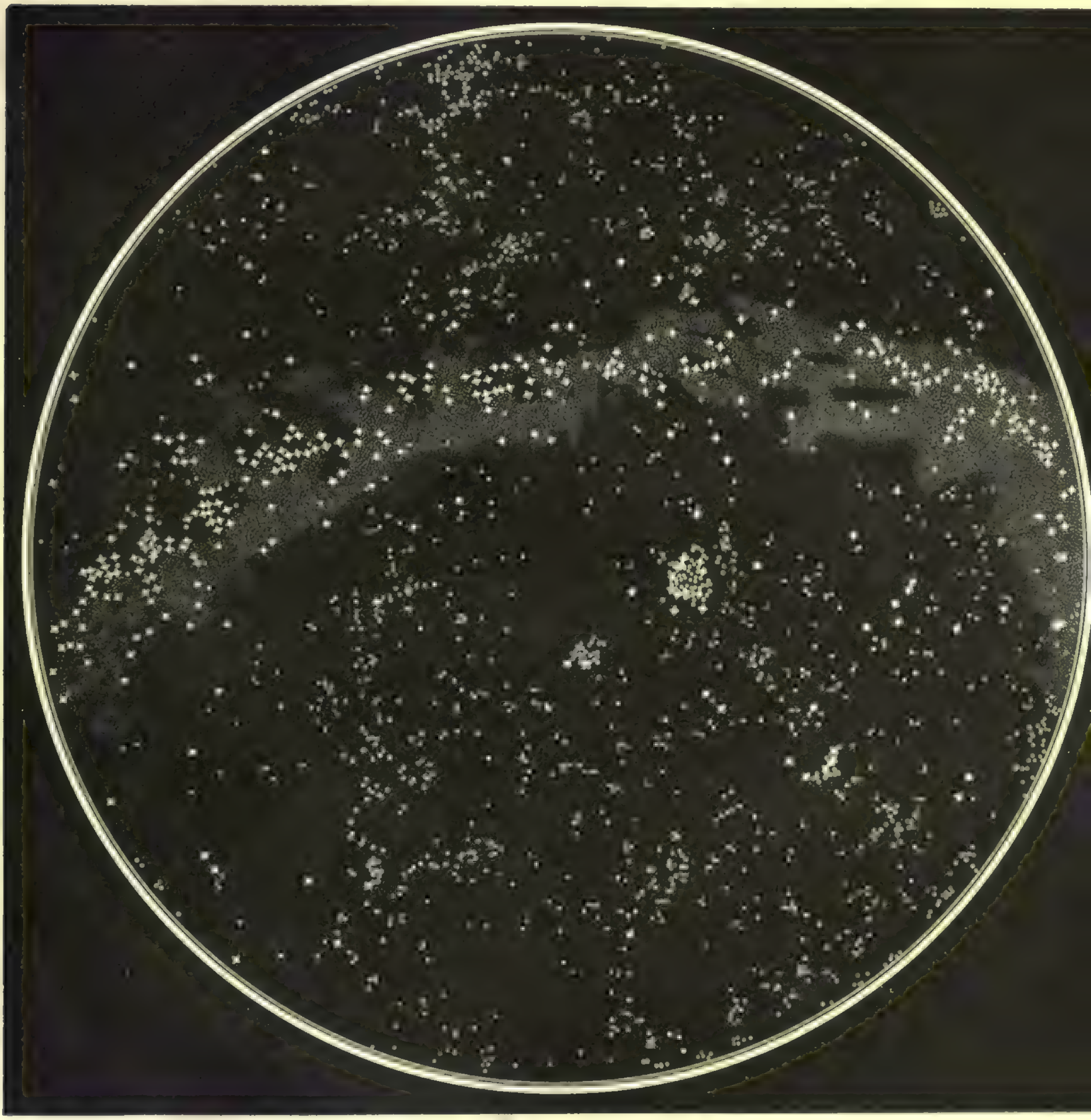


FIG. 447.—The Nebulæ and Clusters in the Southern Hemisphere, plotted by Mr. Sidney Waters. The nebulæ are represented by dots, the clusters by crosses.

There might be streams or groups of nebulæ, and these might be parallel or perpendicular to the plane of the Milky Way. If the members of the nebular system in our neighbourhood were, as Mr. Proctor remarks, arranged sym-

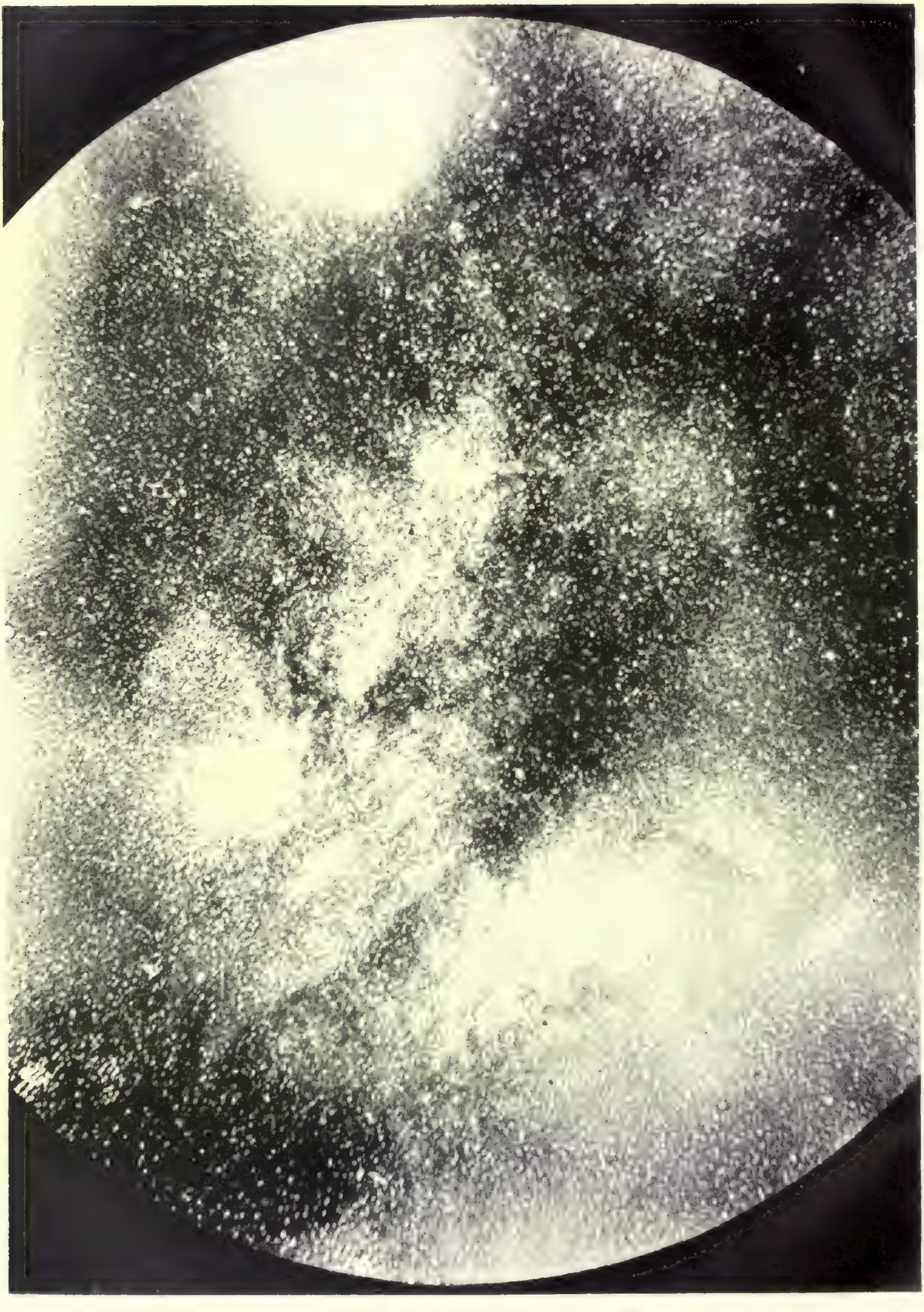
metrically with respect to our galaxy, we should expect to find the streams or groups extending all over the heavens, or at all events not interfered with or complementary to the streams of the stellar system. The pair of maps given in fig. 445 serve to bring out the great vacant zone which extends along the region of the Milky Way, as well as the stream of nebulæ which in the southern hemisphere leads up to and terminates in the Nubecula major, a striking cluster composed of nebulæ as well as stars.

A further pair of maps, prepared by Mr. Sidney Waters in conjunction with Mr. Proctor, appeared in the 'Monthly Notices' for August 1873, showing on a larger scale the distribution of star clusters and nebulæ over the heavens. They showed that streams and clusters of nebulæ which had been ranked as irresolvable by Sir John Herschel were followed by, and associated with streams and clusters of nebulæ which had been ranked as resolvable, in a manner which rendered it probable that they are associated together, forming distinct systems from, but intimately associated with the distribution of the lucid or brighter stars, while the large and irregular gaseous nebulæ, which are frequently associated with star clusters, are grouped along the Milky Way, and seem to be intimately associated with it. The aggregation of star clusters upon the Milky Way, especially along its central region, is also very striking.

(1421.) Besides the general distribution of the larger nebulæ in the neighbourhood of the Milky Way, some of them seem to show a symmetrical arrangement of their parts with respect to the Milky Way. Thus the great nebula in Orion shown in Plate XXIX. consists of a series of structures which curve together forming a synclinal group the axis of which is perpendicular to the general plane of the Milky Way. The edges of these great curving structures are all harder on the inside towards the axis of symmetry, and softer or more nebulous on their outer or convex edges, indicating the existence over an immense region of forces, acting towards or away from the axis of symmetry, that is parallel to the plane of the Milky Way.

After the nebula in Andromeda this nebula is the brightest in the Northern heavens, and since the days of Huyghens many of our best observers have spent weeks and months in trying to draw the forms involved in the wonderful mass of glowing haze which melts imperceptibly away into the surrounding sky. The difficulties of the task can only be appreciated by those who have attempted it, or by those who have endeavoured to compare and reconcile the drawings made by different observers of a corona or other hazy object. There are no sharp outlines which can be definitely measured, and the true gradations of brightness are more difficult to represent than would be credited. In comparing such drawings one has to make allowance

PLATE XXVII.



Part of the Milky Way in Sagittarius

(From a Photograph taken at the Lick Observatory by Prof. E. E. Barnard).

The white patch at the top of the picture is the over-exposed image of Jupiter. The trifid nebula is just within the border of this patch, nearest its lowest part. The centre of the plate is about R.A. 17h. 56m. $\delta = -28°$. Scale 1 inch = 1° 48′.

PLATE XXVIII.

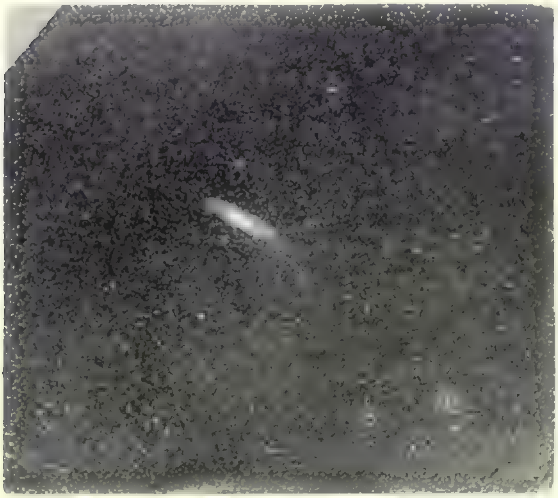
(From Photographs taken at the Lick Observatory by Prof. E. E. Barnard.)

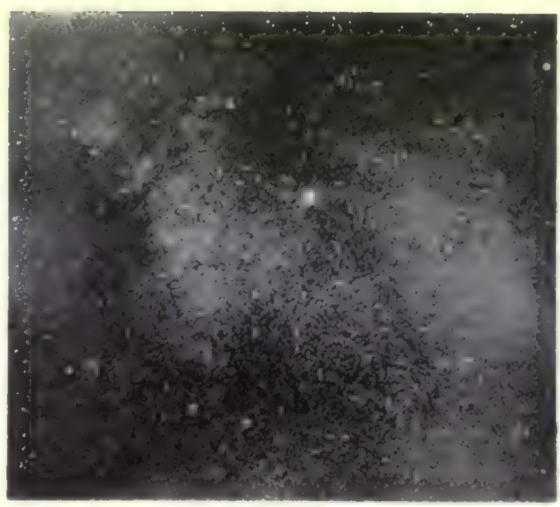
W N S E

1. Region round the Great Nebula of Andromeda
Scale 1 inch = 5° 18′

N E W S

2. Region of Milky Way south of the Trifid Nebula.
The white patch at the top is the over-exposed image of Jupiter. Scale 1 inch = 4° 36′.





W N S E

3. Milky Way in Sagittarius.
Photograph taken July 25, 1889. Region round 21 Messier 18h. 10m $\delta = -18° 28′$. Scale 1 inch = 1° 48′.

S E W N

4. Milky Way in Aquila.
Photograph taken Aug. 2, 1889. Region round 11 Messier 18h. 42m $\delta = -6° 27′$. Scale 1 inch = 2° 30′

for the way in which each observer translates what he sees. Consequently, we have no very reliable evidence as to whether change is taking place in the forms of these great structures at a rate which can be appreciated by man, though photography will now afford us much more trustworthy evidence.

Prof. Holden, the present Director of the Lick Observatory, published in 1882 an important monograph on the central parts of the Orion nebula. It was issued as Appendix I. of the Washington Observations for 1878, and contains some forty woodcuts representing drawings of this nebula by well-known observers. On examining the drawings, the casual reader will probably be more struck by their wide discrepancies than by their similarity ; but Prof. Holden, who has given a great deal of time to the drawings, thinks that there is evidence that certain changes have taken place in the form of the nebula since the date of some of the earlier drawings.

The first photograph of the Orion Nebula (and indeed of any nebula in the heavens) was obtained by Dr. Henry Draper at his observatory at Hastings on the Hudson River on the night of September 30, 1880. The photograph was taken with a refracting telescope of eleven inches aperture and with an exposure of fifty-one minutes. It shows but little of the structure shown in Plate XXIX. from Mr. Isaac Roberts's photograph. But improvements were rapidly made, and in March of the following year, Mr. Common obtained some excellent photographs of this nebula with a 37-inch silver on glass reflector, having a focal length of about 17 feet, which show most of the details shown in Mr. Roberts's photograph. We have, however, at most only a ten years' photographic record of this or any other nebula, and we have no photographic evidence as yet that any perceptible change in the forms of the nebulous structures has taken place.

The diagram (fig. 448) will serve as an index-map to the more remarkable structures traceable on the photographic enlargement given in Plate XXIX. (*a*) is a great curving structure with a sharp inner edge. It is narrower and brighter in its lower parts and seems to spring from the region of the trapezium (a quadruple star in the brightest part of the nebula). (*b*) reminds one forcibly of the tree-like type of solar prominence, or of the large tree-like structures which have been traceable in the photographs of some coronas. But when we remember that no parallax has yet been detected for the stars which are apparently involved in this nebula, the enormous size of the structure as compared with any solar prominence or coronal structure becomes apparent. As seen on the original negatives the stem or trunk of the tree is decidedly sharper on the inner than on the outer edge. And there are some curious notches or irregularities on its inner edge, as there are also

on the inner end of the structure (a). The whole structure is slightly concave towards the axis of the nebula. It is brightest in its lower parts and seems to have sprung from the region of the trapezium and spread out above. The very slow rate at which change is taking place in this colossal structure

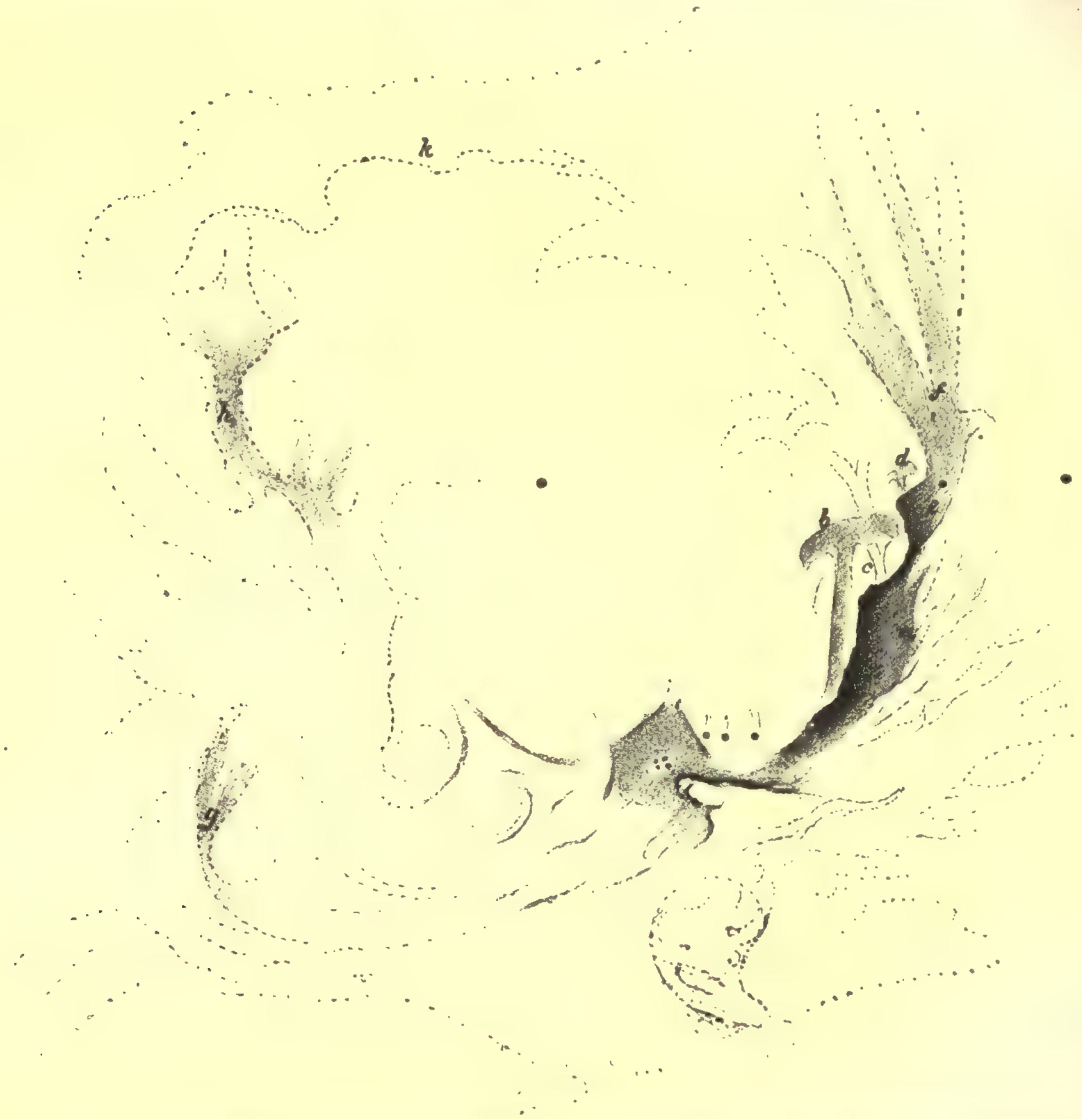


FIG. 448.—Index-map to the structures traceable in the photographs of the Orion Nebula. See Plate XXIX.

is evidenced by the similarity of its appearance as shown in a drawing made by Mr. Lassell in 1862 [1] with its form as shown in the recent photographs, proving that if there are any analogies between it and the tree-like type of gaseous solar prominence which it resembles, the change in this enormous

[1] See *Knowledge* for May 1889.

The Great Nebula in Orion.

Enlarged from an Original Negative taken 4th February, 1889, by Mr. Isaac Roberts.

With an exposure of 3½ hours in the focus of a silver on glass reflecting telescope, by Sir Howard Grubb, of 20 inches aperture and 100 inches focus.

nebulous structure must be very slow as compared with changes in solar prominences. Frequently in a quarter of an hour a solar prominence so changes its form that it would not be recognised. But more than a quarter of a century has wrought no such changes here. Besides the similarity of form, it is worthy of remark that the spectroscope shows that the luminous parts of the Orion Nebula as well as of the solar prominences are due to glowing gas.

(*c*) is a smaller tree-like structure which appears to spring from the summit of structure (*a*). It appears to divide into two branches just below where it reaches the cloud-like summit of structure (*b*), one of the branches apparently passes through the summit of structure (*b*) and again branches. Mr. Lassell seems to have seen it. The whole structure is inclined somewhat towards the axis of the nebula, as it would be if it were the summit of an incurving structure.

Structure (*e*) also springs from the summit of structure (*a*). It has a narrow stem, and a large nebulous head which is harder and notched on the inner side. (*d*) is a small tree-like structure, springing from the summit of (*e*), and inclined like the tree-form (*c*) towards the axis of the nebula.

(*f*) is a gigantic branching structure which either springs from the summit of structure (*e*) or more probably from behind it. It very distinctly curves over and is concave towards the axis of the nebula; the inner edge of each of the branches seems to be slightly harder than the outer edge. The whole structure is very similar in appearance to several branching structures traceable in the photographs of the corona of 1871, some of which are shown in fig. 449, copied from one of Mr. Wesley's drawings in the Eclipse Volume of the 'Memoirs' of the R.A.S.

(*g*) is a curious forked structure on the other side of the nebula, curving sharply inwards towards the axis. It is hardest on its inner side, as also are the structures within and above it, though the enlarged photograph given in Plate XXIX. is too dense to show anything but their inner edges. Below (*g*) are two somewhat similar structures, which, though they are not visible on the enlargement, are recognisable on Mr. Isaac Roberts's negatives, and curve inwards similarly. (*h*) is a tree-like structure, which also bends over towards the axis of the nebula.

(*k*) is a curious canopy of cloud-like loops of nebulous matter, which arches over the summit of the whole nebula, and has the appearance in the negatives of being formed of the heads of a number of tree-like structures. It is well shown in Lord Rosse's great map of this nebula published in the 'Philosophical Transactions.'

In addition to the similarity of form already pointed out between the

structures traceable in this nebula and those traceable in the solar corona, there seems to be a synclinal tendency of the great curving branches of the nebula, similar to the synclinal tendency which is traceable in so many groups of coronal structures, as, for example, in fig. 449.

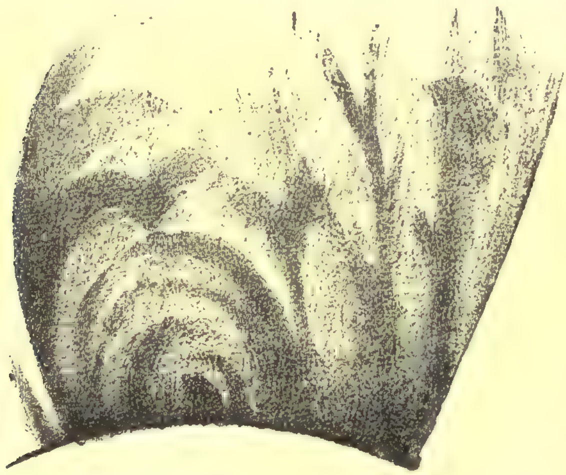


Fig. 449.—Group of Synclinal Structures traceable in the photographs of the Corona of 1871, copied from 'Memoirs of the Roy. Astron. Soc.' Vol. XLI.

One can hardly rise from an examination of these photographs without the conviction that the structures we have been examining have, like the structures in the corona, had their origin in the region where they are brightest and narrowest. Some vast explosion seems to have taken place in the region from which all these structures appear to spring, and there seems to be in the nebula, as in the corona, some drifting action which subsequently tends to curve inwards towards a central axis the great explosive jets.

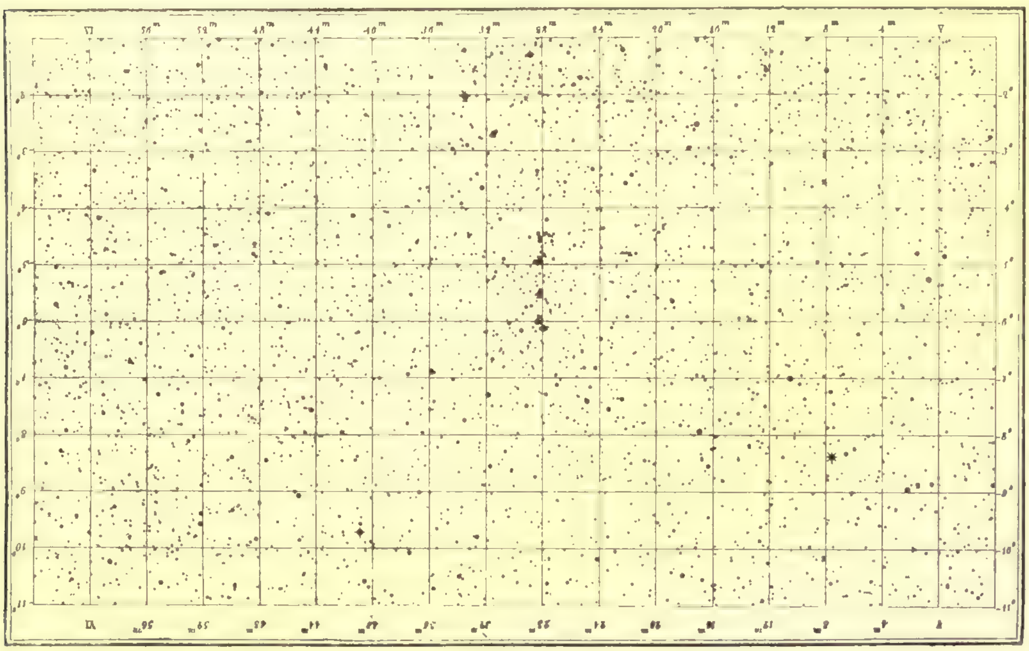


Fig. 450.—Photographic Reduction from a portion of Schönfeld's Map of the Orion region, showing long straight streams of stars running parallel to the medial line of the Milky Way. The two large stars at the top of the map belong to Orion's Belt.

The drifting action in this case is in a direction parallel to the medial line of the Milky Way, and parallel to the direction of several remarkable streams or lines of stars in the neighbourhood, as will be seen from fig. 450, which is a photographic reduction from Schönfeld's *Durchmusterung* map of this region.

The streams of small stars referred to run diagonally across the plate, from the lower left-hand corner to the upper right-hand corner, and are parallel to the line joining the three great stars of Orion's Belt. Only two of the stars of the belt are shown near the top of the map. The three stars in a vertical line near the centre of the map are the stars of Orion's Sword. The nebula surrounds the central star of the sword, which is known as *θ Orionis.* It is quadruple, and is frequently referred to as the trapezium. Prof. E. C. Pickering has succeeded in photographing several faint nebulæ in this Orion region, and amongst them a faint nebulous line of light which passes through sixteen faint stars.[1] This nebulous stream joins the *Durchmusterung* stars $-8°$ 1119 in R.A. 5h. 19m. 0s., Dec. $-8°$ 40′, with D.M. $-8°$ 1132 in R.A. 5h. 21m. 6s., Dec. $-8°$ 50′, and lies in a direction parallel with the streams of stars referred to above. Such a series of coincidences cannot be accidental, and points to the conclusion that in this region of space forces have been in action which have tended to distribute luminous matter in lines which lie parallel to the plane of the Milky Way.

The grouping of the great tree-like structures of the Orion Nebula seems to indicate that they have had their origin in a tremendous explosion or series of explosions in the neighbourhood of the trapezium which has sent forth enormous streams of gaseous matter into a resisting medium. If the stars of the trapezium are fragments of the colliding masses, they show no motion which has as yet been detected, and the larger stars of the group exhibit but slight variability, if any.

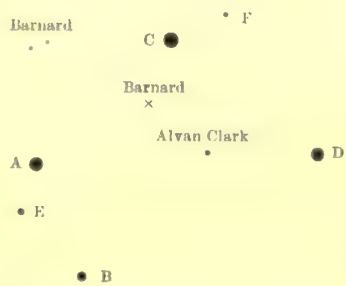


Fig. 451. Stars of the Trapezium in the Orion Nebula.

The above diagram is from a drawing by Mr. Burnham, who is of opinion that even the smaller stars are not variable. Other observers, however, differ from him as to this, though none have at present produced evidence tending to show that there is any observable relative motion amongst the members of this interesting little group, which has, perhaps, been more closely studied by the possessors of large telescopes than any other equal region in the heavens.

[1] See *Annals of Harvard College Observatory*, vol. xxvii. p. 155.

Dr. Boeddicker, in his large drawing of the Milky Way made at Parsonstown, gives a stream of nebulous light extending from the main body of the Milky Way and including the brighter stars of Orion ; a view to which probability is lent by the form and position of the numerous large nebulæ associated with stars in this constellation which have been catalogued by Dr. Dreyer[1] and Professor Pickering.[2] One of these large nebulæ streaming southward for about 60′ from the bright star ζ *Orionis* has recently been well photographed by Dr. Max Wolf of Heidelberg, though it had previously not been overlooked by Dr. Dreyer, and had been photographed by the brothers Henry[3] as well as by Prof. Pickering. Dr. Max Wolf also suspects a similar connection by a chain of nebulæ between the Pleiades and Hyades[4] groups and the Milky Way.

(1422.) There seems to be no obvious connection between the plane in which the Great Nebula in Andromeda lies (*see* Plate XXX.) and the plane of the Milky Way, and the same remark applies with regard to other spiral nebulæ as well as to the Ring Nebula in Lyra. The elliptic patches of light into which these nebulæ project have not their major and minor axes parallel and perpendicular to the medial line of the Milky Way, as would be the case if these nebulæ laid in planes parallel or perpendicular to the plane of the Milky Way. Though the Andromeda Nebula lies on the borders of the Milky Way, the spiral nebulæ do not seem to be associated with the Milky Way in the same intimate manner that the other large and irregular nebulæ are ; thus the spiral nebulæ in Ursa Major and Canes Venatici are at some distance from the Milky Way, and their planes are evidently not parallel to one another. Even the small elliptic nebula, H.V. 18, discovered by Miss Caroline Herschel close to the Great Andromeda Nebula,[5] appears to lie in a different plane to the Great Nebula. The Nubecula Major and the Nubecula Minor in the Southern hemisphere, which are now known to be spiral structures, are also at some distance from the Milky Way, and the planes in which the spiral structures they exhibit appear to lie seem to have no connection with the plane of the Milky Way. Similarly, the planes of the orbits of binary stars seem to be inclined at all angles to the plane of the Milky Way.

(1423.) The Milky Way is by no means a uniform stream of stars encircling the heavens. In some regions it is broad and bright, while in others its light is comparatively faint. Some regions, as we shall presently see, are

[1] *Memoirs of the Royal Astronomical Society*, xlix. pt. 1, p. 1.

[2] *Annals of Harvard College Observatory*, vol. xviii. p. 115.

[3] *Rapport Annuel sur l'Etat de l'Observatoire de Paris pour l'année* 1887.

[4] See a paper by Dr. Max Wolf in *Sirius* for May 1891.

[5] This nebula is shown on Plate XXX. on the north-preceding side (*i.e.* a little above and to the right hand) of the Great Nebula.

Great Nebula in Andromeda.

From an Original Negative taken 29th December, 1888, by Mr. Isaac Roberts.

With an exposure of 4 hours.

rich in red stars, while others are rich in stars exhibiting bright-line spectra. Sir John Herschel long ago drew attention to the fact that in some regions it appears to be composed of larger stars than in others. Thus, speaking of the Milky Way in the region about η Argus, shown in Plate XXXI., he says ('Cape Observations,' p. 32): 'In all this region the stars of the Milky Way are well separated, and, except within the limits of the nebula, on a perfectly dark ground, and on the average of larger magnitude than in most other regions.'

The differences in character of different regions of the Milky Way will be best realised by comparing Plates XXVII., XXVIII., and XXXI. The portions of the Milky Way shown in Plate XXVIII. are represented on a much smaller scale than the portions shown in Plates XXVII. and XXXI., but the scale of Mr. Barnard's picture of the Sagittarius region (Plate XXVII.) is about equal to the scale of Mr. Russell's pictures of the Coalsack region (Plate XXXI.) In describing the Sagittarius region Sir John Herschel says:[1] 'Here the Milky Way is composed of separated, or slightly or strongly connected, clouds of semi-nebulous light, and as the telescope moves the appearance is that of clouds passing in a *scud*, as the sailors call it. . . . The Milky Way is like sand, not strewed evenly as with a sieve, but as if thrown down by handfuls (and both hands at once), leaving dark intervals, and all consisting of stars of the 14th, 16th, 20th magnitudes[2] down to nebulosity, in a most astonishing manner. Again, at 17h. 53m., after an interval of comparative poverty, the same phenomena, and even more remarkable. I cannot say it is *nebulous*. It is all *resolved*, but the stars are inconceivably numerous and minute. There must be millions on millions, and all most unequally massed together, yet they nowhere run to nuclei, or *clusters much brighter in the middle*. This extraordinary exhibition terminates about the eighteenth hour of R.A., where the Milky Way resumes its usual appearance.'

(1424.) The Sagittarius region of the Milky Way referred to above and shown in Plate XXVII. contains stellar structures which seem to afford evidence of the projection of matter into a resisting medium. As the tree-like forms (Art. 1421) in the great Orion Nebula and the forms of the structures in the Corona bear witness to explosions on a colossal scale that have taken place below their bright bases, causing a stream of matter to be projected upwards, which stream has subsequently been divided and its branches deflected from their original course by a resisting[3] medium, so the tree-like forms

[1] *Cape Observations*, p. 388.

[2] These are Herschel's magnitudes, not the photometric magnitudes referred to above.

[3] We have direct evidence that the solar prominences are projected into a resisting medium, for they have in more than one instance been

shown in figs. 452 and 453, as well as on Plate XXVII., afford evidence of the projection of matter into a resisting medium extending through that region of the Milky Way.

If there were no resisting medium, and the only force tending to alter the direction of motion of the projected matter were gravity acting towards the region from which the explosion took place, or towards some other centre, each part of the stream would move in a trajectory. And if we assume that all the matter of the stream was projected in the same direction, and with approximately similar velocities, the whole stream would approximately have the form of a trajectory, or conic section, and would, in whatever direction it was viewed, be projected into some conic section curve. But the streams observed exhibit contrary flexure and branching, which cannot be accounted for by the superposition or projection on one another of many trajectories, which must by hypothesis be all confocal about the centre to which gravity is acting. We are, therefore, driven to assume the existence of a resisting medium in order to account for the curvature of the streams.

FIG. 452. Outline of tree-like structure in the Milky Way. See Plate XXVII.

The actual existence of the great tree-like form shown in Mr. Barnard's photograph seems to be confirmed by the arrangement of small stars in streams along and parallel to many of its branches. This is best seen in the two small photographs of the same region on the right-hand side of Plate XXVIII. The resisting medium into which the matter of the tree-like form has been projected need not necessarily be gas; dust moving in space or meteors or large masses would equally offer resistance.

In the case of the solar prominences, we know that the tree-like forms are shot upward and consist of gaseous matter; and it seems very probable that the tree-like structures in the Orion Nebula also consist of gaseous matter, for

observed to be shot upward more rapidly than a projectile would rise to a similar altitude if unimpeded by a resisting medium. This may at first sight appear contradictory, but a projectile needs to be shot upward with greater velocity to rise to a given altitude in a resisting medium than in free space. (See *Monthly Notices*, xli. p. 77.)

they are the brightest parts of the nebula, and the spectroscope shows that the chief part of the light of this nebula is emitted by incandescent gas.

Besides the streams of small stars which may be traced following some of the lines of the branches of the tree-like structure in Mr. Barnard's photograph there are many streams of larger stars evidently unconnected with the tree-like structure, for they lay athwart its branches and are in some instances connected with dark channels and dark patches. One of the most striking of these dark patches bordered by stars is shown near to the centre of fig. 452, and is well shown on Plate XXVII. It is only one of many such dark patches surrounded by stars which occur over this and other regions of the heavens, though it is more striking than most of such dark patches owing to the size of the stars by which it is surrounded and the brightness of the region in which it occurs. There is a succession of dark areas or patches, forming a curving stream, which stretches from the great white patch that corresponds to the over-exposed image of Jupiter at the top of Plate XXVII. down beside the tree-like structure to the dark region near the bottom of the plate. Mr. Barnard, in referring[1] to this curving dark lane amongst the stars, says: 'At the middle of the picture it is seen to pass behind some of the stars and emerge beyond, showing us clearly which part of the Milky Way at that point is nearest to us.' It is best shown in the small pictures of the same region on Plate XXVIII., where it is seen to be hardest and most sharply defined on the inner or concave side of the arch. There are also traces of other and fainter dark channels which run parallel with the main arch, and at least two branching dusky structures, B and C (see fig. 453), which spring from the dark region between the clusters at the bottom of Plate XXVII. The bright tree-like structure also springs from the same region, and the dark arch A leads into it. It will be noticed that some dark prominence-like structures spring from the southern or convex side of the arch which are somewhat similar to, though on a smaller scale than, the dark structure B. The contrast of these darker branching structures with the comparatively uniform bright background on which they are seen seems to indicate the existence of dark absorbing matter, either like cold gas or a fog of opaque particles in space, cutting out or dimming down the light of the region beyond.

The two dark structures B and C are evidently nearer to us than the bright stellar or nebulous structures on which they are seen projected; while the dark arch seems to be nearer than the bright structures at its southern or lower end, it is apparently further from us than the bright structure which seems to pass across and eclipse it at A′. But this is not the only evidence which tends to show that the bright and dark structures are closely

[1] *Monthly Notices*, vol. L. p. 814.

associated together in space. The way in which the dark structures A, B, and C, as well as the bright tree-like structure shown in fig. 452, all seem to spring from the dark region D, indicates that they are all intimately connected

FIG. 453.—Index-map, showing dark structures on Plate XXVII.

with this great dark region, and with the star clusters which border it, as well as with some streams of small stars which also seem to spring from the dark region. One of the most striking of these streams of small stars is easily recognised on Plate XXVII. It lies about midway between the dark structure B and the lower part of the dark arch A, and after running for some distance parallel to A, it branches in a direction away from the dark region D as if it sprang from it. It would seem, therefore, that the bright and dark structures, as well as certain streams of small stars which surround and seem to spring from the dark space D, probably have had their origin in matter projected from within the dark region into a resisting medium surrounding it.

(1425.) Fig. 454 is printed from a block which has been made by a photographic process from the central part of Mr. Barnard's negative reproduced in Plate XXVII. The photograph from which fig. 454 was made was so over-printed that all the fainter stars and the nebulous structures are lost in it, and only the larger stars remain ; but it enables us to judge of the disposition of the larger stars with regard to the nebulous structures and fainter

stars shown in Plate XXVII. It will be seen that the brighter stars are disposed in stream lines and closed curves, which apparently have no connection with the forms of the bright nebulous structures or with the streams of faint stars which in some instances appear to conform to and lie along the branches or outlines of the bright nebulous structures.

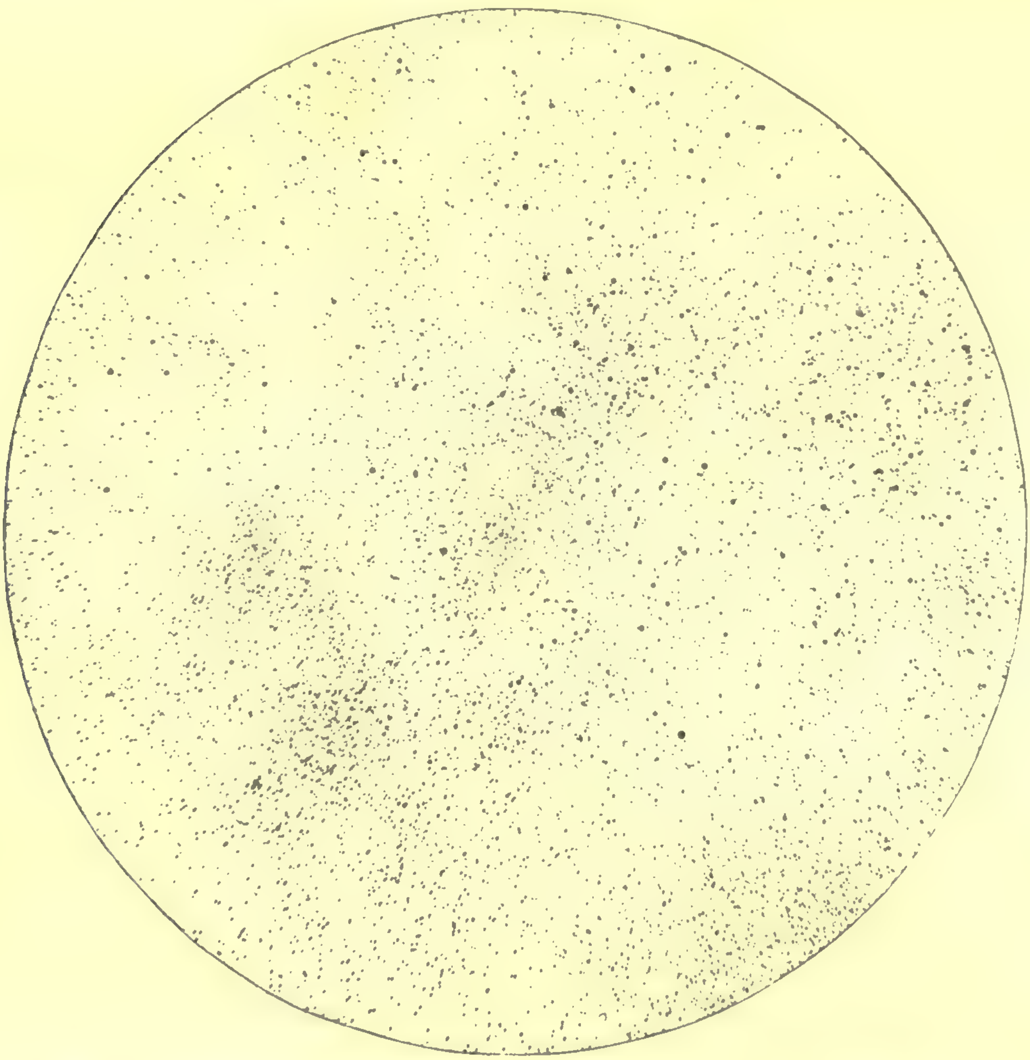


FIG. 54.—Photographically prepared block showing streams and groups of stars in Sagittarius.

The closed chains or rings of stars, of which there are very many examples in this and other regions of the heavens, appear to have an intimate connection with small dark patches which they frequently surround. It is curious that so many of these rings of stars are roughly circular or but slightly elliptical—as circular rings would appear if they laid at all angles to the line of sight. The actual arrangement of stars which would give rise to such an

appearance is difficult to conceive of. If the dark patches surrounded by stars had only been observed by the eye at the telescope, one might account for the darkness within the rings of stars as being probably caused by an optical illusion. But the photographic plate cannot be deceived by effects of contrast, and it registers the regions surrounded by many of these rings or curves of stars as distinctly darker than the surrounding area.[1] It seems certain that the dark regions and dark channels seen in the heavens are in some way intimately associated with, or are surrounded by, clusters or streams of stars in a manner which cannot be accidental. Notice, for example, the dark region at the bottom of Plate XXVII. lying between the two clusters, the stars which lie along the dark structure B and those which border the dark regions referred to in describing figs. 456, 457, 458, and 459, as well as the bright stars which lie along the border of the dark channels near *η Argus*, Plate XXXI.

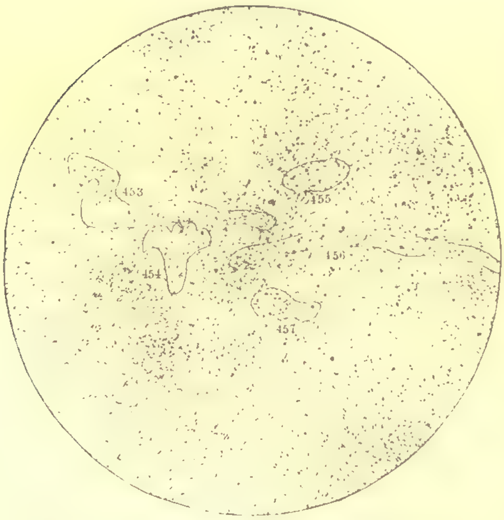


Fig. 455.—Index-map of Sagittarius region.

Fig. 455 is an index-map which will enable the reader more readily to find the small groups of stars shown on fig. 454, which it seems desirable to refer to at some length.

Fig. 456

Fig. 456 shows a double stream of stars associated with a dark channel, which will be recognised a little above the centre of Plate XXVII. Considered from the probability point of view, the evidence is overpoweringly in favour of a physical connection between the stars which fall into such lines and curves as those shown in fig. 456. Many of such streams contain twenty or thirty or more stars of about the same

[1] If dark nebulæ were usually surrounded by a stratum or region of bright stars, we should expect to see the stars more thickly distributed in a zone surrounding such nebulæ where the line of sight passes through the greatest thickness of the star stratum as seen in perspective; but we should also expect to see a certain number of stars projected on the dark region, even supposing the central nebula to be sufficiently opaque to obliterate the light of the stars on the further half of the star-bearing stratum. The light of the central stars on the nearer side of the nebula would be visible to us unless we suppose them only to be bright on the side towards the dark nebula. But this explanation involves the assumption of phenomena so different from those with which we are familiar, that one hesitates before advancing such a theory.

magnitude, following one another in a straight or curving line at approximately equal intervals. If we assume that the chance is one to four[1] that a point thrown down at random should appear to fall into line with two points already in position, the chance against ten points falling into line with two already in position so as to form a smooth curve would be more than a million to one, and the chance against twenty such points falling into line would be more than a million of millions to one. We might, therefore, without any further evidence take it as established that there must be an intimate connection between the stars of such a stream, and that they must have had a common origin and form a system. But in the Pleiades cluster we have ocular evidence of a physical connection between the stars forming two such streams.[2] The individual stars are connected together by a narrow nebulous band 'which threads them together.' Professor Pickering has also succeeded in photographing a faint nebulous line of light in the region of Orion which passes through sixteen faint stars.[3]

The stars in fig. 456 seem to belong to two long streams separated by a dark channel through a great part of their length. But they curve and join together on the left hand, uniting and being continued as a stream of rather larger stars, which is interrupted by one of the small rings of stars referred to above. The stream does not appear to pass through the small ring of stars, but it ceases and appears again on the other side, bisecting the little circle of stars so centrally that one can hardly doubt that the ring has a physical connection with the stream and is not projected upon it. The stream then branches into a triple stream or tassel, each branch being terminated by a large star, in a manner which reminds one of the configuration of the stars in the constellation Scorpio.

Fig. 457

Fig. 457 corresponds to an area just below that comprised in fig. 456; it shows some streams of stars that border a branching dark channel which is clearly recognisable on Plate XXVII., and appears darker than the surrounding nebulous area. It seems to spring from a dark area or series of dark patches which lie in front of the bright tree-like structure (the outline of which is

[1] This is much understating the improbability. Leaving out of consideration the chance that a series of adjacent stars should all be of nearly the same magnitude and at about equal spaces from one another, the chance that the next adjacent star should fall into line with a series already in a straight or curving row must be less than $\frac{1}{12}$th, for the eye would certainly detect irregularities or deviations from the general trend of the curve amounting to 30°.

[2] These streams are well shown in a photograph by the Brothers Henry (see *Rapport Annuel de l'Observatoire de Paris*, 1887), and also in Mr. Isaac Roberts's photographs (see *Knowledge*, January 1889).

[3] See *Annals of Harvard College Observatory*, vol. xxvii. p. 155.

shown in fig. 452), and to be connected by a series of dark patches or a dark channel with the dark structure C shown in fig. 453.

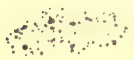


Fig. 458

Fig. 458 shows a string of small stars bordering an elongated dark area which will be recognised on Plate XXVII. as near to the summit of the bright tree-like structure on its right hand or following side.[1] The region within the ring of stars is distinctly darker than the surrounding area.

Fig. 459 shows the stars surrounding the striking dark patch or area referred to on p. 739. It also includes two streams of small stars which border a very narrow dark channel that leads away from the lower right hand (*i.e.* south following) side of the large dark patch. The dark channel referred to is very narrow, but it will be recognised at once on Plate XXVII., where, when examined with a lens, it is seen to break up into a series of intensely black dots, forming a chain or curving dark line leading from the lower part of the dark patch to a point just below the bright star on the following side. There is a similar narrow black channel between the double line of stars to the north of the black patch.

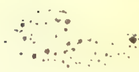


Fig. 459

Fig. 460 shows a long chain of small stars which also seems to be associated throughout a great part of its length with a narrow dark channel or chain of very small dark patches. This string of stars will be found on fig. 454 stretching away nearly radially from the centre of the circular area to the right or following edge, and by the help of the adjacent large stars will be recognised on Plate XXVII. Several similar strings of small stars associated with minute dark patches will also be found upon the plate. I have only referred to and attempted to aid the reader in tracing for himself the more conspicuous bright and dark structures shown on this plate.

Fig. 460

It is evident that in this region of the Milky Way we see projected on one another a complicated series of bright and dark structures and tree-like forms. The dark structures are of various degrees of blackness, and they appear to be more intimately associated with lines or streams of stars than the bright structures. The great bright tree-like form (the outline of which is given in fig. 452) has the appearance of being nebulous. If it is composed of minute stars, they are too small and too close to one another to be distinguished on the scale of Plate XXVII. There are, however, parts

[1] All these figures, as well as Plate XXVII., are reversed right and left.

of this structure which seem to be associated with streams of very minute stars.

(1426.) There are other regions of the Milky Way which also have the appearance of shining with nebulous light, notably the area round the η *Argus* nebula and the area near ϵ *Cygni*, in which two striking curved streams of nebulous light associated with stars are to be seen ;[1] these nebulous streams are well shown in a photograph by Dr. Max Wolf, of Heidelberg. His photographs also show many dark areas bordered by stars, as well as narrow dark channels with lines of stars on either side. The various degrees of blackness of these dark areas may possibly be accounted for by nebulosity in front of them, some of them being seen through a greater thickness of faint nebulosity than others ; or the dark regions themselves may be faintly luminous but relatively dark compared with the luminosity of the surrounding region.

(1427.) The stars throughout the whole of the *Cygnus* region are evidently associated with nebulosity. Dr. Max Wolf states that α and γ *Cygni* appear on his negatives to be connected by nebulous films, and that a nebulosity may be traced in brighter or fainter patches throughout the whole region. Probably the whole of the Milky Way is faintly nebulous, the stars being connected by a nebulous envelope, somewhat after the manner of the stars in the Pleiades group. There seems to be evidence that the stars of the Hyades group are similarly surrounded by a faint nebulosity, and that both the Pleiades and Hyades groups are connected with the Milky Way by a faint nebulous stream or a succession of nebulous areas in a manner similar to that already referred to on page 736 with regard to the principal stars in the constellation of Orion.

(1428.) Such a nebulous connection of large stars with the Milky Way indicates that the large stars so connected must be at about the same distance from us as the stream of small stars which give the bulk of the light of the Milky Way. The association of large stars with the Milky Way is also proved by the way in which many large stars fall into line with curves of small stars. One or two such coincidences might be accounted for as accidental, but the very frequent alignment of small and large stars shown by the photographs may with safety be taken as proving a close association between such large and small stars. As early as 1767 the Rev. John Michell showed, by a simple application of the doctrine of probabilities, that the Pleiades group cannot reasonably be assumed to be an accidental grouping by projection of

[1] These are very striking objects, best seen in the Milky Way on a dark night with a large telescope and a low power. Mr. Denning refers to the remarkable patches of nebulosity in this region of the Milky Way in his *Telescopic Work for Starlight Evenings*, p. 339.

large and small stars on a small area of the heavens, but that it must correspond to an actual clustering together of stars in the same region of space.

Prof. Pickering's observations, quoted at p. 722, show that there is in this cluster a range of at least twelve magnitudes of stars visible in the telescope, and the photographs probably include a range of fifteen magnitudes ;[1] which means that the brightest stars of the cluster give a million times as much light as the faintest stars which have just impressed themselves on the photographic plate. But Plate XXXI. affords evidence of a still greater difference of light-giving capacity : *α Crucis*, the lower of the large stars in the picture of the Coalsack region, is seen to be centrally situated with respect to several diverging streams of small stars ; a circle of small stars may also be traced round the large star within the sharply-defined bright ring, which is a photographic defect due to the reflection of the light of the bright star from the back of the photographic plate.[2] The symmetry of the arrangement of the diverging streams of small stars with respect to the large star hardly permits us to doubt that the streams of small stars are associated with and are at the same distance from us as the large star from which they appear to radiate ; but the probability of such a connection is greatly increased by a closer examination of the large star, which is seen to be surrounded by a dense cluster of small stars similar in magnitude to those forming the diverging rays. The spurious disc of the large star is, when examined with a magnifier, seen to contain several small stars ; some seven or eight of them may be recognised within the edge of the spurious disc of *α Crucis*, as shown in Plate XXXI. The large star is ranked by Gould as of the 1·3 magnitude, and the smaller stars of the radiating stream which extends over the Coalsack region are probably not above the eighteenth magnitude of the photometric scale. According to this estimate, the large star would radiate about three or four million times the light radiated by one of the smaller stars. As observed in the telescope

[1] Over an area about *Alcyone*, measuring 120′ by 90′, M. Wolf, of the Paris Observatory, catalogued 625 stars which he estimated as above the fourteenth magnitude. Prof. Pickering gives the number of stars above the sixteenth photometric magnitude in the same area as 1,254; see *Harvard Annals*, xxviii. p. 200. In 1885 the Brothers Henry photographed 1,421 stars in this same region, and in 1887 they brought the number up to 2,326 with an exposure of four hours. Some seven or eight stars in this area are proved by their proper motions not to belong to the group.

[2] During the exposure of the photographic plate a point on the sensitive film was lit up by the image of the star, and shone like a little lamp. The rays from this illuminated region which fell nearly perpendicularly on the back of the plate emerged, whereas those which fell more obliquely were in greater and greater proportion reflected back again, and produced a photographic effect on the sensitive film. The inner edge of the bright ring corresponds to the position where the light was reflected at the critical angle ; beyond this position the whole of the light is reflected back again to the sensitive film, and none gets out at the back of the plate. I have shown by experiment that the diameter of the ring varies with the thickness of the photographic plate and with the refractive index of the glass of which it is composed. The question is more fully discussed in the *Memoirs of the Royal Astronomical Society*, vol. xlvi. pp. 231–3, where I have given a curve representing the intensity of the reflected light at different distances from a star's image.

PLATE XXXI.

The η Argus Region of the Milky Way.

From a Photograph taken by Mr. H. C. Russell, of Sydney.

α and β Crucis and the Coal-sack Region of the Milky Way.

From a Photograph by Mr. H. C. Russell, of Sydney.

α Crucis is seen as a sharply-defined double star with components about 5″ apart, which move about one another very slowly, for their position seems hardly to have changed since Sir John Herschel observed them in 1834.

(1429.) Though the mind may at first be staggered by the conception of stars giving a million times as much light as our sun, we are forced either to admit the possibility of such vast sun-like bodies, or to believe that the smaller stars associated with them are relatively minute compared with our sun. Those who accept the nebular hypothesis as giving the most probable explanation of the formation of the planets of the solar system must be prepared to believe that there was a time when the sun had a diameter as large, or nearly as large, as the diameter of the orbit of Neptune; and if before these more than geologic ages of radiation into space the surface of the solar mass shone as brightly as the photosphere now shines,[1] it would have given about nine million times as much light as the sun now gives, and seen from a distance equal to the distance of *α Centauri* it would have appeared about 45″ in diameter;[2] if it had been removed to a distance equal to 100 times the distance of *α Centauri* it would still have presented a diameter of nearly half a second, and would have been easily recognised in a large telescope as differing from an ordinary star. But no star has at present been observed which shows such a measurable disc. We may therefore feel some confidence that there is no such vast sun-like body with a photosphere shining as brightly as the photosphere of our sun within a distance from us equal to a hundred times the distance of *α Centauri*.

If we knew the actual size and brightness of the stars of the Milky Way as compared with our own sun or with *α Centauri* we should have a means of estimating the distance of the Milky Way. Thus, if we knew that a first magnitude star involved in the Milky Way gave a hundred times as much light as our sun, we should know that the star and the region of the Milky Way with which it was associated must be situated at less than ten times the distance of *α Centauri*, for our sun would certainly appear less than a star of the 1st magnitude, and more probably as a star a little above the 2nd magnitude, if seen from a distance equal to the distance of *α Centauri*. A star with a diameter as great as the diameter of the orbit of Saturn, and a photosphere as bright as the photosphere of our sun, would give about four million times as much light as our sun. Such a star would appear as a star of about the 1st magnitude if it were seen from a distance equal to fifteen

[1] Mr. Homer Lane, of Washington, has shown that a gaseous sphere losing temperature by radiation and contracting under its own gravity will actually grow hotter until it ceases to be a 'perfect gas,' obeying Boyle's law of pressure and density.

[2] There are many planetary nebulæ which present such discs, but they are evidently much dimmer bodies than our sun. A planetary nebula 45″ in diameter with a surface as bright as the surface of the sun's photosphere would give us about 400 times as much light as is given by the full moon.

hundred times the distance of *α Centauri*, and there were no absorption or loss of light in passing through space. The fact that no such vast sun-like bodies have been recognised within a distance from us equal to a hundred times the distance of *α Centauri* gives some ground for concluding[1] that such vast stellar bodies are not likely to be found at still greater distances; and the considerations referred to on p. 725, which tend to show that there is considerable absorption of light in passing through great distances of space, would lead us to conclude that the Milky Way must be much nearer to us than one thousand five hundred times the distance of *α Centauri*, or the smallest stars we see in it must be vastly larger than our sun.

It might be argued that the brighter stars of the Milky Way need not necessarily be large stars, and that it is possible to conceive of their photospheres as many hundred times brighter than the photosphere of our sun; but this seems hardly probable in view of many facts tending to show the similarity of the matter in different parts of the universe, and the fact that our sun's photosphere about corresponds in brightness with the highest incandescence obtainable in the laboratory. The photosphere of the sun, from which we derive its light, seems to consist of a cloud stratum of incandescent liquid or solid particles which, if similar to the matter we can experiment upon in terrestrial laboratories, would be driven into vapour and cease to give out a continuous spectrum if raised to a temperature many times exceeding the temperature at which the carbon of an electric arc-light is driven into vapour. Consequently, we should not expect to find the photosphere of any star a hundred times more brilliant than the photosphere of our sun, unless the star was composed of elements altogether different from the elements we can experiment upon in terrestrial laboratories; the spectroscope seems to show that the colour of the red stars is not due to red incandescence, but to absorption taking place above the level of the photosphere. It is easy to conceive of stars relatively very faint compared with the sun, owing to great absorption of light taking place above their photospheres, or to the fainter incandescence of their photospheres compared with that of the sun, or even to the dull red incandescence of a visible solid nucleus; one can also conceive of all stages of brightness of gaseous incandescence such as we recognise in many nebulæ; but we know that the brighter stars appear to have continuous spectra interrupted by dark absorption lines more or less similar to those we are familiar with in the solar spectrum.

That the incandescence of the sun's photosphere does not differ greatly

[1] A sphere with radius equal to a hundred times the distance of *a Centauri* would contain more than four million stars distributed uniformly at distances from one another equal to the distance of *a Centauri* from the sun.

from the highest incandescence which can be produced in terrestrial laboratories is proved by comparisons which have been made of the light from the sun's surface with the glowing carbons of an electric arc light. According to Prof. C. A. Young, the positive carbon (which is always more brilliant than the negative) attains a radiance fully half as great as that of the solar surface. The incandescent portion of the carbon is in contact with colder portions which rapidly conduct the heat away, and we should expect it to be a little less brilliant[1] than if it were surrounded on all sides by similarly incandescent material. No doubt there is some absorption of the sun's light in the region above the photosphere, but the percentage of loss near to the apparent centre of the sun's disc is probably not very great, or the absorption at the limb would be more pronounced. According to Prof. E. C. Pickering, only about 63 per cent. of the light from the limb is lost by absorption.

If we assume a distance fifteen times as great as the distance of *α Centauri* for a part of the Milky Way in which a 1st magnitude star is found to be associated with stars of the $17\frac{1}{2}$ magnitude, we must be prepared to assume a diameter for the large star twenty times as great as the solar diameter unless its photosphere is brighter than the solar photosphere; while the smaller stars, if their photospheres were as brilliant as the solar photosphere, would have diameters equal to about one-hundredth part of the solar diameter—that is, they would not much exceed the earth in magnitude.

(1430.) That the region of the Milky Way is richer in large stars than other parts of space around us is proved by the facts referred to on p. 696. The zone of the heavens richest in stars which appear large to us is, however, not coincident with the galactic circle, but is inclined to it at an angle of about 20° (see p. 713). Dr. Gould is disposed to account for the appearance of this belt of large stars as being due to a flattened cluster of stars around the sun, the medial plane of which is inclined at an angle of about 20° to the plane of the Milky Way. The outer stars of the cluster would thus be seen projected on the heavens in the neighbourhood of the more distant Milky Way.

If we assume that these stars form an isolated cluster within the Milky

[1] The brilliant incandescence of an arc light is many thousand times greater than that of melted iron, and about a hundred times greater than a mass of lime in an oxyhydrogen flame. Thus Foucault and Fizeau, in 1844, found the solar surface to be one hundred and forty-six times more brilliant than the calcium light. And Prof. Langley, in 1878, found the surface of the sun to be five thousand three hundred times brighter than the molten metal in a Bessemer 'converter.' He states that the brilliance of the molten metal is so great that a stream of melted iron, which at one stage of the proceedings is poured in, to mix with the metal already in the crucible, 'is deep brown by comparison, presenting a contrast like that of dark coffee poured into a white cup.' There is thus a great range in the light-giving capacity of incandescent bodies, but the brilliance of the solar surface does not greatly exceed the brightest incandescence obtainable in the laboratory.

Way, as supposed by Dr. Gould, we are forced to admit a striking similarity between the structure of the cluster and the form of the Milky Way, which extends to such details that it appears unlikely to be the result of chance, and seems to indicate an intimate connection between the zone of stars and adjacent parts of the galactic stream. Thus, the main stream of stars extends round the heavens nearly in a great circle, and like the Milky Way it is bifid [1] during nearly half its course. The Milky Way is brighter in the Southern hemisphere than in the Northern, and the stars of the zone are brighter and more thickly strewn along the part of the zone in the Southern hemisphere than they are in the Northern. The zone of stars is, like the Milky Way, rich in large and irregular nebulæ as well as in star clusters. If the zone of bright stars corresponds to a stream of stars in space there can be little doubt that it lies at about the same distance from us as the Milky Way, for some of the groups of stars in the zone appear to be connected with the Milky Way by nebulous wisps or streams of smaller nebulæ. Thus the stars in the brilliant constellation of Orion, as well as the stars in the Pleiades and the Hyades [2] groups, are intimately associated with nebulous structures which appear to be connected with outlying streams of nebulous matter proceeding from the main body of the Milky Way. And the stars α and γ *Cygni*, which lie upon the Milky Way, are surrounded with nebulous matter, which appears to be also associated with groups of small stars in the Milky Way.

The reasoning which induced Dr. Gould to believe in the existence of a cluster of stars about the sun assumes that the apparent magnitude of a star may be taken as a criterion of its distance, but the facts we have become aware of with regard to the great range in the actual magnitudes of stars vitiates the class of reasoning employed by Dr. Gould. He found that an enumeration of the stars in photometric order disclosed a systematic excess of objects brighter than the $4\frac{1}{2}$ magnitude (see fig. 441), and he found that when a deduction was made of some 400 stars ranging from the 1st to the $4\frac{1}{2}$ magnitude, that the remaining numbers of stars down to the 9th magnitude form a tolerably regular series, increasing in nearly the theoretical space-

[1] See Dr. Gould's account of the cluster in the *Uranometria Argentina*, p. 348; see also *The American Journal of Science*, vol. viii. p. 333. Dr. Gould says: 'Few celestial phenomena are more palpable than the existence of a stream or belt of bright stars, traceable with tolerable distinctness through the entire circuit of the heavens, and forming a great circle as well defined as that of the galaxy itself, which it crosses at an angle of about 20° in Crux and Cassiopeia. Traversing in the Southern hemisphere Orion, Canis Major, Argo, the Centaur, Lupus, and Scorpio, it pursues its way in the Northern through Taurus, Perseus, Cassiopeia, Cepheus, Cygnus, and Lyra, its line being less obviously continued by the stars of Hercules and Ophiuchus. Like the Milky Way, it seems to bifurcate near *a Centauri*, the branch then thrown off reuniting with the parent stem in Andromeda.'

[2] See Dr. Max Wolf's observations, referred to on p. 736. See also Miss Clerke's *System of the Stars*, p. 387. Miss Clerke says: 'Visually, Orion and the Pleiades belong to the star belt, but they are associated with the Milky Way as well.'

ratio down to the 9th magnitude.[1] Dr. Gould consequently assumed that there is a cluster of some 400 or 500 stars about the sun, with a nearly uniform distribution of stars outside the cluster extending to a distance further than the average distance of a star of the 9th magnitude. Dr. Gould points out ('Uran. Arg.' p. 361) that the brighter stars are more uniformly distributed with regard to the star-zone than with respect to the medial zone of the Milky Way. There seems, therefore, to be little doubt that there is an actual grouping in space of the stars which appear to us as brighter than the $4\frac{1}{2}$ magnitude; but the appearance observed may be accounted for by a ring of actually large stars at a considerable distance from us, as well as by a symmetrically shaped cluster of stars which only appear large by reason of their proximity.

The geometrical law of increase noticed by Prof. Argelander, as well as by Dr. Gould, and assumed by them to indicate a uniform distribution of stars beyond the cluster, only applies to the number of stars included in the 6th to the 9th magnitudes in the Northern heavens, and may be a chance coincidence, depending on the number of actually small and large stars. A reference to fig. 442 will show that the space ratio is not accurately followed, and that the geometric law with the common ratio 3·9129 suggested by Dr. Gould only approximately represents the observed numbers. We have seen that the geometrical law altogether fails beyond the 9th magnitude for the small area about the Pole, and, though the enumeration of stars of different magnitudes in the Southern hemisphere has not been carried below the 7th magnitude, a different coefficient is needed for the Northern and Southern hemispheres to indicate the number of stars in a given space, on the theory of uniform distribution (see fig. 441).

(1431.) It has long been known that many stars are slowly shifting their places in the heavens. The fact was first pointed out by the accurate and ingenious Edmund Halley, who in 1718 communicated a paper to the Royal Society entitled 'Considerations on the Change of the Latitudes of some of the principal Fixed Stars.' He had been occupied upon an investigation as to the precession of the equinoxes, and had been comparing the declinations of the stars as given in Ptolemy's 'Almagest' with recent observations, and had found that the places of some of the larger stars seemed 'to contradict the supposed greater obliquity of the ecliptic, which seemed confirmed by the latitudes of most of the rest.' He satisfied himself that the places given in the 'Almagest' were confirmed by other early observers and could not be due to errors of

[1] The actual numbers deduced by Dr. Gould when the numbers of the 1st, 2nd, 3rd, 4th, and 5th magnitude stars have been reduced by taking away the stars which Dr. Gould assumes to belong to the stellar cluster, form a series nearly in geometrical progression with the common ratio, 3·9129 for the Northern heavens, and for the whole heavens down to the 7th magnitude, 3·912, the actual space-ratio being 3·981 (see *Uranometria Argentina*, p. 366).

transcription, and he continues : ‘ What shall we say then ? It is scarce credible that the ancients could be deceived in so plain a matter, three observers confirming each other. Again, these stars, being the most conspicuous in heaven, are in all probability the nearest to the earth, and if they have any particular motion of their own it is most likely to be perceived in them.’

The suggestion of Halley was followed up by Jacques Cassini, at the Paris Observatory, who satisfied himself that several stars had moved from the places assigned to them in ancient catalogues,[1] while other stars of equal magnitude showed no evidence of having shifted. This investigation was continued by Bradley, Maskelyne, and La Lande, Sir William Herschel, Bessel, and Argelander, until evidence has now been collected showing that between two or three thousand stars exhibit a sensible proper motion.

[1] The following list of stars having proper motions of one second and upwards per annum has been taken from a paper published by M. J. Bossert in the *Bulletin Astronomique* for March 1890. M. Bossert's paper contains a list of about three hundred stars having proper motions of more than half a second per annum :—

Name of star	Magnitude	Proper motions	Direction	Authority
1830 Groomb	6	7″·05	145°	Argelander
9352 Lac. . .	7	6·97	79·2	A. N. 2377
32416 Cordoba	8·9	6·08	112·5	A. N. 2661
61^1 Cygni . .	5	5·20	51·5	Auwers
61^2 Cygni . .	6	5·14	53·9	Auwers
21185 Lal. .	7·8	4·75	186·6	Argelander
ϵ Indi . .	5	4·60	122·2	Stone
21258 Lal. .	8·9	4·10	282·4	Argelander
o^2 Eridani .	4·5	4·05	211·9	Auwers
14318 A.O. .	9	3·76	194·2	A. N. 2377
14320 A.O. .	9	3·76	194·2	A. N. 2377
μ Cassiopeiæ .	5	3·73	115·1	Auwers
α_1 Centauri .	3	3·62	282·9	Stone
α_2 Centauri .	1	3·62	282·9	Stone
11677 A.O. .	9	3·05	274·0	A. N. 2192
1060 Lac. . .	4·5	2·99	75·5	Stone
248 Lal. . .	7·8	2·83	81·9	Paris
25372 . . .	8·9	2·33	128·1	Argelander
Arcturus . .	1	2·28	209·7	Auwers
β Hydri . .	3	2·24	82·0	Stone
7443 Lal. . .	9·10	2·24	126·0	Argelander
123 Piazzi 11h	6	2·19	53·3	Paris
ζ Toucani . .	5	2·08	56·8	Stone
3077 Bradley	—	2·08	82·0	Auwers
15290 Lal. .	8·0	1·97	156·6	Argelander
τ Ceti . . .	3·4	1·96	296·0	Auwers
212 Piazzi 14h	6	1·93	150·5	Paris
σ Draconis .	5	1·84	163·9	Auwers
18115 Lal. .	8	1·69	247·5	Argelander
δ Pavonis . .	3·4	1·69	136·7	Stone
8362 Lac. . .	6	1·65	166·3	Stone
30694 Lal. . .	7	1·61	206·1	Schjellerup
31055 Lal. . .	8	1·59	221·2	Schjellerup
2959 Lac. . .	6	1·52	349·8	Stone
61 Virginis .	5	1·52	225·8	Auwers
ζ_1 Reticuli . .	5·6	1·47	63·8	Stone
3386 Lac. . .	6	1·47	298·5	Stone
ζ_2 Reticuli . .	5·6	1·44	63·1	Stone
1643 Fedorenko	6	1·43	249·4	Argelander
30044 Lal. . .	7	1·43	197·8	Argelander
ν Indi . . .	5·6	1·43	118·9	Stone
8381 Lac. . .	6	1″·42	57°·7	A.N. 398
147 Lac. . .	6	1·41	92·4	Paris
1384 Fedorenko	7	1·40	254·4	Argelander
6888 Lal. . .	9	1·38	153·8	Argelander
38383 Lal. . .	8	1·38	227·1	A.N. 2940
46650 Lal. . .	9	1·36	134·4	Astr. Journ. 200
38692 Lal. . .	9	1·33	101·3	Argelander
189 Piazzi 0h .	6	1·32	153·4	Paris
Sirius . . .	−1·4	1·32	204·2	Auwers
γ Serpenti . .	3	1·32	167·7	Auwers
20452 A.O. .	8·9	1·32	155·0	Cincinnati
27714 Lal. . .	6·7	1·31	247·4	Argelander
322 Weisse .	8	1·31	207·2	Astr. Journ. 200
ι Persei . . .	4	1·30	90·0	Paris
85 Pegasi . .	6	1·29	139·4	Auwers
17415 A.O. .	9	1·27	197·1	Argelander
Procyon . .	1	1·26	214·6	Auwers
36 Ophiuchus	5	1·26	204·5	Auwers
9383 Stone .	6·7	1·26	204·5	Stone
30 Scorpii . .	7	1·25	205·1	Auwers
4887 Lac. . .	6	1·21	288·7	Jacob
4955 Lac. . .	7	1·20	239·4	A.N. 2565
43 Comæ . .	4	1·20	318·4	Auwers
28607 Lal. . .	7	1·20	256·5	A.N. 2806
η Cassiopeiæ .	4	1·19	113·8	Auwers
15565 Lal. . .	8	1·16	184·9	Paris
δ Trianguli .	5·6	1·14	101·1	Auwers
1189 Weisse .	7·8	1·14	28·8	Schjellerup
70 Ophiuci .	4	1·13	169·8	Auwers
θ Ursæ Majoris	3	1·12	240·0	Auwers
20 Cræteris .	6	1·10	322·0	Auwers
27298 Lal. . .	9	1·10	296·3	Argelander
22986 Lal. . .	8·9	1·08	261·0	A.N. 2299
72 Herculis .	6	1·04	173·9	Auwers
27026 Lal. . .	7·8	1·02	241·9	A.N. 2734
8620 Lac. . .	6	1·02	206·8	Stone
16304 Lal. . .	6	1·01	166·8	Paris
44964 Lal. . .	8	1·01	270·0	A.N. 2578
5490 Lal. . .	7·8	1·00	133·6	Argelander

It is evident that the magnitude of the observed motions of stars upon the celestial sphere must depend upon their distance from the sun, as well as upon the actual velocity of the star's motion with respect to the sun; it was at first assumed that the larger stars must be the nearest to us, and that consequently their proper motions would as a general rule greatly exceed the proper motions of smaller stars; but this has by no means been found to be the case.

Mr. Proctor, from a discussion of the facts at his disposal in 1869, recognised that there is no agreement between the magnitude of stars and their apparent proper motions, and, 'judging from the analogy of the solar system, in which the range in the variations of magnitude is enormously greater than the range in the variations of velocity,' he was inclined to look on the proper motions of stars as affording the best evidence we possess respecting their distances.[1]

This investigation has been carried much further by Prof. Eastman. In a paper published in 1889[2] he discussed the relations between the magnitudes and proper motions of 345 stars, ranging from the 5th to the 9th magnitude. Prof. Eastman arranged these 345 stars, whose proper motions had been well determined by Argelander and Bischof, in four classes, according to the amount of their observed proper motion, and he found that the first class, which included the *largest* proper motions, also included the *faintest* stars. In the second class, which contained smaller proper motions, there was a slight increase in the size of the stars; and in all the classes, as the mean proper motion decreased, the mean magnitude increased. Prof. Eastman then examined the proper motions deduced by Prof. Newcomb from 307 of Bradley's[3] stars, and, combining these with the 345 stars contained in the former list, he arranged them in *nine* groups, according to magnitudes. The mean proper motion of the 1st magnitude stars he found to be $0''\cdot52$ per annum—a somewhat anomalous result; but the number of stars contained in this group is quite small, and it contains three stars having large proper motions. For the succeeding magnitudes, from the 2nd to the 9th inclusive, the mean proper motions are respectively $0''\cdot16$, $0''\cdot18$, $0''\cdot14$, $0''\cdot17$, $0''\cdot29$, $0''\cdot42$, $0''\cdot46$, and $0''\cdot68$—a result which shows a general increase of proper motion with decrease of magnitude.

Since the date of the above-mentioned paper Prof. Eastman has made a further classification of stars having well-determined large proper motions,[4]

[1] *Monthly Notices*, vol. xxx. p. 18.

[2] Paper by Prof. J. R. Eastman, published in the *Proceedings of the American Association for the Advancement of Science*, 1889, p. 71.

[3] Prof. Simon Newcomb, *Astronomical Papers of the American Ephemeris*, 1882, p. 147.

[4] See address of Prof. Eastman to the Philosophical Society of Washington, *Bulletin*, vol. xi. p. 166.

excluding from his list all stars having a proper motion less than 0″·05. Five hundred and fifty stars were thus arranged in nine groups. In the first group were placed all stars whose magnitudes were greater than 1·5. In the second group were included all stars between the 1·6 and 2·5 magnitudes; and so on to the ninth group, which contained all stars fainter than the 8·5 magnitude. His results are contained in the following table: [1]—

Group	Number of stars	Mean magnitude	Mean proper motion
			″
1	14	1·13	0·668
2	28	2·15	0·237
3	42	3·08	0·272
4	70	4·02	0·187
5	61	4·89	0·243
6	64	6·12	0·293
7	128	7·04	0·422
8	114	8·08	0·460
9	29	8·78	0·678

—neglecting the result obtained for stars greater than the 1·5 magnitude, which is peculiar by reason of the small number of such stars included in the first group. The proper motions almost uniformly increase as the stars grow fainter, the average proper motion of stars of the ninth magnitude being nearly three times as great as the average proper motion of stars of the second magnitude. In commenting on these results, Professor Eastman remarks that 'assumption, which has developed into a quasi-theory and gained general acceptation, asserts that the largest stars are nearest the solar system. Observation plainly shows this theory is untenable.'

It does not, however, follow from the observed proper motions that the smaller stars must as a general rule be nearer to us than larger stars; the smaller stars may be actually moving in space with so much greater velocity than the larger stars, that, notwithstanding the greater average distance of stars which appear small, their apparent proper motions exceed the apparent proper motions of the stars which appear large. From the facts we are in possession of with regard to observed parallaxes, this seems to be a more probable supposition than that our nearest neighbours amongst the stars are as a general rule small.

(1432.) The apparent proper motion of a star over the heavens will be affected not only by its distance from the sun, but by any motion of the sun in space, for only the relative motion can be detected by us. The problem of the direction of the sun's motion in space was first attacked by Sir William Herschel in a paper which was read before the Royal Society [2] in March,

[1] Compare Ludwig Struve's table given in his paper published in the Mem. of the Imp. Acad. de St. Pétersbourg, tome xxxv. No. 3, page 8.

[2] *Phil. Trans.* for 1783, p. 247. Paper 'On the proper Motion of the Sun and Solar System.'

1783, though the suggestion that the proper motions of the stars were probably partly due to a real motion of the sun in space seems to have been thrown out seven years previously by La Lande.[1] From an examination of the proper motion of thirteen well-known stars, Herschel came to the conclusion that the sun and solar system was moving towards a point in the constellation of Hercules near to the star λ Herculis.

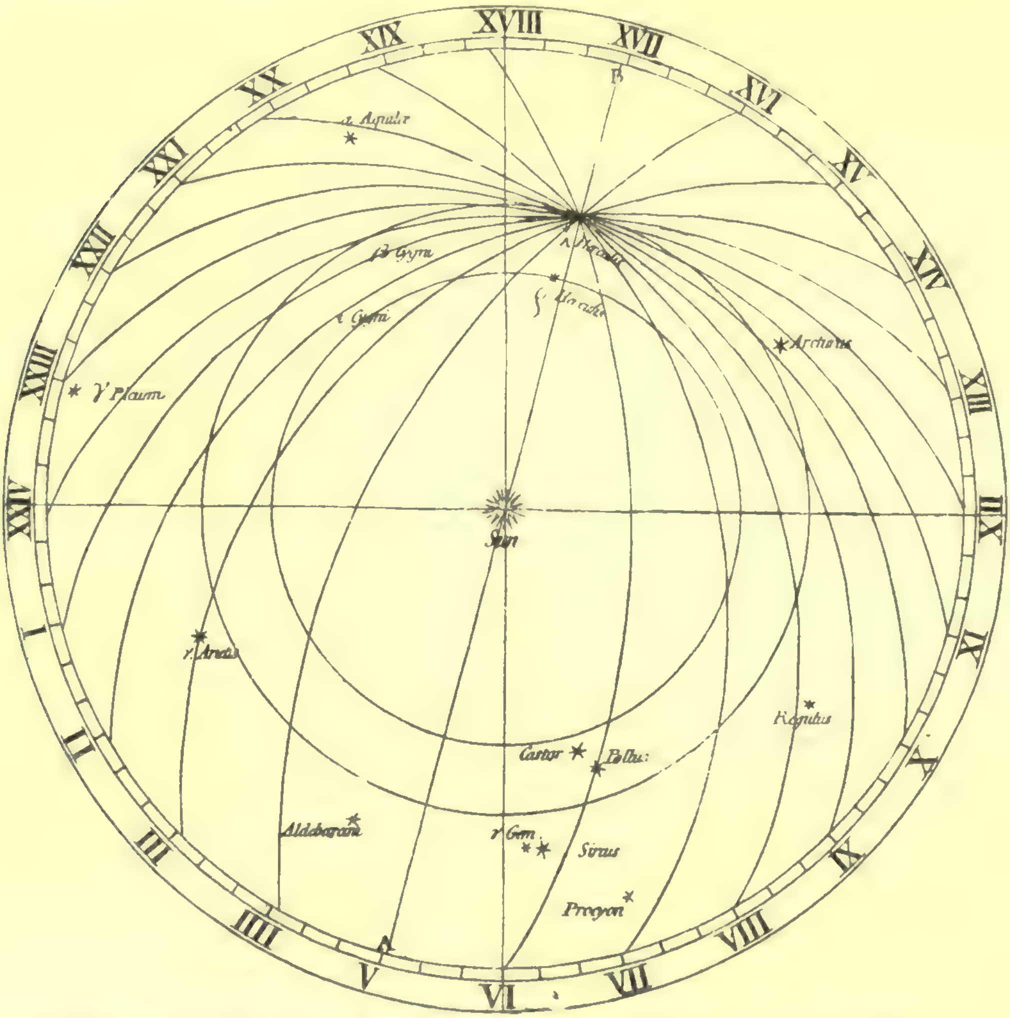


FIG. 461.–Diagram taken from Sir William Herschel's paper in the 'Philosophical Transactions' for 1783, 'On the proper Motion of the Sun and Solar System.'

In fig. 461, taken from Sir W. Herschel's article, the stars are represented as projected on the plane of the equator, and the sun is supposed to be situated at the centre of the sphere. It is evident that if the sun were to move towards the point marked by the intersection of the great circles near λ Herculis, the other stars, if really stationary in space, would all appear

[1] See *Histoire de l'Académie Royale des Sciences*, Paris, 1776, p. 513.

to move away from λ Herculis in directions indicated by the arcs of great circles drawn through their places and the point in the heavens towards which the sun was moving (a point which is usually spoken of as the apex[1] of the sun's way). Thus Pollux would apparently move so that its north polar distance increased, and its right ascension decreased; on the other hand, γ Arietis would move so that its right ascension increased while its north polar distance increased. Sirius being in the Southern hemisphere, must be supposed to be viewed on the concave part of the opposite half of the sphere. Treating Castor as double, Sir William Herschel reckoned that he had 14 stars to deal with, and that the motion of the sun should give rise to 28 motions—*i.e.* 14 motions in R.A., and 14 in declination. By assuming the apex of the sun's way to be near to λ Herculis, Sir W. Herschel found that 22 of these motions 'were satisfied'—that is, the observed motions are of the same sign as they would be if all the motions were produced by the sun's motion in space. As to the velocity of the sun's motion, Sir William Herschel says, p. 273 :—

'From the annual parallax of the fixed stars, which from my own observations I find much less than it has hitherto been proved to be, we may certainly admit (without entering into a subject which I reserve for a future opportunity) that the diameter of the earth's orbit at the distance of Sirius or Arcturus would not nearly subtend an angle of one second ; but the apparent motion of Arcturus, if owing to a translation of the solar system, amounts to no less than 2″·7 a year. . . . Hence we may, in a general way, estimate that the solar motion can certainly not be less than that which the earth has in her annual orbit.'

It is remarkable that the direction of the sun's motion derived by Sir W. Herschel from such meagre data compares very favourably with recent results, and, owing to our want of exact knowledge with respect to the distances of the stars, we should probably be safer if, instead of making numerical estimates as to the velocity of the sun's motion, we still restricted ourselves to the general statement in Sir William Herschel's first paper, that the velocity of the solar motion is comparable with the velocity of the Earth's motion in its orbit.

In 1805[2] Sir W. Herschel, with ampler data at his disposal, returned to the problem of the sun's motion in space, and made a fresh determination of the position of the apex of the sun's way, which he gave as in R.A. 245° 52′ 30″, and N.P.D. 40° 22′ ; and after a year's interval[3] he presented to the Royal Society a paper on the 'Quantity and Velocity of the Solar Motion,'

[1] Sir W. Herschel seems to have been the first to introduce this term.

[2] *Philosophical Transactions*, 1805, p. 233.

[3] *Ibid.*, 1806, p. 205.

in which he concluded 'that in the present state of our knowledge of the observed proper motions of the stars, we have sufficient reason to fix upon the quantity of the solar motion to be such as by an eye placed at right angles to its direction, and at the distance of Sirius from us, would be seen to describe annually an arc of $1''\cdot116992$ of a degree; and its velocity, till we are acquainted with the real distance of this star, can therefore only be expressed by the proportional number 1116992.'

The results of Sir W. Herschel's investigations were doubted by Burckhardt and Bessel, who considered that the apparent stellar motions were so diverse that they could not be reconciled or partly accounted for by any assumption as to the sun's motion. The question cannot be considered as having been finally settled till 1837, when Argelander[1] published an elaborate investigation, in which he divided the stars into three classes according to the amounts of their proper motion. In the first class he placed 21 stars whose proper motions were greater than $1''\cdot0$; in the second class 50 stars with proper motions between $0''\cdot5$ and $1''\cdot0$; and in the third class 319 stars with proper motions between $0''\cdot1$ and $0''\cdot5$. All three classes gave approximately the same position for the apex of the sun's way—viz. R.A. 259° 51′, and dec. + 32° 29′.

I do not propose to endeavour to give a history of the successive investigations with respect to the sun's motion in space; the reader will find a very excellent summary of such investigations given in a presidental address delivered by Prof. Eastman to the Philosophical Society of Washington. It is published in the 'Bulletin Phil. Soc. Washington,' vol. xi. pp. 143–172. The following table, giving a précis of the results obtained, is taken from the same source. I have added to it the results recently obtained by Prof. Lewis Boss,[2] of Albany, and Dr. Oscar Stumpe,[3] of Bonn. In the mean values at the end of the table equal weight has been given to the separate results, though some of the recent determinations are much more reliable than the earlier ones, and such determinations as those of Prof. Boss and Dr. Oscar Stumpe ought not to be combined together as they are derived from different classes of stars.

The values of the solar motion given by Herschel and Airy were considered by the authors themselves as of little weight. Rancken's value is indeterminate, and the value derived by Stumpe from the 58 stars with large proper motion is so large compared with the other determinations that it has been omitted in taking the mean. The mean of the other ten determi-

[1] *Mémoires présentées à l'Académie Imp. des Sciences de St. Pétersbourg par divers Savans,* tome iii.

[2] *Astron. Jour.*, No. 213.

[3] *Astron. Nach.*, Nos. 2999, 3000.

nations is therefore given, though but little reliance can be placed upon it, as it depends upon very unsatisfactory assumptions with regard to the mean distance of stars of various magnitudes.

Author	Date	Number of stars used	Deduced position of the apex of the sun's way		Epoch	Annual angular solar parallax as seen from the mean dist. of a 1st mag. star
			Right Ascension	Declination		
			° ′	° ′		″
W. Herschel	1783	14	260 6	+26 3	—	—
Do.	1805	6	245 52·5	+49 38	—	—
Do.	1806	36	-	—	—	1·117
Gauss	—	—	259 10	+30 50	—	—
Argelander	1838	390	259 51·8	+32 29·1	1792·5	—
Lundahl	1840	147	252 24·4	+14 26·1	1792·5	
O. Struve	1841	392	261 21·8	+37 36·0	1790	0·3392
Galloway	1847	78	260 0·6	+34 23·4	1790	—
Mädler	1848 1856	2,168	261 38·8	+39 53·9	1800	—
Airy	1859	113	256 54	+39 29	1800	1·269
Do.	1859	113	261 29	+24 44	1800	1·912
Dunkin	1863	1,167	261 14·0	+32 55	1800	0·3346
Do.	1863	1,167	263 43·9	+25 0·5	1800	0·4103
Stone	1863	91	—	—	—	0·434
Do.	1863	91	—	—	—	0·341
Gyldén	1872		274 1	-	—	—
Do.	1877	—	260 5	—	1800	—
De Ball	1877	67	269 0	+31 9	1860	—
Rancken	1882	106	285 8·3	+31 52·1	—	(9·79)
Bischof	1884	480	285 2	+48 5	1855	0·3367
Ubaghs	1886	464	262 4	+26 6	1805	
L. Struve	1887	2,509	273 3	+27 3	1805	0·4364
Lewis Boss	1890	135 bright stars	280	+43	1890	—
Do.	1890	144 fainter than 8th mag.	286	+45		—
Do.	1890	253 stars proper motion less than 0″·4	289	+51		—
Oscar Stumpe	1890	551 stars proper motion 0″·16 to 0″·32	287 4	+42	—	0″·140
Do.	1890	340 stars proper motion 0″·32 to 0″·64	279 7	+40 5		0·295
Do.	1890	105 stars proper motion 0″·64 to 1″·28	287 9	+32 1	—	0·608
Do.	1890	58 stars p.m. 1″·28 and upwards	285 2	+30 4	—	2″·057
Mean	—	—	269·13	35 8	—	0·3685

Fig. 462 shows the position with respect to the Milky Way of the different determinations of the place of the apex of the sun's motion as given in the table. Several classes of stars have been made use of to determine the direction of the sun's motion, and they all give approximately similar results. Thomas Galloway,[1] in 1847, discussed the proper motions of 78 stars in the southern heavens, derived from a comparison of the catalogues

[1] *Phil. Trans.*, 1847, p. 79.

of Johnson and Henderson as compared with those of Lacaille and Bradley, and found the most probable position of the solar apex to be in R.A. 260° 0′·6 ± 4° 31′·4, and in dec. + 34° 23′·4 ± 5° 17′·2. Dr. De Ball,[1] in 1877, also arrived at a somewhat similar result from a discussion of the proper motions of 67 southern stars. Prof. Boss has recently discussed the proper motions of 284 stars situated in an equatorial zone 4° 20′ in breadth, with a mean declination of + 3°, which he had observed at Albany for the catalogue of the 'Astronomische Gesellschaft.' Setting aside five stars with

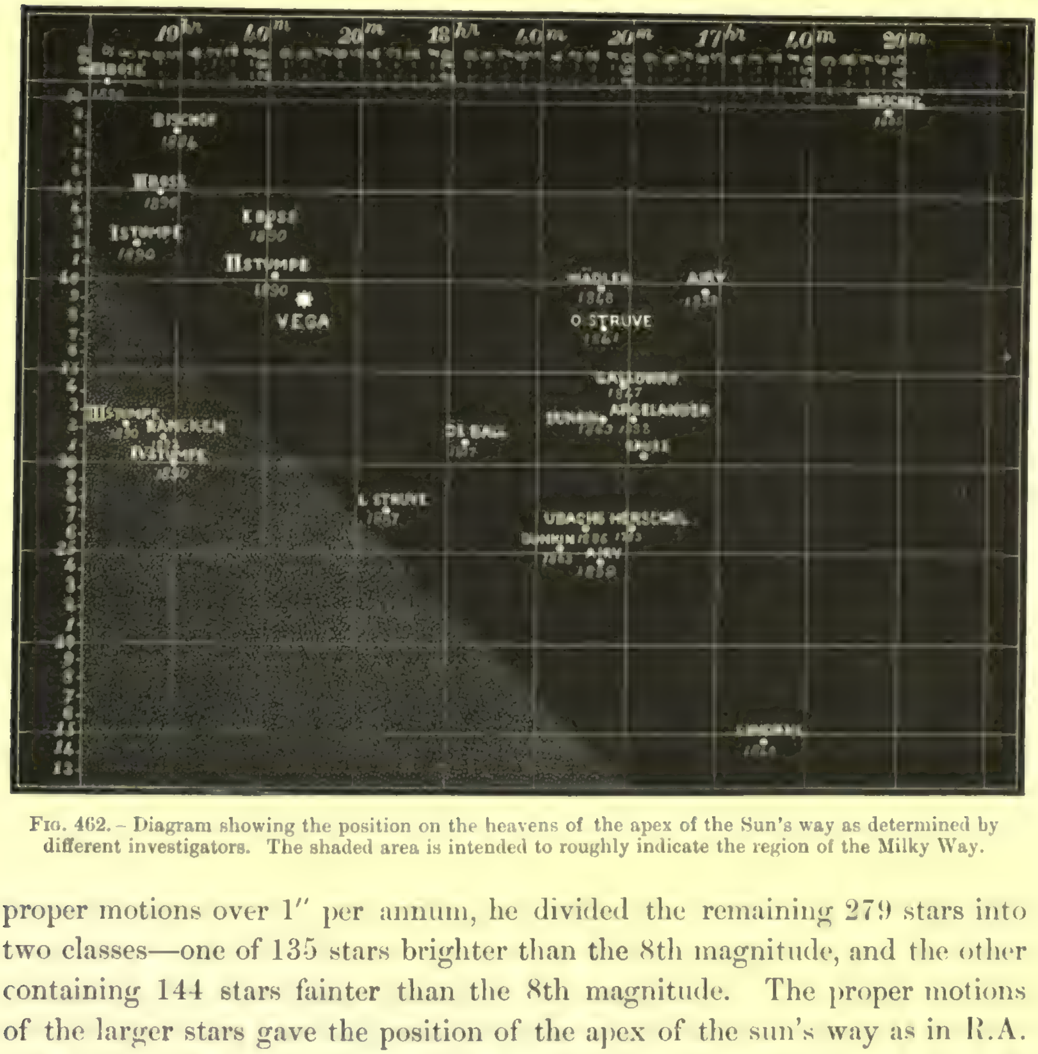


Fig. 462.—Diagram showing the position on the heavens of the apex of the Sun's way as determined by different investigators. The shaded area is intended to roughly indicate the region of the Milky Way.

proper motions over 1″ per annum, he divided the remaining 279 stars into two classes—one of 135 stars brighter than the 8th magnitude, and the other containing 144 stars fainter than the 8th magnitude. The proper motions of the larger stars gave the position of the apex of the sun's way as in R.A. 280°, dec. + 43°; and the smaller stars indicated a position more than 6° towards the north-east—viz. at R.A. 286°, dec. + 45°. Finally, Prof. Boss rejected 26 of the swifter-moving stars with proper motions between 0″·4 and 1″, and from the remaining 253 stars, classed together as a single series,

[1] *Inaugural Dissertation*, Bonn, 1877.

he found the position of the apex of the sun's way to be still further shifted towards the north-east—viz. to R.A. 289°, dec. + 51°; indicating a predominant set towards the north-east of the 26 swifter-moving stars. On the other hand, Dr. Oscar Stumpe, of Bonn, found that the position of the solar apex was shifted towards the south-east by including stars of larger and larger proper motion. He selected 1,054 stars[1] with well-determined proper motions greater than 0″·16. These he divided into four classes, in the first of which he included 551 stars with proper motions between 0″·16 and 0″·32. In the second class he included 340 stars with double the average proper motion of those in the first—viz. from 0″·32 to 0″·64. The third class contained 105 stars with proper motions ranging from 0″·64 to 1″·28; and the fourth class contained 58 stars with proper motions greater than 1″·28. The positions of the solar apex derived from these four classes of stars show a marked and progressive descent of the apex of the sun's motion with the increase in swiftness of the stellar motions; but the advance in right ascension is not progressive, and, considered in conjunction with the three results obtained by Prof. Boss, we are probably not warranted in concluding that the swifter-moving stars (which are probably also near to us) have a common motion or drift in one direction, while those at greater distances have a drift in another direction, which slowly changes according to some simple law as we recede from the sun's place.

It will be noticed that the earlier determinations of the position of the apex of the sun's way form a straggling group towards the south-west, while the more recent determinations, as a general rule, are grouped towards the north-east. The earlier investigations were based on the motions of comparatively large stars, while the more recent investigations include many swifter moving as well as smaller stars. This is in accordance with what might be expected from the results obtained by Prof. Boss, rather than with those of Dr. Stumpe.

(1433.) The classification made by Prof. Eastman only refers to 550 stars having a well-determined proper motion of over 0″·05. But if proper motions afford us the best criterion of distance that we at present possess, this group of stars may be assumed to correspond to our nearest neighbours in space. They appear to be moving in all directions, and do not exhibit any motion about or towards a centre that has at present been detected,[2] or any general drift which would lead one to suppose that they form a system, though there is evidence of parallel motion of groups of stars, called by the

[1] *Astr. Nach.*, Nos. 2999 and 3000.

[2] The conclusions with regard to such a centre arrived at by Mädler and by Mr. Maxwell Hall (see *Mem. R.A.S.* xliii. p. 196), have not been generally accepted.

late Mr. Proctor 'Star-drift.' This drifting together through space of certain groups of stars seems to have been first noticed by Bessel[1] in 1818, who mentions as an instance of it the similar and parallel proper motions of the three stars 36 Ophiuci (A. and B.) and 30 Scorpii. Mr. Proctor called special attention to the evidence which such parallel motion affords that large groups of stars are associated together and form separate systems which probably have a common origin. He gave as a striking instance of such associated motion the parallel proper motion of five of the seven stars of Charles's Wain,[2] which Dr. Huggins also believed from spectroscopic evidence to be moving together in the line of sight. But recent evidence, as pointed out by Mr. Sadler,[3] seems to show that this instance was probably a mistaken one, though the stars δ and γ Ursæ Majoris, separated by an arc of more than 4°, have, according to Dr. Auwers, similar proper motions, and ζ Ursæ Majoris and its naked-eye companion Alcor, as well as the well-known *comes* between the two, form another group with similar proper motions.

Prof. Safford, in the 'Monthly Notices' for 1878, in discussing the proper motions of stars within 10° of the North Pole, picks out six groups of stars which appear to have approximately equal and parallel proper motions, and Mr. Sadler, in the 'English Mechanic' for April 22, 1887, gives several instances of apparently associated proper motions. But perhaps the most interesting of such groups of associated stars was pointed out by Mr. E. J. Stone, who has discussed the motions of four swiftly moving stars in the Southern hemisphere in a paper published in the 'Monthly Notices.'[4] These stars—ζ Toucani, ϵ Eridani, ζ^1 Reticuli, and ζ^2 Reticuli—are distributed over an area of more than 30° upon the heavens, and they appear to be slowly separating and moving away from a common region in the heavens. Their proper motions are not equal, but they are all large—viz. 2″·13, 2″·99, 1″·47, 1″·45—and they appear to be separating with velocities which are, roughly speaking, proportional to the present angular separation of the stars. Though they have been observed for the last 130 years, their proper motions are not sufficiently accurately known to enable us to assert that they are diverging from a point on the heavens; but they must have been much nearer together some 300,000 years ago, and, as Mr. Stone remarks, there seems considerable probability that they at one time were sufficiently near together to sensibly affect each other's motions. They may have been separated by an

[1] *Fundamenta Astronomiæ*, p. 311.

[2] According to Mr. J. K. Esdaile this familiar name has probably nothing to do with King Charles, but is a corruption of the churl's or husbandman's wain or waggon.

[3] 'On the proper motions of $\beta, \gamma, \delta, \epsilon, \zeta$ Ursæ Majoris and Alcor,' *English Mechanic*, April 8 1878.

[4] Paper 'On the Evidence of a Past Connection between Four widely separated Southern Stars,' *Monthly Notices*, Nov. 1879.

explosion, or, as Mr. Stone suggests, these four stars, which range from the 4th to the 5·5 magnitude, may at one time have been a quadruple star moving with considerable proper motion, which passed sufficiently near to another star or system to disturb the relative motions, and to change the closed into open orbits.

(1434.) In the case of stars whose parallax is approximately known we can estimate the actual velocity of the star athwart the line of sight; but if the real motion is oblique to the line in which we see the star, its relative velocity with respect to the sun must be greater than the velocity thus calculated. For example, *α Centauri* has a parallax of about three-quarters of a second, and a proper motion of 3″·62 in a year. Therefore, the relative motion of *α Centauri* with respect to the sun must, in the course of a year, carry it over a distance about five times as great as the distance of the Earth from the sun, or through a distance of 450,000,000 miles in the year—that is, it must at least have a relative velocity with respect to the sun of a little more than fourteen miles a second. Assuming the parallax of 61 *Cygni* to be four-tenths of a second, its annual proper motion of 5″·2 carries it over a distance about thirteen times as great as the distance from the Earth to the sun—that is, it must be moving athwart the line of sight with a velocity relatively to the sun of more than thirty-seven miles a second. Most of the recent measures of 1830 Groombridge give it a very small parallax, not amounting to much more than a tenth of a second, while its proper motion amounts to 7″·05 per annum; and if we assume, as we doubtless safely may, that its parallax does not amount to more than three-tenths of a second, its annual proper motion amounts to more than twenty-three times its parallax. In other words, in the course of a year the star moves athwart the line of sight over a distance equal to more than twenty-three times the Earth's distance from the sun—that is, it must at least be moving with a relative velocity with respect to the sun of sixty-seven miles a second; and, as its position is not very far removed from the apex of the sun's way, nearly the whole of its apparent motion athwart the line of sight must be due to the motion of the star.

Professor Simon Newcomb, more than ten years ago, pointed out that such enormous velocities prove that these swiftly-moving stars cannot be permanent members of the visible universe, but only visitors flying on their way through space at a rate that the combined attraction of all the stars could never stop. He puts his reasoning thus:[1] 'The number of stars actually visible with the most powerful telescopes probably falls short of fifty millions; but, to take a probable outside limit, we shall suppose that within

[1] *Popular Astronomy*, second edition, p. 501.

the regions occupied by the farthest stars which the telescope will show there are fifty millions more, so small that we cannot see them, making one hundred millions in all. We shall also suppose that these stars have, on the average, five times the mass of the sun, and that they are spread out in a layer across the diameter of which light would require thirty thousand years to pass. Then a mathematical computation of the attractive power exerted by such a system of masses shows that a body falling from an infinite distance to the centre of the system would acquire a velocity of twenty-five miles per second; and, *vice versâ*, a body projected from the centre of such a system, with a velocity of more than twenty-five miles per second, in any direction whatever, would not only pass entirely through it, but would fly off into infinite space, never to return. If the body were anywhere else than in the centre of the system, the velocity necessary to carry it away would be less than the velocity just given.'

But this calculated limit is only about a third of the low estimate of the velocity of the star 1830 Groombridge which we have made above. The force required to impress a given velocity on a body falling through any distance is proportional to the square of the velocity; four times the attracting force being required to give double the velocity, and nine times the attracting force to increase it threefold. If, therefore, 1830 Groombridge belongs to the Milky Way system, the mass of that system must be nine times greater than it is assumed to be according to the very liberal hypothesis of Professor Simon Newcomb.

It may be urged that the attracting matter of the Milky Way system may not all be visible to us, and that such dark regions as that from which the tree-like structures shown in figs. 152 and 153 spring[1] may not only be seats of great explosive energy, but regions containing masses of matter the attracting power of which is immense compared with that of the visible stars. But if this were the case we should expect to find that the stars in the neighbourhood of such dark regions possess considerable proper motion, and we should expect that the stars having large proper motions would be moving in a manner which would indicate the positions of such attracting masses. But there is no such symmetry detectable about the directions of the flight of stars having large proper motions, and the paths of the swiftest moving stars appear to be straight; at least no evidence has at present been found indicating that their paths deviate from straight lines,[2] and no evidence has been found indicating a continued acceleration or retardation of the proper motion,

[1] That is, the region marked D in fig. 453.

[2] There are at least two stars which show small periodic irregularities in their proper motions, the cause of which will be referred to later on, but there are no stars whose proper motion seems to indicate the presence in their neighbourhood of bodies sufficiently large to account for the whole of their proper motions.

as might be expected if the swiftest moving stars were passing round dark attracting bodies in their immediate neighbourhood. We are therefore forced to conclude that such swift-moving stars as 1830 Groombridge and 61 *Cygni* are, as Professor Simon Newcomb has aptly termed them, 'runaway stars,' flying through space with such momentum that the attraction of all the bodies of the stellar system associated with the Milky Way can never stop them.

A star moving with a velocity of twenty-five miles a second would pass across the space which separates the earth from α *Centauri* in less than thirty-three thousand years, and it would pass from one side of the Milky Way to the other and out into the space beyond in a period which must be considered as brief compared with the lapse of ages indicated by the geologic record. We may therefore feel assured that the region outside the Milky Way is not devoid of stars.

(1435.) As we have seen, most of the swiftly-moving stars are apparently small. But this is what might be expected if stars occasionally approach one another sufficiently closely to affect each other's motions. Thus, if a small star B, moving in the direction B *b*, with a velocity V, were to come into the near neighbourhood of a large star A, moving in a contrary direction A *a*, also with a velocity V, the small star, after passing round the large star in a narrow[1] hyperbolic path, would on the return branch of the curve have a velocity equal to the velocity of approach (that is, V + V), plus the velocity of the centre of gravity of the two bodies at the time of their nearest approach (that is, a velocity nearly equal to V if the mass of the star B is small compared with the mass of the star A), plus a velocity derived from the accelerating action of gravity during the approach; which latter portion of the velocity would be approximately lost as the small star receded from the large star. In the case we have assumed the small star would, after receding from the large star to a distance where the retarding action of mutual gravity may be neglected, have a velocity equal to nearly three times V, while the large star would have a velocity a little less than V.

Fig. 463.—Diagram illustrating change of velocity on approach of small and large star.

If the small star caught up the large star from behind and passed round it, the small star would lose velocity, or its direction of motion would be

[1] When the approach is so near that the velocity of the small star at its nearest approach is large compared with the original velocity V, the hyperbolic path will be a narrow one. If the original velocity is not small compared with the periastron velocity, the hyperbolic path will be more open. The velocity in a hyperbolic orbit at a distance of a million miles from the centre of our Sun would necessarily be greater than 251 miles per second; a velocity which is probably large compared with most of the stellar proper motions.

reversed if its original velocity were not double that of the larger star. There would, however, be more approaches between stars moving in contrary directions than approaches in which a swifter star catches up a more slowly-moving star, for the chance of an approach or encounter increases with the relative velocity ; consequently, if there were many such approaches between stars there would be a tendency to a distribution of velocities similar to the distribution of velocities amongst the molecules of a mixed gas, where the square of the mean velocity of a molecule is in inverse proportion to its mass. Thus, a star of mass one would on the average move twice as fast as a star of four times its mass.[1]

Besides the small stars above referred to, there are evidently some large stars which are moving with velocities that would carry them out of the limits of the Milky Way system. Thus, Arcturus is ranked by Professor E. C. Pickering as of the 0·0 magnitude of the photometric scale, that is, it is more than two and a half times as bright as a star of the first magnitude. Most observers have found its parallax to be small ; Dr. Ekin gives it as only $+ 0{\cdot}018'' \pm 0{\cdot}022$, and it may no doubt safely be assumed that it is at more

[1] There would be many more approaches than actual collisions between stars, but occasionally collisions must occur, especially in regions like the Milky Way, where stars are thickly distributed. No doubt, as pointed out by Professor George Darwin, when bodies come into collision with planetary velocities, gas will be developed between them at the region of impact sufficiently rapidly to blow them apart again and make them act somewhat like elastic bodies. But bodies of any considerable size would, even if solid, be torn to pieces by the forces developed. In the case of two unequal spheres approaching one another from opposite directions the different portions would have different resulting velocities after the collision. Thus the masses of the colliding portions which lie between the parallels $a\,a_3$ and $b\,b_3$ would be more nearly equal than the masses of the outer colliding zones, which lie in section between the parallels $d\,d_1\,d_2\,d_3$ and $e\,e_1\,e_2\,e_3$. After the impact the two bodies would be torn to pieces, their central portions receding from one another more rapidly than the outer zones. The outermost zone, $e\,f\,e_2$, of the largest body would not suffer any collision, and would proceed onward with its original velocity v, while the velocity of each of the other zones of the larger body would be diminished in different proportions, and the two bodies would be distributed into two long streams of matter in space. A somewhat similar result would evidently follow if the two colliding bodies were not moving in exactly opposite directions but approached one another obliquely.

This may be the origin of the streams of stars linked together by nebulous matter, of which several have been observed, see page 785 and also page 743.

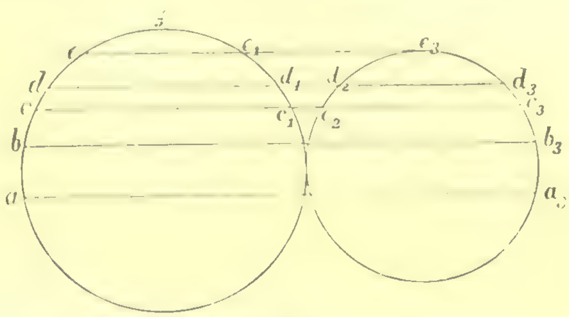


FIG. 464.—Diagram illustrating collision of two spherical bodies.

than double the distance of *α Centauri.* Its annual proper motion is $2''\cdot28$, consequently it must be moving with a velocity of at least 20 miles[1] a second athwart the line of sight. Our sun would certainly appear less than a star of the first magnitude at the distance of *α Centauri*, and at double its distance it would probably appear as a star of about the third magnitude, Arcturus must therefore emit considerably more light than our sun ; and as its light yields a spectrum of the same type as the solar spectrum. it seems probable that Arcturus must be a body considerably larger than our sun. Sirius, the brightest star in the heavens, whose distance we may safely assume to be more than double that of *α Centauri*, is moving with an annual proper motion of $1''\cdot32$, and our own sun is moving with a velocity which cannot be much less than the velocity of the earth in its orbit.

For the reasons already given it seems improbable that the velocity of these large stars can be due to the acceleration of gravity towards large dark bodies in their neighbourhood, but the fact that no change has been observed in the direction of motion or velocity of Arcturus and other swift moving stars for a period of 150 years, enables us to make a minimum estimate of the mass of an attracting body which would be necessary to give rise to, or which would be sufficient to reverse, the motion of such swift moving stars. Our sun is only capable of giving a velocity of 4·76 miles per second to a body at the distance of Neptune[2] which has fallen towards it from an infinite distance, and the velocity of a body about it in a circular orbit at the same distance is only 3·37 miles per second. Consequently it would require a mass two thousand times as great as that of the sun to give a velocity of 21 miles per second to a body which had fallen towards it from an infinite distance, to a distance one hundred times as great as the distance of Neptune, and the velocity in a circular orbit around such a gigantic mass at a distance equal to a hundred times the distance of Neptune would only be 14·96 miles per second, and at double the distance of *α Centauri* from us, two stars at a distance from one another equal to a hundred times the distance of Neptune from the sun. would, if at right angles to the line of sight, only appear to be separated by an angle of $18'\ 45''$.

In 150 years a star moving as swiftly as Arcturus would pass over a space of five and three quarter minutes on the heavens, and if it were moving

[1] This is a minimum estimate ; there can be little doubt that its actual velocity is more than thirty miles a second athwart the line of sight. See an interesting article by Mr. C. W. Maunder on the actual magnitude of the star Arcturus in *Knowledge* for February, 1891. Mr. Maunder is disposed to adopt a much higher estimate of the velocity than I have assumed for the purposes of this argument.

[2] At the distance of *α Centauri* (assuming its parallax to be $0''\cdot75$), our sun is only capable of giving a velocity of 263 feet (or less than the twentieth of a mile) per second, and the velocity in a circular orbit about the sun would be 186 feet per second.

under the influence of gravitation about a dark body at a distance of 18′ 45″, its path would either be sensibly curved or its motion would very sensibly differ from uniform motion. That no such change of direction or want of uniformity in the motion of Arcturus has been detected, shows that if it is moving with an apparent velocity of 2″·28 seconds of arc per annum under the influence of gravitation, the attracting centre must be at a greater distance from it than a hundred times the distance of Neptune from the sun—that is, the attracting mass would have to be much greater than two thousand times the mass of our sun to give such a velocity. Such vast attracting masses would evidently affect the motions of other stars in their neighbourhood, but no such systematic motion of any groups of stars has yet been detected.

If we abandon as altogether improbable the theory that such vast masses of non-luminous matter exist, we are not necessarily thrown back upon the supposition that the large stars must have been created with the proper motions they are now endowed with; there may have been frequent approaches to other stars giving rise to modifications of velocity. But such frequent approaches would, as we have seen, tend to accelerate the mean velocity of the smaller stars and to retard the mean velocity of larger stars. If, therefore, we find a large proportion of the large stars whose motions we are able to estimate endued with velocities greater than can be accounted for by acceleration due to gravity towards the matter of the Milky Way and the stars and non-luminous matter associated with it, we should be forced to assume either that these large stars have acquired their velocity during approaches to still larger stars or non-luminous masses moving with velocities which have originated by the action of gravity, or to abandon the theory that the proper motions of stars can be fully accounted for by the fall of matter towards centres of aggregation. But it must be remembered that only a very small proportion of the stars whose positions are known with sufficient accuracy to detect a shift in their place show any considerable proper motion, and that in the distribution of velocities brought about by approaches the resulting velocities will be ranged about the mean velocity corresponding to the mass of the star according to the law of probable error—some will be larger than the mean and some below it. At present we know too little as to the parallax of stars or their motion in the line of sight to be able to make any estimate of the mean of their actual velocities.

All stars having velocities greater than can be controlled by the gravitation of the Milky Way system cannot permanently belong to that system; but we have seen that a general arrangement of the brighter stars can be traced with respect to the Milky Way, or more probably with respect to the zone of bright stars associated with the Milky Way, but inclined to it at an

angle of about 20° (see pp. 707, 713). This by no means implies that all the stars visible to us belong to the Milky Way system, but it seems probable that at least half of the stars visible to the naked eye, and probably a still larger proportion of the smaller stars down to the 10th and 11th magnitudes, must be members of the Milky Way system, or the symmetry of the stellar distribution with respect to the Milky Way would be less recognisable than it is, owing to such symmetry being masked by the presence of wandering stars which we should not expect to find symmetrically arranged with respect to the Milky Way. It is interesting to note in connection with this, that Mr. Isaac Roberts finds a less obvious connection between the arrangement of the small stars and the clusters of nebulæ which he has been able to photograph in the rich nebulous regions distant from the Milky Way than is recognisable when only the stars of larger magnitude are taken into account. This is as we should expect if these photographs enable us to reach further beyond the limits of the Milky Way system than we were previously able to do.

(1436.) In 1844, Bessel, by comparing the places of Sirius and Procyon as observed by Bradley in 1755 with later observations up to his own time, found that neither of the stars were moving uniformly.[1] And he suggested that the irregularities of the proper motion were in each instance due to the influence of a not very distant and possibly opaque dark body of considerable mass compared with the luminous stars. The elements of the orbit of the hypothetical Sirian satellite were subsequently computed by Peters and Auwers, and its precise position was assigned by Safford in a paper published in 1861. On January 31, 1862, the suspected satellite was detected by Mr. Alvan G. Clark, of Cambridgeport, near Boston. Its position was found to agree with that indicated by Professor Safford, and its motion during the thirty years since the discovery has also conformed fairly well with that of the hypothetical body indicated by the irregularities of the proper motion of Sirius. Fig. 465 shows the orbit as deduced by Mr. Burnham[2] from the observations of the last thirty years. He finds that these indicate a period of fifty-three years. The small star is now so close to the bright star that it cannot even be detected with the great Lick telescope. About the end of 1894 it will probably have passed periastron and be at a sufficient distance from Sirius to enable its position again to be measured. Its radius vector will then have described so large an angle that its period and the elements of its orbit will be deducible with greater accuracy than at present. According to Mr. Gore, the small companion is of about the tenth magnitude

[1] *Astronomische Nachrichten*, Nos. 514, 515, 516.

[2] See *Monthly Notices* of the R.A.S., April 1891, p. 386.

of the photometric scale, and as the large star is ranked as of the $-1 \cdot 4$ magnitude, the small star is about eleven and a half magnitudes fainter than the large star—that is, the large star gives more than 39,000 times as much light as the small star, while the irregularities in the proper motion of Sirius as compared with the dimensions of the ellipse which the small star appears to describe about the large one indicate that the large star cannot have much more than three times the mass of the small one, although the amount of light it gives is more than 39,000 times as great.

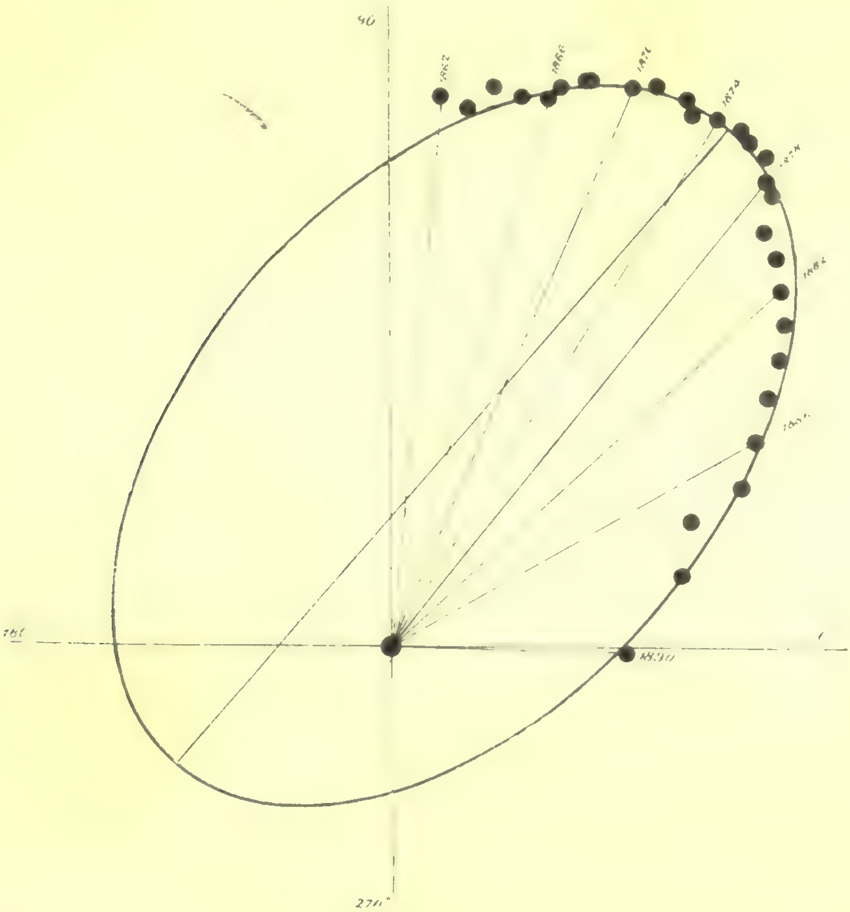


FIG. 465. Apparent orbit of the companion of Sirius, as plotted by Mr. Burnham from measures of its position angle and distance, from 1862 to 1890.

If we assume the parallax of Sirius to be a quarter of a second—that is, that it is situated at about three times the distance of *a Centauri*—and that the mean distance of the companion star from Sirius would subtend an angle of 7″ if the orbit were at right angles to the line of sight, then the actual mean distance of the companion star from Sirius would correspond to about twenty-eight times the distance of the earth from the sun. A planet revolving round

the sun at a distance equal to twenty-eight times the mean distance of the Earth would, according to Kepler's law, occupy just over 148 years in passing round the sun, and the mass of the sun would need to be increased to 7·8 times its present amount in order that the planet might complete its circuit in fifty-three years.[1] According to this estimate the sum of the combined masses of Sirius and its companion would be a little less than eight times the mass of the sun. If, however, we assume with Dr. Elkin that the parallax of Sirius is $0''\cdot4$, the actual mean distance of the companion star from the centre of gravity of the system would be about 17·5 times the mean distance of the Earth from the sun, and the mass of the combined bodies would be only about 1·9 the mass of the sun.

Assuming the distance of Sirius to be three times as great as that of *α Centauri*, our sun would probably appear to shine about as brightly as a fourth magnitude star when seen from a distance equal to that of Sirius. The larger star of this binary system consequently appears about 5½ magnitudes brighter than our sun would appear, and the companion star about six magnitudes fainter than our sun as seen from three times the distance of *α Centauri*; that is, the larger star gives about one hundred and sixty times as much light as our sun would give if seen from Sirius, while the companion star only gives about a two hundred and fiftieth part of the light which would be given by our sun.[2] We must therefore either assume that the photosphere of the large star is very much brighter than the solar photosphere, or that the area of photosphere from which we derive the light of the large star must be very much greater than the photospheric area of the sun—that is, the large star must be much less dense than our sun; while the companion star, which only gives about a two hundred and fiftieth part of the light of the sun, but seems to have sufficient mass to influence the proper motion of the larger star, must either be surrounded by a dense absorbing envelope, or it must be a denser body than our sun, with a relatively small area of photosphere; or its photosphere must be less luminous, area for area, than the photosphere of our sun. This system affords ample evidence that the brightness of stars is not directly proportional to their mass.

The companion star to Procyon, which produces the recognizable irregularities in its proper motion, has not yet been discovered, though Professor Otto Struve announced in 1873 that he had detected a minute star at about

[1] This follows from the law that the square of the periodic time is inversely proportional to the sum of the masses of the attracting bodies.

[2] According to Prof. Pickering (see *Harvard Ann.* vol. xi. p. 281), the small companion of Sirius is 11·93 magnitudes smaller than Sirius—that is, Sirius gives 59,160 times as much light as the small star, while according to Mr. Gore's estimate it gives only about forty thousand times as much light as its companion.

ten seconds distance from the large star in a position where theory indicated that this disturbing body must be. But Mr. Burnham's search for it with the great Lick refractor has proved fruitless, and it seems now to be acknowledged that the appearance of the minute star must have been due to an optical delusion, probably a ghost or false image of the large star due to reflection of its light from the lenses of the object-glass.

(1437.) The movements of the components of the triple star ζ Cancri have been thought to afford evidence of the existence of a large dark companion, whose mass is probably considerable compared with that of the bright stars. ζ Cancri was first divided into a fifth and sixth magnitude star $5\frac{1}{2}''$ apart by Tobias Mayer in 1756. Sir William Herschel in 1781 succeeded in dividing the larger star of the pair into two of the sixth magnitude, which it will be convenient to designate by the letters A and B, calling the more distant single star C. In 1873 M. Flammarion[1] noticed that the motion of the star C about the pair A, B, was not uniform, but was alternately accelerated and retarded at intervals of about seventeen and a half years. Further researches of M. Otto Struve[2] and M. Seeliger[3] led to the theory that the star C is associated with a large dark companion, about

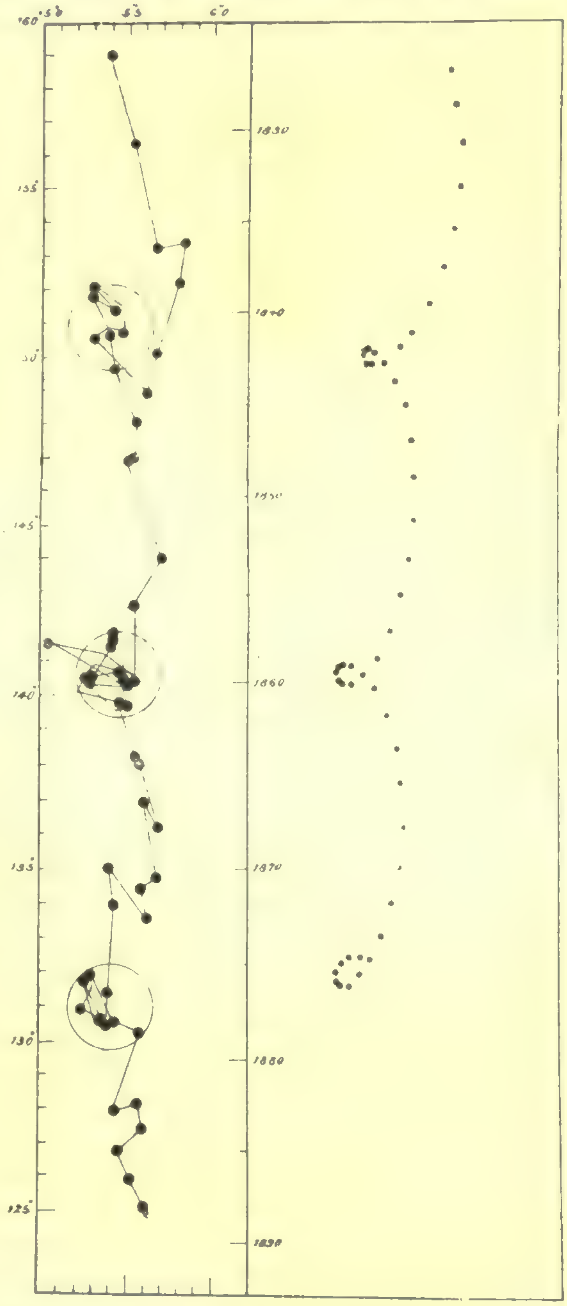


Fig. 466.—Apparent orbit of the star ζ Cancri (C); on the left-hand side as plotted by Mr. Burnham from actual observations, on the right hand side as deduced from theory.

[1] *Catalogue*, p. 49.

[2] *Comptes Rendus*, tome lxxix. p. 1463.

[3] *Observatory*, February 1889, p. 116.

which it revolves in a period of seventeen and a half years in a small nearly circular orbit, with a radius of about one-fifth of a second. But Mr. Burnham[1] has shown that there are considerable probable errors in measuring the position angle and distance of a star revolving about a double star. The observer has to guess at the middle point between the close stars, and to measure the position angle from it. If the two close stars are not equal in mass, the middle point will be a shifting centre to measure from, for it will not correspond to the centre of gravity of the close pair. Mr. Burnham observes that in this instance the star C moves forward through about 10° in seventeen and a half years, while A and B make exactly a quarter of a revolution with respect to the position of C. It seems therefore, as he remarks, more probable that A and B are of unequal mass, and the central point between them is consequently not fixed, than that there should be an invisible star round which C is rotating in a period which happens to correspond so exactly with the intervals between the epoch when A B and C occupy the same relative position with respect to one another. Whichever theory we adopt to account for the observed motion of the system, we are forced to conclude that the relative brightness of the stars composing it does not correspond with their mass[2]—that is, that the largest stellar mass has not in the process of cooling remained the most luminous—as we might reasonably expect, if we knew that all the members of the system had a contemporaneous origin, that they were all composed of similar material, and that they all commenced to cool from the same initial temperature, and had not been disturbed or heated up again during the process of cooling. We learn a similar lesson from stars of the Algol type.

(1438.) There are now ten stars of this type known. Algol, one of the brightest of them, had long been known to be subject to variations in brightness, but no systematic attempt seems to have been made to ascertain its period of change till Goodricke (an amateur observer living near York) commenced to observe the star in 1782. By continuing for some time to watch its changes of brightness, he found that it had a regular period, which he estimated at 2d. 22h. 48m. 59s.[3] For the greater part of this time its light is subject to no variation, and it shines as a star of the second magnitude. It then commences to decrease, and in about four and a half hours loses nearly three-fifths of its light, and for a period of about twenty minutes remains at its minimum brightness, shining as a star of about the fourth magnitude, and

[1] *Monthly Notices*, April 1891, pp. 388–394. This is a very important and critical paper on the evidence with regard to 'invisible double stars.'

[2] On the one theory we are forced to assume the existence of a dark star of considerable mass, and on the other theory we must assume that the two close stars, though nearly equal in brightness, have very different masses.

[3] See *Phil. Trans.*, 1783, p. 474, and *ibid.*, 1784, p. 289.

then in four and a half hours recovers its original brightness. Goodricke suggested that the changes of light must be caused by the interposition of a dark satellite, and this theory has been shown by a stricter enquiry of Prof. E. C. Pickering [1] to be highly probable, for the light of Algol decreases, as it should do if the radius of the bright star were 1·000 and the radius of the eclipsing satellite were ·764. To give a more definite idea of the dimensions of this system, Prof. Pickering gives the accompanying diagram, in which fig. 1 represents the projection of the orbit as seen from the Earth, and fig. 2 the orbit in its own plane. In both projections A denotes the primary, B the satellite at first contact, C when half across the disc, D at the last contact, and E and F at the elongations.

Prof. Vogel, of Berlin, found in 1888–9, by the spectroscopic method, that the whole system is moving towards us with a velocity of about $2\frac{1}{3}$ miles a second, and that before each minimum, when the satellite is at greatest elongation, and is moving towards us, the bright star is moving away from the sun with a velocity of about 24 English miles per second; after each minimum the bright star moves towards us till the time when the eclipsing satellite would be at greatest elongation on the other side, when the bright star is moving towards us with a velocity of about $28\frac{2}{3}$ miles a second. This corresponds to a velocity for the bright star of $26\frac{1}{3}$ miles per second in its orbit, and enables us to determine the diameter of the orbit, supposing it to be circular. Multiplying the distance described in a second ($26\frac{1}{3}$ miles) by the number of seconds in Algol's period (247,732 secs.), we get an orbital circumference corresponding to a diameter of about two million miles.

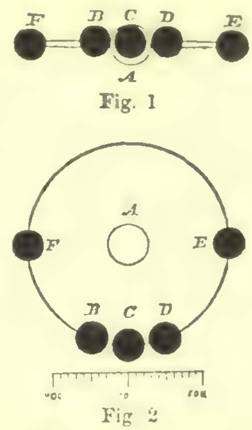


FIG. 467. Prof. Pickering's diagram of the orbit of the dark companion of Algol.

If we assume that the two bodies are of equal density, their relative mass may be determined from the obscuration of light on the assumption that, at central transit, the dark body as seen from the earth is wholly projected on the disc of the bright body. By the aid of this supposition Prof. Vogel arrived at the following provisional data for the system of Algol:—

Diameter of Algol	1,061,000 English miles
„ „ satellite	830,300 „ „
Distance from centre to centre	3,230,000 „ „
Orbital velocity of Algol	26·3 miles per second
„ „ „ satellite	55·4 „ „ „
Mass of Algol	$\frac{4}{9}$ solar mass
„ satellite	$\frac{2}{9}$ „ „

[1] 'The Dimensions of the Fixed Stars, with special Reference to Binaries and Variables of the Algol Type,' *Proceedings of the American Academy*, vol. xvi.

This gives a mass for Algol and its satellite which denotes a density less than a quarter that of the sun, or 0·38 that of water.

The period of Algol seems to be slowly changing ; it is now some six seconds shorter than it was in Goodricke's time. The following values are given by Prof. Pickering as corresponding to the periods of this curious binary at intervals of ten years since it was first accurately observed :—

	days	hours	min.	sec.		days	hours	min.	sec.
1780 . . .	2	20	48	58·6	1840 . . .	2	20	48	56·6
1790 . . .	,,	,,	,,	58·6	1850 . . .	,,	,,	,,	53·7
1800 . . .	,,	,,	,,	58·5	1860 . . .	,,	,,	,,	53·3
1810 . . .	,,	,,	,,	58·5	1870 . . .	,,	,,	,,	53·0
1820 . . .	,,	,,	,,	58·4	1890 . . .	,,	,,	,,	52·7
1830 . . .	,,	,,	,,	58·3					

Dr. Chandler, in an interesting discussion of the observations of Algol in Gould's 'Astronomical Journal,'[1] concludes that the irregularities of the period of revolution of the satellite recur in a cycle that evidently exceeds a hundred years, which it is difficult to account for by the perturbing action of a third body exterior to the close pair—for, in order to produce any marked perturbing effects, it should revolve much nearer to them than would be consistent with so long a period. He is, therefore, inclined to believe that the apparent variation of the period is produced by the motion of the close pair round a dark body, at a distance equal to about the distance of Uranus from the sun in a period of about 130 years ; and that the varying intervals between the minima are due to the time occupied by the transmission of light across the orbit described by the close pair around the distant dark star, in a manner similar to that in which the eclipses of Jupiter's satellites are apparently accelerated and retarded in proportion to the distance of the Earth from Jupiter.

According to Dr. Chandler, Algol and its close dark companion describe a nearly circular orbit around the remoter dark star in a plane which is inclined at an angle of about 20° to our line of vision. In the interval between 1804 and 1869 Algol was sweeping out the remoter part of its orbit, and, consequently, all the observed eclipses were in arrear of the calculated times. From 1804 to 1843 the retardation accumulated until the eclipses were 165 minutes in arrear. Since light takes about eight minutes to pass from the sun to the earth, this would correspond to the passage of light across a space equal to about twenty times the Earth's distance from the sun, or to a little more than the distance of Uranus from the sun.

If Dr. Chandler's theory is true, Algol must be a very brilliant star as compared with our sun, for its proper motion is estimated at less than two

[1] *Astronomical Journal*, Nos. 255, 256.

seconds of arc in a century. A star revolving in an orbit twenty times the diameter of the earth's orbit would, if it were situated at the distance of *α Centauri* from the sun, appear to shift its place in the heavens through about half a minute of arc in passing from one side of its orbit to the other; and such motion across the line of sight would be very readily detected. If its proper motion does not exceed two seconds, and Dr. Chandler's theory furnishes the true explanation of the observed irregularities in the period between the eclipses, Algol must be fifteen times as distant as *α Centauri*. Our sun at such a distance would probably not appear brighter than a star of the $7\frac{1}{2}$ magnitude, while Algol shines as a star of the second magnitude, and consequently gives about one hundred times as much light as our sun would give at such a distance. If, in conjunction with this, we accept Prof. Vogel's estimate of the diameter of Algol (1,061,000 English miles), derived from the spectroscopic method of determining the motion in the line of sight, Algol must be more than sixty times as brilliant, area for area, as our sun, while it is accompanied by two dark bodies, one of which is more massive than the bright body.[1]

(1439.) Very various estimates have been made of the amount of light given by the sun as compared with that given by the fixed stars. Christian Huyghens, in the 'Cosmotheoros,' published at the Hague in 1698, estimated, from rough observations made by observing the sun through a minute hole, that it gives us 756,000,000 times as much light as *Sirius*. The Rev. John Michell in 1767 compared the light of the sun with the light of Saturn, which, he assumed, shines as a first magnitude star; and, with the striking ingenuity which always distinguished him, he derived some remarkably correct conclusions.[2] If, said he, the mean distance of Saturn from the sun is equal to 2,080 of the sun's semi-diameters, and if the diameter of Saturn as seen from the earth is one 105th part of the diameter of the sun as seen from the earth, then, on the assumption that Saturn reflects the whole of the light which falls upon it from the sun, it can only return to us one 48,100,000,000th part of the light of the sun. In other words, a star of the first magnitude probably

[1] Miss A. M. Clerke has given an interesting account of various speculations with regard to the Algol system in *Knowledge* for May 1892. If we make Dr. Chandler's assumption as to the motion round a distant dark body, it is still possible to conceive that the apparent proper motion of Algol might be small, and its distance much less than fifteen times the distance of *α Centauri*. For Algol and its dark companion might be moving in an elongated ellipse about the distant dark body, with the major axis of the orbit directed towards the Earth. Dr. Chandler's reason for rejecting this supposition is, that the period of Algol seems to have varied very little between 1780 and 1830; it then decreased very rapidly, and is now decreasing more slowly; whereas, according to the above supposition, the period ought, if Algol is approaching periastron with the perturbing dark body, to have shortened with continually increasing rapidity. It is evident that there must be considerable uncertainty in determining the epoch of a single minimum within a few minutes.

[2] *Phil. Trans.*, 1767, p. 235.

gives less than one 48,000,000,000th part of the light of our sun—that is, it gives about as much light as our sun would give if removed to 220,000 times its present distance from us. But if Saturn, instead of reflecting the whole of the light that falls upon it, should, like the moon, only reflect a sixth part of the incident light, we must increase the distance computed above in the proportion of $2\frac{1}{2}$ to 1 to make the sun's light no greater than that of a star of the first magnitude. Michell consequently concluded that the fixed stars, if similar to the sun, were probably at distances where their parallaxes would be found not to exceed two seconds, and probably not to be greater than one second of arc.

Wollaston in 1829 compared the image of the sun and of a lamp reflected by a silvered bulb of glass, and concluded that the sun gives 20,000,000,000 times as much light to us as *Sirius* gives.[1] Steinheil in 1836 compared the light of the sun with that of the moon, and the light of the moon with that of *Sirius*, and concluded that the sun gives 3,840,000,000 times the light given by *Sirius*.[2] In 1861 Bond determined the relative light of the sun and moon by comparing their reflections in a glass globe with that of an artificial light, and, combining his measures with the comparisons of the moon and *Sirius* by Herschel and Seidel, he determined the light of the sun to be 5,970,500,000 times that of *Sirius*.[3] In 1863 Clark found that, if the sun was removed to 1,200,000 times its present distance and *Sirius* to 20 times its distance, they would appear equally bright, and would about correspond to a star of the sixth magnitude, which gives the ratio of brightness as 1 to 3,600,000,000.[4] Prof. E. C. Pickering in 1880 reduced these measures of the sun's brightness to stellar magnitudes, and obtained the following values: Huyghens, 22·20; Wollaston, 25·75; Steinheil, 23·96; Bond, 24·44; and Clark, 23·89. The mean of all these is 24·05, and assuming the magnitude of *Sirius* to be −1·5, he obtained as the stellar magnitude of the sun, −25·5.

On the above assumption with regard to the sun's stellar magnitude, we may calculate the distance to which the sun would need to be removed in order to reduce its light to equality with a star of the first magnitude, or the stellar magnitude which the sun would present if seen from a distance corresponding to any parallax. Thus, if the sun were removed to 206,200 times its present distance, it would have a parallax of about one second, and its light would be reduced in the proportion of $(206{,}200)^2$ to 1, or about 26·57 stellar magnitudes, and it would appear to shine as a star of the 1·07 magni-

[1] *Phil. Trans.* 1829, p. 28.

[2] *Elemente der Helligkeits-Messungen*, Munich, p. 24.

[3] *Mem. Amer. Acad.*, viii. N.S., p. 298.

[4] *Amer. Journal of Science*, xxxvi. p. 76.

tude. The following table gives the light in stellar magnitudes which would be emitted by the sun if removed to the distances corresponding to the parallaxes given in the first column :—

Parallax	Magnitude	Parallax	Magnitude
0″·1	6·07	0·6	2·18
0·2	4·57	0·7	1·84
0·3	3·68	0·8	1·56
0·4	3·06	0·9	1·30
0·5	2·58	1·0	1·07

If the stellar magnitude of the sun is assumed to be $-26{\cdot}0$ instead of $-25{\cdot}5$, all the stellar magnitudes in the above table must be increased by 0·5 ; and if there is any loss of light in its passage through space owing to absorption or obstruction by opaque matter, the stellar magnitudes in the table must be diminished in an increasing ratio as the parallax diminishes.

Prof. E. C. Pickering, in his paper on 'The Dimensions of the Fixed Stars,' [1] gives the apparent diameter in seconds of arc of stars of various stellar magnitudes, calculated on the assumption that they are bodies similar to our sun, and that their photospheres give an equal amount of light, area for area, with the solar photosphere. It will be seen from the following table that the apparent diameters of the fixed stars are probably very much less than the smallest bodies which can be recognised as presenting a visible disc in the largest telescopes yet constructed.

Magnitude	Diameter	Magnitude	Diameter	Magnitude	Diameter
0 [2]	0″·01528	5	0″·00153	10	0″·00015
1	0·00964	6	0·00096	11	0·00010
2	0·00608	7	0·00061	12	0·00006
3	0·00384	8	0·00038	13	0·00004
4	0·00242	9	0·00024	14	0·00002

When the parallax of a star is known, its linear diameter, as estimated from its magnitude, may be compared with the linear diameter of the sun as seen from the same distance. Thus, if the sun were removed to the distance of a star having a parallax p, its diameter would have the same ratio to the parallax that the chord of the sun's diameter, as seen from the Earth, has to unity. It would, therefore, equal

$$2\,p \sin 16'\,2'' = 0{\cdot}00933\,p.$$

The great Lick telescope, which is 36 inches in diameter, will not properly divide a double star whose components are a tenth of a second apart.

[1] *Proceedings of the American Academy*, vol. xvi. p. 4.

[2] 0 magnitude is one magnitude above the first magnitude.

It would consequently need a telescope of fully seven times the diameter of the Lick telescope to show a recognisable disc with a star of 0 magnitude, and a telescope of ten times the diameter of the Lick telescope to show a recognisable disc with a star of the first magnitude, if its photosphere was similar in brightness to the photosphere of our sun.[1]

(1440.) A century and a quarter have passed since the Rev. John Michell pointed out that, if we knew the period of revolution of a double star and the stellar magnitudes and apparent mean distance of its components, we should have the means of discovering the proportion between the light of the sun and the light of those stars relatively to their respective quantities of matter. At the date of his paper, which was communicated to the Royal Society in 1767, no binary star was known to have one of its components actually revolving about the other. In a note to his paper Michell says :—[2]

> If, however, it should hereafter be found that any of the stars have others revolving about them (for no satellites shining by a borrowed light could possibly be visible), we should then have the means of discovering the proportion between the light of the sun and the light of those stars, relatively to their respective quantities of matter; for in this case, the times of the revolutions and the greatest apparent elongations of those stars that revolved about the others as satellites being known, the relation between the apparent diameters and the densities of the central stars would be given, whatever was their distance from us; and the actual quantity of matter which they contained would be known whenever their distance was known, being greater or less in proportion to the cube of that distance. Hence, supposing them to be of the same density with the sun, the proportion of the brightness of their surfaces compared with that of the sun would be known from the comparison of the whole of the light which we receive from them with that which we receive from the sun; but if they should happen to be either of greater or less density than the sun, the whole of their light not being affected by these suppositions, their surfaces would indeed be more or less luminous accordingly as they were upon this account less or greater; but the quantity of light corresponding to the same quantity of matter would still remain the same.
>
> The apparent distances at which satellites would revolve about any stars would be equal to the semi-annual parallaxes of those stars seen from planets revolving about the sun in the same periodical times with themselves, supposing the parallaxes to be such as they would be if the stars were of the same size and density with the sun.

Michell's important suggestion remained unused and unnoticed long after the necessary data with respect to the approximate periods and orbits of

[1] These calculations are made on the supposition that the stars are spherical or similar in form, and similarly placed with regard to the line of sight. Thus, a binary star, with equal spherical components, would, if neither of the components eclipsed the other, give $\sqrt[3]{2}$ times the light given by a single spherical star of similar mass, density, and surface brightness to the binary star.

[2] *Phil. Trans.*, vol. lvii. p. 238. The actual physical connection between binary stars was not established till the publication of Sir William Herschel's paper in the *Phil. Trans.* for 1803.

several binary stars had been in the hands of astronomers. It was not till 1880 that Prof. E. C. Pickering independently arrived at a similar conclusion [1] with regard to the possibility of calculating the relation between the mass and light-giving power of binaries, whatever their distance.

He pointed out that the angular diameter of a star having the same mass as the stars of a binary system, and the same density and intrinsic brightness as our sun, might be calculated without any knowledge of the distance of the binary when the period of the binary is known as well as the mean angular distance of its components. By comparing the angular diameter thus calculated with the angular diameters corresponding to the stellar magnitudes of the components of the binary, we may determine the brightness of the stellar photospheres relatively to the solar photosphere; and assuming the stars to have the same density as our sun, or assuming the intrinsic brightness of their photospheres to be equal to that of the sun, we may calculate the density of the binary stars compared with the density of our sun, or the brightness of their photosphere compared with the solar photosphere. Prof. Pickering applied his method to several well-known binary systems, and showed that they exhibit a wide range in their emissive powers; or, if their photospheres are all equally bright, that the density of the matter within the photospheres differs very considerably in passing from one binary to another.

According to Prof. Pickering, the photosphere of γ Leonis must be three hundred times more brilliant than the solar photosphere if it has the same density as the sun; or, if we suppose its photosphere to be no brighter than the sun's photosphere, its density would need to be only one-seventh of that of atmospheric air at the standard density and pressure to give it sufficient bulk to emit its observed light. Most of the binaries examined by Prof. Pickering were found to have greater emissive power per unit of mass than the sun, while a few appear to have either fainter photospheres than the solar photosphere or to be denser bodies. Thus, according to Prof. Pickering, 61 Cygni has a relative diameter 0·66 compared with the sun—that is, its photosphere gives a little less than half the light, area for area, of the solar

[1] *Proceedings of the American Academy of Arts and Sciences*, vol. xvi. p. 5. Prof. Pickering says: Let N denote the mass of the binary in terms of that of the sun; P the period of revolution in years; a the semi-axis major, or mean distance of the components; and b the equivalent diameter, or the diameter of a star having the same mass as the binary, and the same density and intrinsic brightness as the sun. Comparing the binary with the system formed by the sun and Earth seen at the same distance, we see that the two systems have masses in the ratio of N to 1, mean distances in the proportion of a to p (where p is the parallax), and periods of revolution as P to 1. Accordingly, by Kepler's law, $N : 1 = \frac{a^3}{P^2} : \frac{p^3}{1}$, or $N = \frac{a^3}{p^3 P^2}$. But $N = \frac{b^3}{(0{\cdot}00933\,p)^3}$, since $0{\cdot}00933\,p$ will equal the diameter of the sun at the distance of the binary. Hence, equating these two values of N, p is eliminated, and we have $b = 0{\cdot}00933\,.\,a\,P^{-\frac{2}{3}}$. The stellar magnitude corresponding to the diameter b may be found from Prof. Pickering's table given on p. 777.

photosphere—or the component stars have a greater density than the sun; and the components of p Eridani only give about one-sixteenth the light of the sun, area for area, or, if their photospheres are of equal brightness to the solar photosphere, the component stars are together sixty-four times as dense as the sun.

In 1887 Mr. Monck of Dublin—also, apparently, independently of his two predecessors[1]—arrived at a similar method of comparing the mass-brightness of different binary systems. Like Prof. Pickering, he made use of Kepler's law, connecting the mass, the period, and the mean distance to get rid of the parallax; but one binary system is compared directly with another, and no comparison is made with the solar brightness and mass.

Pickering's and Monck's method is only accurate on the assumption that the two components of the binary have the same density and brightness, or when one component is so much smaller than the other that its mass may be neglected; both methods of comparison break down when the two components do not differ greatly in mass but differ considerably in brightness.

(1441.) Upwards of 10,000 pairs of stars in close proximity to one another in the heavens are now known. ζ Ursae Majoris, the middle star in the tail of the Great Bear, seems to have been the first pair of this class recognised. It was found by Riccioli[2] at Bologna, in 1650, to consist of a $2\frac{1}{2}$ and a fourth magnitude star, within fourteen seconds of arc of each other. Robert Hooke, in 1665, discovered γ Arietis to consist of two fourth magnitude stars, eight seconds apart. Huyghens, in 1656, found θ Orionis to be triple, and in 1684 he perceived it to be quadruple; α Crucis and α Centauri were also discovered to be double during the seventeenth century. In addition to these five, four more—γ Virginis, Castor, 61 Cygni, and β Cygni—were discovered in the course of the next half-century. All these were discovered with non-achromatic telescopes. The invention and manufacture of achromatic telescopes by John Dollond in 1758, and great improvements in the manufacture of reflecting telescopes, made further discoveries in this field of research easier. Christian Mayer of Mannheim began, in 1776, a deliberate search for stellar couples, and was rewarded by the discovery of 33 double stars in the course of two years. Four years later Sir William

[1] See the *Observatory* for February 1887, p. 97. Mr. Monck shows that, if we take I as the apparent brightness of a binary photometrically determined, a the semi-major axis of its orbit (in seconds of arc), and t its period in years, while k represents the brilliancy or illuminating power of the binary, that we have, on comparing two binaries,

$$k_1 : k_2 = I_1 t_1^{\frac{4}{3}} a_1^2 : I_2 t_2^{\frac{4}{3}} a_2^2, \text{ or } \frac{k_1}{k_2} = \frac{I_1}{I_2}\left(\frac{t_1}{t_2}\right)^{\frac{4}{3}}\left(\frac{a_2}{a_1}\right)^2$$

—that is, the relative brightness of the binaries may be expressed in terms of the apparent brightness, the periods, and the angular distance of the components, independently of their parallax.

[2] See Miss Clerke's *The System of the Stars* p. 163, where an excellent summary of the history of the discovery of double stars is given.

Herschel writes[1] that he 'set himself to examine every star in the heavens with the utmost attention and a very high power,' in order that he might obtain materials for a research with regard to the parallax of the fixed stars. In 1782 he presented to the Royal Society a classified catalogue of 269 double stars, and three years later he followed it with an additional list of 434 double stars, giving the distances separating the components and, with very few exceptions, the direction of the line joining their centres.

Herschel subsequently enlarged these lists of telescopic double stars, which he sought for and measured in the hope of finding some in which the distance and position angle might vary in the course of the year, indicating a parallax as the earth moves round its orbit. But, instead of finding a yearly oscillation in the distance and position angle as he expected, he found in many cases a progressive change, indicating that one of the stars was slowly describing an orbit round the other—a discovery which was announced by him to the Royal Society in 1803;[2] and before his death he had succeeded in bringing together evidence of the orbital revolution of fifty double stars.

According to Mr. Gore,[3] the number of double stars known to be physically connected now amounts to nearly a thousand. In most of these the motion is very slow, and in only about sixty cases has the relative change of position since their discovery been sufficient to enable an orbit to be computed. Of such telescopically observable double stars, the periods of revolution in the computed orbits vary in length from $11\frac{1}{2}$ to 1,626 years. Of the sixty orbits which have been computed, there are about twenty-one with periods under 100 years.

The planes of the orbits in which double stars move about one another are generally not at right angles to the line of sight, and the true orbits can only be determined from the apparent orbits on the supposition that the stars are moving about one another under the influence of mutual gravitation, so that the line joining the stars sweeps out equal areas in equal times. So far as the true orbits have already been determined, it appears that the planes in which the stars are revolving about one another are inclined at all angles to the plane of the Milky Way.

[1] *Phil. Trans.* vol. lxxii. p. 97.

[2] In speaking of this discovery, Herschel said that he 'went out, like Saul, to seek his father's asses, and found a kingdom'—the dominion of gravitation extending to the stars. The discovery was, however, by no means unexpected. In 1767 the Rev. John Michell, arguing from the doctrine of probabilities, wrote in the *Phil. Trans.*, vol. lvii. p. 249: 'It is highly probable in particular, and next to a certainty in general, that such double stars as appear to consist of two or more stars placed very near together do really consist of stars placed near together, and under the influence of some general law.' In 1784 Michell wrote (*Phil. Trans.*, vol. lxxiv. p. 56): 'It is not improbable that a few years may inform us that some of the great number of double, triple stars, &c., which have been observed by Mr. Herschel are systems of bodies revolving about each other.'

[3] See paper 'On Binary Stars of Short Period' published in *Knowledge* for August 1890.

(1442.) It can hardly be argued that the binary stars have all had their origin in the approach of isolated stars from different parts of space; for, under all ordinary circumstances, such separate stars would describe hyperbolic or parabolic orbits about one another, and, after their nearest approach, they would separate again for ever, unless they became involved in one another's coronas or atmospheres at their nearest approach, or unless there were a third attracting body in the neighbourhood whose perturbating action might, under certain circumstances, sufficiently reduce their velocity to leave them revolving about one another in ellipses. If their velocity were reduced at nearest approach by the impact of their gaseous surroundings, the periastron distance would be reduced at each succeeding revolution, and they would speedily coalesce and become one body. The great number of such associated pairs of stars in the heavens seems to render it probable that nebulous or stellar masses in the process of cooling and condensing frequently divide into two.

Mr. T. J. J. See has remarked that the stellar orbits, as far as we at present know them, are much more eccentric than the orbits of the planets of the solar system and their satellites.[1] He accounts for this fact by the perturbations produced by tidal action. In the case of such large bodies revolving in close proximity to one another the tidal effects would be very considerable, for the tide-generating force varies inversely as the cube of the distance of the tide-raising body, and the tidal friction varies as the square of the tide-generating force—that is, inversely as the sixth power of the distance; consequently, in the case of stars in close proximity, all the effects of tidal action would be greatly exaggerated. Mr. See has shown that the tidal action would, as in the case of the Earth and moon, tend slowly to separate two closely-revolving stars, and at the same time it would tend to make their orbits more and more elongated. Figs. 468 and 469 represent the elongated apparent orbits of 70 Ophiuchi and 228 Struve, as given by Mr. Gore.

According to this theory of the genesis of double stars, the axial rotation of the parent nebula or star would increase as the nebula or star cools and shrinks until it becomes so oblate that the matter at its equatorial regions parts from the central portions, and either forms a ring, or, in the case of a non-homogeneous parent body, a single satellite might be formed without the interposition of the ring stage. Mr. See remarks that the double forms of some of the nebulæ seem to him to indicate that the process of 'fission' frequently takes place without the formation of a ring.

[1] Of the sixty-four stellar orbits now roughly known, the mean eccentricity is 0·48; while the mean eccentricity of the orbits of the eight great planets and their twenty satellites is less than 0·0389. See *Knowledge* for May 1892, p. 83.

There is a large class of double stars—perhaps amounting to one-third of the total number of double stars known—in which the two components are nearly equal in brightness; outside this class there are examples of all gradations of relative brightness, and a large number in which the smaller companion, or *comes*, is relatively very faint. Frequently the component

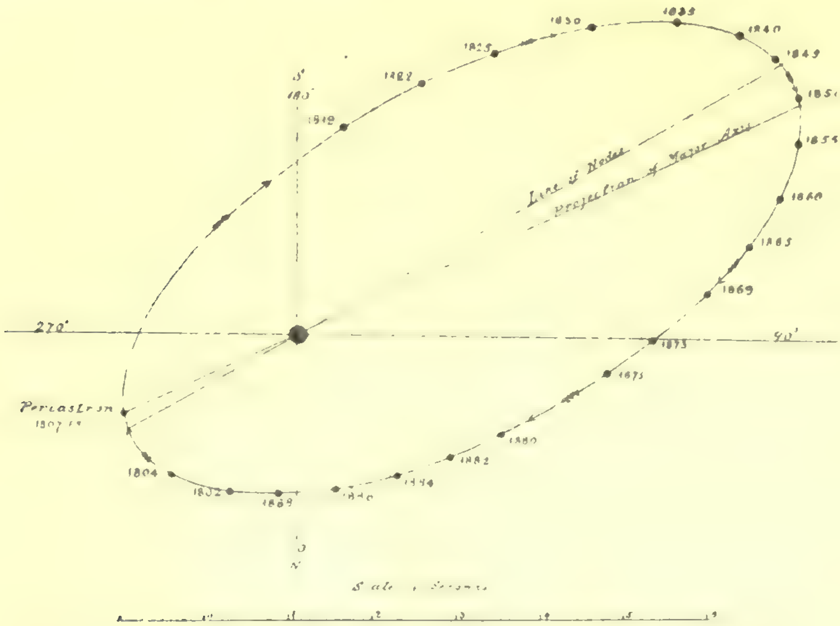


FIG. 468.—Diagram, by Mr. J. E. Gore, representing the apparent orbit of 70 Ophiuchi.

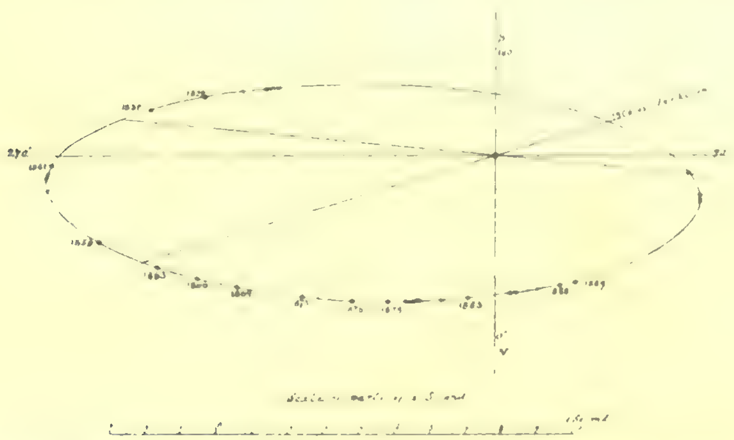


Fig. 469.—Diagram, by Mr. J. E. Gore, representing the apparent orbit of 228 Struve.

stars present a fine contrast of colour; but this is *never* the case when the two adjacent stars are nearly of equal magnitude, and it seems to be a universal law that, when there is a contrast of colour, the prevailing tint of the smaller star always lies higher in the spectrum than that of the larger star. Thus, if the larger star is orange or red, the smaller star is green or bluish.

The universal application of this law, which has been recognised since the time of Sir John Herschel, seems to indicate that it must have a physical cause, and that the smaller star of a pair must in all cases be either composed of different materials, or it must be in a different physical state from the larger star. We have no satisfactory evidence at present that the star which appears the brightest is actually the largest. Of two neighbouring stars of unequal mass, the larger may be so much less brilliant than its neighbour that it appears to us to be the fainter of the two. We should expect the smaller star to cool more rapidly than its larger neighbour; and, since one must assume both stars of a binary system to be of the same age, the smaller would be in the later stage of development, or furthest removed from the nebular condition.

Assuming for the moment that the fainter star of a binary pair is actually the smallest, and that the prevailing tint of a star passes upwards towards the blue end of the spectrum as it condenses from the nebular condition, the bluer tint of the smaller components of binaries would result; but such a theory does not enable us to account for the prevailing red and green tints of the larger components of binary systems. There is certainly a much larger proportion of red and green tints among the brighter components of binaries than among stars generally, or than among the stars which are believed to be single. This fact points to the conclusion that the binary connection of stars exercises an influence on the tints of both their components, and that the larger component, as well as the smaller, must either be in a different physical condition, or must be composed of different chemical elements from those which compose the majority of other stars.

In the earlier stages of the life history of a binary system there must be a long period during which the component stars are sufficiently near together for their coronas or atmospheres to be joined or merged into one another, while the main bodies of the stars, chiefly composed of less volatile constituents, are entirely separated. It is well known that the height of the atmosphere surrounding a body in space depends upon the temperature and weight of the body around which the atmosphere reposes. The temperature remaining unchanged, the smaller the attraction of the body enveloped by an atmosphere, the less will be its power of compressing the gases which envelope it, and the higher will its atmosphere extend. Thus, if the mass of the central attracting body were doubled, other conditions remaining unchanged, each stratum of its atmosphere will occupy half its former thickness, for the weight of the superincumbent gases which compress the stratum would be doubled.

In the case of two stars at the same temperature, one of four times the

mass of the other, the atmosphere enveloping the smaller star would extend to four times the height[1] of that enveloping its larger neighbour; and, if the atmospheres of the two stars were sufficiently extended to intermix, the gaseous envelope of the smaller star would extend far beyond the neutral region C, where gravitation towards either star is equal, and we should expect the gases constituting the upper atmosphere of the smaller body to flow towards and surround the larger body. It seems probable that only the more volatile elements extend into the upper atmosphere of a sunlike body.

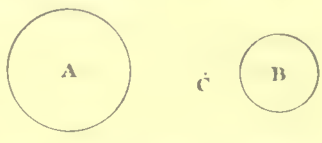


FIG. 470.—If A has four times the mass of B, AC = 2 CB.

[1] The height of an atmosphere surrounding a spherical body in space, and held to it by the force of gravitation, is independent of the amount of atmosphere; for if the total quantity of atmosphere be increased or diminished, other conditions remaining unaltered, the density of the lower half of the atmosphere will be increased or diminished in the same proportion; but the volume of the lower half of the atmosphere, and consequently its height, will not be altered, because the lower half is compressed by the weight of the upper half, and if the total amount of the atmosphere be doubled, the lower half will be under double the pressure; similarly, all other strata of the atmosphere will retain their original thickness, for their density will increase proportionately with the weight of the superincumbent gas.

In the case of our own Earth, the lower half of its atmosphere forms a stratum of about 3·44 miles thick. The air at a height of 3·44 miles has only half the density of the air at the sea level, because it is subjected to only half the pressure. On going up another 3·44 miles the density will be again halved, for every such layer of the upper stratum is under half the pressure of a similar layer of the stratum below, and with every rise of 3·44 miles the density will again be halved—the lower stratum of 3·44 miles thickness being under the pressure of the upper half of the atmosphere, the next being under the pressure of the upper one-fourth of the atmosphere, the next under the pressure of the upper one-eighth, and so on. Thus, at a height of seventy miles we should expect to find the density about one-millionth of the density at the sea-level, and at a height of about 140 miles the atmosphere would only have a density of about one-millionth of a millionth of the density at sea-level.

If the Earth's atmosphere were of hydrogen, it would extend to about 14½ times its present height, for a volume of atmospheric air weighs about 14½ times as much as an equal volume of hydrogen at the same temperature and pressure. If the temperature of an atmosphere is increased, its height will increase with the absolute temperature in the same manner as the volume of a gas increases with temperature. It should, however, be noted that the above laws only apply within the limits of temperature and pressure where Boyle's law holds good for the particular gases of which the atmosphere is composed. Thus, if the height of the lower half of an atmosphere be h at a temperature of 0° Cent., its height at a temperature of t° Cent. will be $\frac{t}{273} h$.

We may thus calculate the height of an atmosphere of hydrogen about the sun. The narrow character of the lines in the spectrum of the prominences seems to indicate that the gaseous pressure in the region where they exist is very small compared with the pressure of the Earth's atmosphere at the sea-level; but, assuming that the gaseous pressure at the level of the photosphere is equal to the pressure of our atmosphere at the sea-level, and assuming the height of the solar atmosphere to be the height at which the pressure is reduced to a millionth of a millionth of the pressure at the level of the photosphere, we have, with solar gravity at the level of the photosphere $27\frac{3}{4}$ times as great as terrestrial gravity, the height of a hydrogen atmosphere about the sun

at a temperature of 0° Cent. = 27 miles,
at a temperature of 2,457° Cent. = 270 miles,
at a temperature of 27,027° Cent. = 2,700 miles.

The strong polarisation of the coronal light renders it probable that the light of the corona is chiefly due to the sun's light dispersed by dust particles; and, if such particles consist of any elements with which we are familiar on the Earth, they would not remain in a solid or liquid form at a temperature of 27,000 degrees Centigrade. These facts point to the conclusion that the matter of the solar corona is carried by explosions to a much greater height than a solar atmosphere (consisting of any gases with which we are familiar) could extend in a state of equilibrium above the solar surface.

for those which condense from the gaseous state first would be precipitated in the cool outer atmosphere, and would fall in the solid or liquid state to lower levels, leaving the elements which retain their gaseous condition at a lower temperature to form the exterior parts of the gaseous envelope. It seems, therefore, probable that, during a long period in the history of a binary system, the more volatile elements rise into the outer atmosphere of the smaller star, and continue to drain from the smaller to the larger star. Thus, from elementary considerations we are led to expect that ultimately the chemical constitution of the two bodies would differ.

(1443.) If a prism be placed in front of a telescope so as to send the refracted rays from a star through the object-glass and down the telescope, an observer, looking in at the eye end, will see the image of the star spread out into a narrow line of light, which is blue at one end and red at the other. This narrow band, or spectrum, is not continuous, but is interrupted by small dark gaps, which are not very easy to see ; but, if the narrow spectral band of light be made to appear a little broader by looking at it with a cylindrical lens, the dark gaps become visible as lines across the spectrum.

This method of observing stellar spectra with a prism in front of the object-glass and a cylindrical lens was devised by Fraunhofer. In 1814 he examined the light of many of the larger stars in the Northern hemisphere, and noted the variations in their spectra. He found that the spectrum of Pollux closely resembled the spectrum of our sun, while there were recognisable differences in the spectra of Capella, Betelgeuse, and Procyon ; they were all crossed by narrow dark lines, some of which he identified with solar lines. Fraunhofer[1] also noticed that the spectra of Sirius and Castor were of a different type ; they were seen to be crossed by three massive dark bars, two in the blue and one in the green. After a lapse of forty-five years, Professor Kirchhoff of Heidelberg made an experiment which gave a clue to the meaning of these dark lines. He passed a beam of sunlight across a space occupied by burning sodium vapour, and perceived with astonishment that the dark Fraunhofer lines D, instead of being blotted out by the luminous rays of the same refrangibility as those given out by the flame, were rendered

[1] This remarkable man greatly improved the achromatic telescope. He succeeded, after many experiments, in making the first large achromatic object-glass of high quality and finish. It was secured by the elder Struve for the Russian Government, and was long known as 'the great Dorpat refractor,' though it was only 9½ inches in diameter, and was of 14 feet focal length. Fraunhofer discovered nearly a thousand lines in the solar spectrum, and mapped 576 of them, naming the principal ones by the letters of the alphabet. He recognised the double D line in many terrestrial spectra, and noted the identity of its place with the solar D lines, though the true interpretation of the coincidence was not recognised for more than forty years after he had laid such ample foundations for the deductions of modern spectrum analysis. After many ingenious experiments, he succeeded in making a diffraction grating, which showed him the lines in the normal spectrum.

blacker and thicker by the superposition. He tried the same experiment, substituting the continuous spectrum derived from the light of a Drummond lamp for sunlight ; but a dark line, corresponding in every respect to the solar double line D, was seen to cross the spectrum. The inference was irresistible that these dark lines were produced by absorption of light corresponding in wave-length to the bright lines given out by the vapour, and that there must be an absorbing layer of sodium vapour about the sun. This discovery was quickly followed up by Prof. Kirchhoff and his pupil, Dr. Bunsen, by a comparison in the laboratory of the bright lines of other incandescent vapours ; and many of the bright lines given out by such vapours were identified with dark lines in the solar and in stellar spectra.

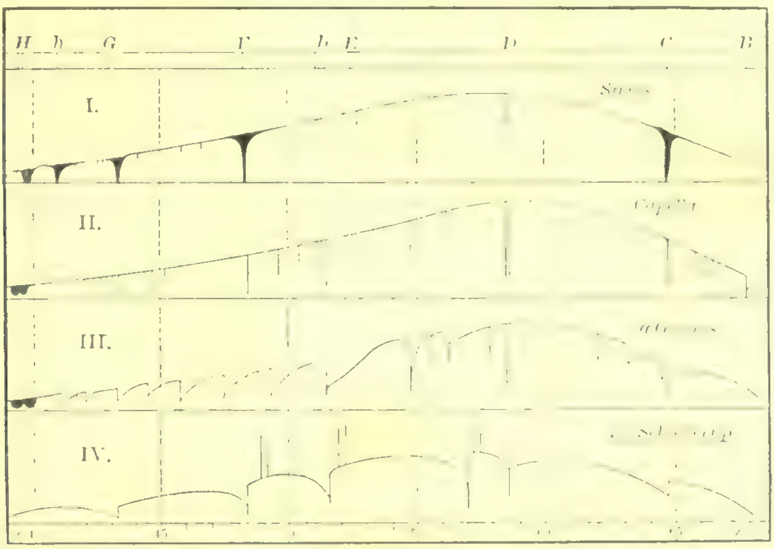


FIG. 471.—Secchi's types of stellar spectra, from a diagram by Prof. C. A. Young.

Soon after the publication of Kirchhoff's and Bunsen's results, Mr. Lewis M. Rutherfurd in New York, Donati in Florence, Secchi in Rome, and Huggins and Miller in England set themselves to the task of applying the new methods of research to the spectra of stars. Rutherfurd seems to have been the first to point out, in 1863, that the spectra of stars could be grouped into three[1] well-marked classes ; but he seems not to have been able to observe any lines in the spectra of α Virginis, Rigel, &c., and grouped them in a separate class. Father Secchi in 1863 undertook the labour of making an extended spectroscopic survey of the Northern heavens. Between the years 1863 and 1867 he examined the light of four thousand stars, and divided their spectra into four groups.[2]

[1] *Silliman's Journal*, vol. xxxv. p. 71.

[2] *Brit. Assoc. Report* for 1868, p. 166, and Padre A. Secchi's *Catalogo delle Stelle di cui si è determinato lo spettro luminoso*, Paris, 1867 ; *Spettri prismatici delle Stelle fisse*, Roma, 1868.

The first class contained more than half of the stars examined by Secchi. It comprised the white and bluish stars, in which the lines of the hydrogen spectrum present a very marked appearance. They appear broad and hazy, like the H and K lines in the solar spectrum ; other lines, when visible, are generally very narrow and faint.

Secchi's second class contained stars which exhibit a spectrum somewhat similar to that of our sun. H and K are broad, and the spectrum is crowded with narrow dark lines.

Secchi's third class included most of the red and variable stars. In addition to dark lines, their spectra show a number of dark absorption bands which are most sharply defined on the blue, or short wave-length side, and they fade away towards the red end of the spectrum.

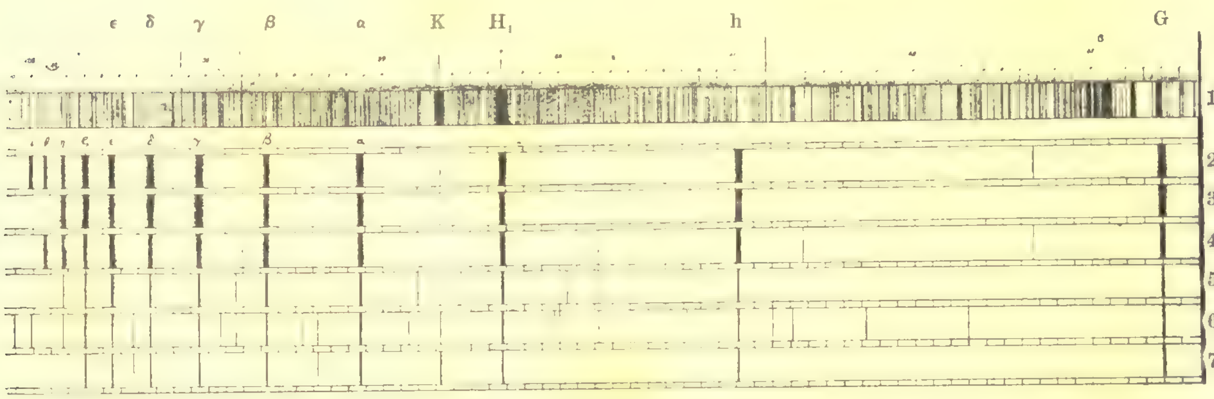


FIG. 472.—Diagram showing the relative intensity of the lines of the hydrogen spectrum in six stellar spectra. Photographed by Dr. Huggins. 1. Solar spectrum. 2. Spectrum of α Lyræ. 3. Sirius. 4. η Ursæ Majoris. 5. α Virginis. 6. α Aquilæ. 7. α Cygni. Copied from a paper on the Photographic Spectra of Stars in the 'Phil. Trans.' for 1880.

Secchi's fourth class contained a few small red stars with banded spectra, but the absorption bands are most sharply defined on the red, or long wave-length, side, and they fade away towards the blue end of the spectrum. Stars of this class frequently show a number of bright lines in their spectra.

(1444.) While Father Secchi's survey was in progress, Dr. Huggins, on August 29, 1864,[1] turned his spectroscope to a bright planetary nebula in the constellation Draco, and found that its light, instead of being spread out by the prism into a spectral band, was resolved into three monochromatic images, showing that its spectrum corresponded to the spectrum of incandescent, or glowing, gas at a low pressure. In the course of the next four years Dr. Huggins examined the spectra of about seventy nebulæ, and found that about one-third displayed a bright line spectrum, indicating their gaseous

[1] *Phil. Trans.* for 1864, vol. cliv. p. 437.

character.[1] Up to the present time about half the nebulæ which have been examined spectroscopically give a spectrum in which six or seven lines are fairly conspicuous on a more or less faint continuous background; the three brightest of these lines are situated in the green,[2] and they give to this class of nebula a distinct bottle-green tint, which enables an observer with a large telescope to recognise one of these gaseous nebulæ at sight as differing from a white nebula, such as the well-known nebula in Andromeda or the great spiral nebula in Canes Venatici, which give a continuous spectrum unmarked by any well-recognised[3] lines or bands. The brightest line in the nebular spectrum was at first identified by Dr. Huggins with the brightest line in the spectrum of nitrogen; but subsequent measures, with larger dispersion, showed him that the lines differed in character, and that, while the brightest nitrogen line is hazy and ill-defined at its edges, the nebular line is narrow and sharply defined, and is not absolutely coincident in position with the nitrogen line. In 1887 Mr. Lockyer announced that he had identified three of the lines in the nebular spectrum with two lines and with the edge of a fluting in the spectrum of magnesium; but more recent measures by Dr. Huggins and by Mr. Maunder[4] have shown that the coincidence is not satisfactory. In 1889 Dr. Huggins succeeded in obtaining a photograph showing some twenty-five sharply-defined lines in the spectrum of the great nebula in Orion, and in the spectrum of the stars of the trapezium involved in the nebula. At least four groups of bright lines in the spectra of these stars can be traced into the nebula for some little distance from the stars, indicating, as Dr. Huggins has pointed out,[5] that the stars of the trapezium 'are not merely optically connected with the nebula, but are physically bound up with it, and are probably condensed out of the gaseous matter of the nebula.' But up to the present time no one has succeeded in identifying any of the brighter nebular lines (with the exception of those that correspond to hydrogen) with the spectral lines of any terrestrial element. It is worthy of remark that the outer and cooler parts of the sun's corona, when examined

[1] *Phil. Trans.* for 1868, vol. clviii. p. 540.

[2] All the gaseous nebulæ give approximately the same type of spectrum. The brightest line is situated in the green region, at wave-length 5004·6 to 5004·8. The next in brightness is also in the green, at wave-length 4957·0, and the next in brightness is the well-known bluish-green line F of the hydrogen spectrum, at wave-length 4860·7. Then follow decidedly fainter lines—D_3 or 5874·0, $H\gamma$ or 4340·1 and 4476·0. The above are the positions of the nebula lines as determined by Dr. Copeland. Dr. Huggins gives the positions of thirty still fainter lines as determined from his photograph of the spectrum of the Orion nebula.

[3] Mr. A. Taylor observed two regions of maximum brightness in the green of the continuous spectrum of the Andromeda nebula (*Monthly Notices*, January 1889, p. 126), but his observation has not, as far as I am aware, been confirmed by other observers.

[4] See paper by Mr. Maunder in the first number of the *Journal of the British Astronomical Association*.

[5] See Paper by Dr. Huggins in the *Proceedings of the Royal Society* for 1889.

with the spectroscope, show a green line (Kirchhoff, 1474) which has not yet been identified with a line in the spectrum of any substance examined in terrestrial laboratories, or with any of the nebular lines.

The faint continuous spectrum of the white nebulæ is not crossed by any dark absorption lines such as we see in the spectra of the stars. If the light of these nebulæ were due to thinly-scattered faint stars, too small to be individually visible, we might expect their combined light to give a faint spectrum crossed by dark absorption-lines common to the spectra of the small stars. That such absorption-lines are not visible in the spectra of the white nebulæ is *primâ facie* evidence that they do not consist of sparsely distributed bodies similar in constitution to the brightest stars.

The nebulous background of the Milky Way is too faint to allow of its light being analysed with the spectroscope, but its whitish colour, as seen with the naked eye, seems to indicate that it does not belong to the type of nebula which gives the green-line spectrum. This conclusion must, however, be received with caution, for evidently a large proportion of the light which reaches us from the region of the Milky Way is derived from small stars.

The light emitted by nebulous masses, whether of the green or white type, appears to be merely a faint glow as compared with the brilliant light emitted by the photosphere of our sun or the photospheres of the stars. Thus, if we assume, with Prof. Pickering, that the stellar magnitude of our sun is $-25{\cdot}5$ (in other words, that the sun, as seen from the Earth, gives about forty thousand million times as much light as a star of the first magnitude), and if we neglect any absorption of light in space, our sun would need to be removed to a distance where its diameter would appear to subtend an angle of only $\cdot00964$ of a second of arc to reduce its light to equality with that of a star of the first magnitude. A nebula which subtends a minute in diameter, and gives the light of a star of the eighth magnitude, gives $\frac{1}{631}$ part of the light of a star of the first magnitude, while its diameter is more than six thousand times as great as the diameter which our sun would appear to have, at a distance where its light would be reduced so as to rank as a star of the first magnitude—that is (assuming there to be no absorption of light in space), the photosphere of our sun must be more than twenty-two thousand million times as bright, area for area, as such a nebula ; and, if we assume that half the light of the nebula is absorbed or lost on its way to us, the ratio between the brightness of the photosphere and the brightness of the nebula still remains more than ten thousand millions to one. According to Prof. Langley's experiment, referred to on p. 749, the sun's photosphere is 5,300 times brighter than the molten metal in a Bessemer 'converter ;' consequently such a bright planetary nebula as we have assumed would glow with

a light which corresponds to less than a millionth of the brightness of white-hot iron. Perhaps one might compare the brightness of such a nebula with the faint glow of phosphorus in air, or with the glow of the trail left by a large meteor on entering our atmosphere. The vapour driven off from the meteor is for an instant intensely luminous. It is probably at first under great pressure, and rapidly expands, driving back the cold upper air. In expanding it cools, till the greater part of the vapour is precipitated into a glowing mist, which, having no elasticity, and not being able to do work by driving back the surrounding air, can only cool by radiation. In this condition the trail from a large meteor is said sometimes to remain faintly glowing for half an hour.

(1445.) Zöllner seems to have originated the suggestion—which has been a very fascinating one to some minds—that stars of the first, or white (*Sirian*), type were the hottest and, therefore, the youngest, while the yellow colour of the stars of the second, or solar, type showed that they had advanced some way further in the process of cooling. Angström suggested that the shaded bands of the orange stars indicated the formation of compound bodies in their atmospheres, consequent on the lowering of temperature ; and Lockyer has carried this idea still further, assuming as an axiom that 'the hotter the star, the simpler its spectrum,' and 'the older a star, the more does free hydrogen disappear from its spectrum.' We must not, however, be misled by any supposed analogy between the change in tint of a cooling solid and a cooling mass of gas. Indeed, it seems theoretically probable that a mass of gas radiating into space and contracting under its own gravity would not cool, but would grow steadily hotter, while it remained in the gaseous state. Mr. Homer Lane's theory (referred to on p. 747, note) is a necessary consequence of the kinetic theory of gases, and those who accept the nebular hypothesis, and believe that stars have developed from nebulous masses similar to the faintly-glowing nebulæ we see, are forced to assume that there must have been a long period during which the contracting nebulous mass grew brighter and, also, probably hotter.

During the early history of a contracting nebula only its most volatile elements would be in the gaseous condition. After a time, the more refractory elements which remained in the solid or liquid state would commence to glow with a dull red tint ; as the heat increased, these refractory elements would glow with a whiter and more brilliant incandescence, and one element after another would be driven into a state of vapour until, as the heat increased in the central or hottest part of the nebula, platinum, carbon, and other refractory elements would all be driven into vapour. Just outside this hottest region we should expect to find a slightly cooler region, where carbon and the most refractory elements are not in a state of vapour, but exist in the

solid or liquid form, glowing with an intensely white incandescence, comparable with the brightness of the solar photosphere (see p. 749). From what we know of nebulous stars and nebulæ surrounding stars, such as the great nebula in Orion and the Pleiades nebula, it would seem that the matter within comparatively small regions of nebulæ may glow like a star[1] while still surrounded by an enormous envelope of nebular matter, and that the light from such brilliant regions of condensation is not entirely absorbed in passing through millions of millions of miles of surrounding nebulosity. In no case do we know of a large and brilliant region within a nebula with a surface shining like a photosphere and of sufficient diameter to present a recognisable disc when examined with the largest telescopes. If, therefore, these

[1] It cannot reasonably be supposed that all the nebulous stars are seen projected on a nebulous background. The theory of probabilities negatives such a series of coincidences, and there is an evident connection between the forms of the nebulous structures in the large nebulæ and the places of the stars. Thus, in the great Orion nebula, in the Pleiades nebula, and in some of the smaller nebulæ, we see nebulous structures which appear to spring from stars or groups of stars. The nebulous structures grow gradually fainter as the distance from the star from which they appear to spring increases; and in some instances the nebulous structures branch or divide in a direction away from the star in a manner which leaves no room for doubting that the seat of origin of the structure (that is, the place from which it was shot forth) must have been within the star. We may, therefore, feel certain that we receive the light from many stars after its passage through thousands of millions of miles of nebulous matter.

The extreme transparency of this matter compared with smoke or fog or our own clear atmosphere will be realised when we remember how the light of our sun is dimmed at sunset and sunrise by its passage through a few hundred miles of our dust-laden air, so that the eye can easily gaze on the sun's disc; and a photograph, which at midday can be obtained in a fraction of a second, takes at sunset or sunrise many seconds, or even some minutes, to give it a suitable exposure.

If half of the light of a star were absorbed in its passage through a hundred million miles of nebula, only a quarter of the light radiated by the star would get through two hundred million miles of similar nebulous matter, only an eighth part through three hundred million miles, and only about a millionth of the light would get through two thousand million miles of such nebulous matter. Such vast distances are small when measured on the scale on which nebulæ are built. A nebula four thousand million miles in diameter at the distance of α Centauri would only appear to have a diameter of a little more than half a minute—that is, a little less than thirty-three seconds of arc—while the great Orion nebula has a diameter of more than half a degree, and the Andromeda nebula has a still larger apparent diameter.

The very great transparency of the nebulæ renders it probable that they either contain very little opaque solid or liquid matter, or that the solid or liquid matter is aggregated into discrete masses with an average diameter of more than an inch if the density of the nebula, leaving out of account its gaseous constituents, is as much as one thousand millionth of the density of atmospheric air at the sea-level. For it may be easily shown that, if the Earth were broken into cubical masses an inch in diameter, which were distributed uniformly throughout a sphere with a diameter as large as the diameter of the Earth's orbit, and the sun were situated at the centre of such a spherical cluster of stones, only about half the light of the sun would emerge from the cluster. If the fragments were very irregular in shape, though of the same average weight as before, a larger proportion of the sun's light would be intercepted; and if the stones were broken into fragments as large as sea-sand, a planet outside such a cluster would revolve in practical darkness. The density of such a spherical cluster, if its mass were equal to the mass of the Earth, would be about equivalent to the average density of the solar nebula (if it was of spherical shape) when its diameter was sixty-nine astronomical units, or a little more than twice the diameter of the orbit of Neptune—in other words, when the density of the solar mass was nearly three thousand millionths of the density of atmospheric air at the sea-level. Compare this with what is said in the text as to the density of nebulæ.

nebulæ are condensing into stars, it would seem that great condensation must take place before the nebular matter becomes hot enough to glow like a star.

We are able in a rather rough way to fix a superior limit for the density of the great nebulous masses we observe in the heavens. If we suppose the Orion nebula to be a uniform sphere of only a third of a degree (20′) in diameter, with an average density of only one-millionth of our atmosphere at the sea-level, the mass of the nebula, if it were only at the distance of α Centauri, would be over four and a half million (4,542,000) times the sun's mass. Such a mass would be capable of giving our sun a velocity of a little over 106 miles per second, if it had fallen towards the nebula from a condition of rest at an infinite distance to a distance from the nebula equal to the distance of α Centauri. And the velocity of our sun in a circular orbit about such a nebula, situated at a distance equal to the distance of α Centauri, would be 74·99 miles per second.

If the nebula contained such a vast mass of attracting matter, we should expect to see many stars in the neighbourhood of the nebula moving across the line of sight with very large proper motions, for a velocity of 100 miles a second across the line of sight at the distance of α Centauri would give an annual proper motion of 25·5″—that is, a proper motion more than three times as great as that of 1830 Groombridge. But instead of finding the stars in the immediate neighbourhood of the Orion nebula exhibiting large proper motions, we find the stars in this region of the heavens which appear to be associated with the Orion nebula show hardly any detectable proper motion; and the same remark applies to all the large stars which seem to be connected with the stream of nebulæ which appear to link up the great Orion nebula with the Milky Way (see p. 736). The stars of the Pleiades group, which also appear to be connected with the Milky Way, and to be surrounded by a very extensive nebula, also exhibit only small annual proper motions;[1] and the same remark applies to the nebulous stars α and γ Cygni, which appear to be associated with the nebulosity of the Milky Way.[2]

If, instead of assuming the distance of the Orion nebula to be equal to the distance of α Centauri, we had assumed its distance to be double as great, the velocities referred to above, of our sun falling from an infinite distance, or in a circular orbit about the nebula, would need to be doubled. For at twice the distance a nebula subtending the same angular diameter would occupy eight times the volume, and, the density remaining the same, its mass would be eight times as great, and the periodic time in a circular orbit about such a nebula at double distance would be unchanged, therefore the velocity in the larger orbit would be doubled.

[1] See *Knowledge* for May 1891. [2] See *Knowledge* for October 1891.

That the periodic time in any orbit about such a nebula is independent of the distance, if the angular subtense and density of the nebula remain unaltered, will be evident when it is remembered that the square of the periodic time, in any orbit, is inversely proportional to the attracting mass. Therefore, if the central attracting mass be multiplied by eight, the size of the orbit remaining unaltered, the periodic time will be reduced in the proportion of 1 to $\frac{1}{2\sqrt{2}}$. On the other hand, it is evident from Kepler's law connecting the squares of the times with the cubes of the distances that, if the attracting mass remains unaltered and the size of the orbit be doubled, the periodic time will be increased in the ratio of 1 to $2\sqrt{2}$; therefore, if the size of the orbit be doubled, and the attracting mass be multiplied by eight, the periodic time will remain unaltered,[1] and the velocity in the orbit will be doubled if we double the distance of the nebula.

Thus, if we suppose the Orion nebula to be at double the distance of α Centauri, and the density and angular subtense to remain as we have assumed above, the velocity of the sun in a circular orbit about it would be 150 miles a second, and so on, the velocity increasing directly as the distance of the nebula is increased. In other words, the apparent proper motion of the sun as seen from the nebula, or of stars in the neighbourhood of the nebula as seen from the earth, would remain unchanged, unless the mean density of the nebula were altered.

The small average proper motions of stars in the neighbourhood of the Orion nebula entitle us, therefore, to assert with some confidence that the average density of the Orion nebula cannot exceed one ten thousand millionth of the density of atmospheric air at the sea-level. This would about correspond to the mean density of the solar nebulous mass, supposing it to have been spherical when its radius was a little more than 107 astronomical units, or when the sun occupied a sphere with a radius of a little more than $3\frac{1}{2}$ times the distance of Neptune. Thus we might, from the evidence of proper motion alone, have concluded that the Orion nebula is less dense than the solar nebula was at the epoch when the birth of the planets commenced.

In examining the forms of nebulæ, we find comparatively few oblate spheroids, such as the hypothesis of La Place assumes. There are many apparently spherical masses, a few spirals and rings, and a great many nebulous masses of irregular form. If the stars we see are of very different ages, and the nebular stage of condensation occupies, as has hitherto been supposed, a very long period compared with the stellar stage, we should expect to see

[1] The above reasoning only holds good when the mass of the sun may be neglected as compared with the mass of the nebula.

a far greater number of nebular masses than of fully-formed stars ; but the number of brightly shining stellar points greatly exceeds the number of nebular masses hitherto discovered. Probably we have hitherto been mistaken in supposing that the faintly shining nebular masses we observe afford ocular evidence of the truth of La Place's hypothesis. The nebulæ we see have, it seems, a greater analogy with the solar corona than with the fiery condensing mists conceived of by La Place ; they are very generally associated with stars, and in some cases the nebulous structure clearly indicates that the nebulous matter has issued from the star, and sometimes from a starless region. The forms of such nebulæ rather indicate a violent series of explosions or outbursts from stellar centres than the slow and quiet condensation under the influence of gravity conceived of by La Place.

(1446.) Many facts point to the conclusion that stars exhibiting the first, or Sirian, type of spectrum are less condensed, and are in an earlier stage of development than those yielding the second, or solar, type of spectrum. Thus the stars involved in the Orion nebula, which exhibit the bright lines common to the nebula around them (see p. 789), have a spectrum which must be classed as of the first type. The Pleiades stars and the nebulous star α Cygni also have spectra of the first type. Further, the Algol variables, which, we may infer, are binary systems in an early stage of development, appear all to exhibit spectra of the first type. The 'spectroscopic doubles,' which may be described as Algol type stars in which both components are bright and the plane of revolution does not pass through the Earth, seem also all to be of the first type. On the other hand, the evidence afforded by the colours of double stars seems to point to the fact that difference of type of spectrum does not indicate difference of age, but rather a difference of constitution. If both components of a double star have the same origin, and were formed at the same time, we should expect the smaller component to enter the second stage of development at an earlier date than the larger component, and we should expect, if the types of spectrum corresponded to stages of development, to see many instances of Sirian primaries with solar companions ; instead of which the light of the smaller star is always bluer than that of the large star ; so that, judging by the tint, one would class the small star as having a spectrum of the first type, while the larger star would be classed as of the second or third type.[1] Again, it is found that certain

[1] In most instances it is found impossible to photograph separately, or even to distinguish with the eye, the spectra of the components of double stars; as a general rule they are too close for such separate examination. In the 'Draper Catalogue' Prof. Pickering classifies the components of β Cygni as belonging, the one to his group A, and the other to his anomalous group Q; and in the 'Discussion of the Draper Catalogue,' vol. xxvi. of the *Harvard Annals*, Prof. Pickering says (Introduction, p. xix): 'In many known binaries the brighter component is of a yellowish tint, the

regions of the heavens are rich in stars having similar spectra. Thus, the Wolf-Rayet type of bright-line stars cluster in certain parts of the Milky Way. Red stars and stars of the Orion type also cluster in the heavens, forming, evidently, real groups in space. These groups include stars of various magnitudes, and it cannot reasonably be supposed that the smaller stars are just so much younger than the larger that they happen at the present epoch to have all cooled to the same extent, and to be in the same period of development. It seems more reasonable to assume that they form a natural group of stars, all composed of similar materials in similar proportions. The evidence of the spectroscope teaches us that some of the terrestrial elements exist in the sun and in the stars; but this does not give us ground for concluding that the materials of the universe have been uniformly mixed, or that, having been uniformly distributed at one time, there has since been no process of selective aggregation, which has ultimately distributed the elements in various proportions amongst the bodies and groups of bodies into which the materials of the universe have condensed.

(1447.) There is, however, some ground for supposing that stars exhibiting the first, or Sirian, type of spectrum are, as a general rule, less dense than stars exhibiting the second, or solar, type of spectrum. We are able to assert that, if the Sirian binary stars are not less dense, their photospheres must, as a general rule, be brighter, area for area, than the photospheres of solar binary stars.[1]

The following table is founded on the data given in Mr. J. E. Gore's 'Catalogue of Binary Stars,' published, in 1891, in the 'Proceedings of the Royal Irish Academy,' 3rd ser. vol. i. No. 4, on the assumption that the instrinsic brightness of the photospheres of all stars is the same :—

TABLE SHOWING RELATIVE DENSITIES OF SIRIAN AND SOLAR BINARIES

Sirian Stars

OΣ. 4	0·101	ω Leonis	0·022	ξ Scorpii	0·013
OΣ. 20	0·006	φ Ursæ Maj.	0·002	γ Ophiuchi	0·001
14 Orionis	0·022	γ Virginis	0·017	μ Draconis	0·021
12 Lyncis	0·003	25 Can. Ven.	0·017	ζ Sagittarii	0·002
Sirius	0·011	η Coronæ Bor.	0·109	δ Cygni	0·001
Castor	0·001	μ^2 Boötis	0·040	β Delphini	0·010
ζ Cancri	0·037	γ Coronæ Bor.	0·002	λ Cygni	0·005

fainter of a bluish tint. The spectroscope shows that the spectra are of the second and first types respectively. If the components were too close to be separated, they should still show the lines of both types—that is, a spectrum like that of our sun, but in which the hydrogen lines are strongly marked.'

[1] According to a table given by Mr. Maunder in *Knowledge* for June 1891, p. 101, founded on the materials given in Mr. Gore's Catalogue, and on the assumption that Sirian and solar stars are, on the average, of similar density, the mean brightness of the photospheres of Sirian binaries is more than five times as great as the mean

TABLE SHOWING RELATIVE DENSITIES OF SIRIAN AND SOLAR BINARIES (*continued*)

Solar Stars

Σ. 3062	0·323	Σ. 1757	0·803	τ Ophiuchi	0·009
η Cassiopeiæ	0·956	Σ. 1819	0·196	70 Ophiuchi	1·103
36 Andromedæ	0·012	α Centauri	0·121	γ Coronæ Aust.	0·134
Σ. 228	0·143	γ Leonis	0·0002	OΣ. 387	0·047
OΣ. 149	0·164	ξ Boötis	0·914	OΣ. 400	0·034
Σ. 1037	0·073	44 Boötis	0·061	4 Aquarii	0·026
Σ. 3121	1·882	OΣ. 298	0·600	τ Cygni	0·020
ξ Ursæ Maj.	0·181	α Coronæ Bor.	0·077	π Cephei	0·005
OΣ. 234	0·075	ζ Herculis	0·018	The Sun	1·000
OΣ. 235	0·059	Σ. 2107	0·079		
42 Comæ	0·047	Σ. 1273	0·219		

The above table gives 0·3094 as the mean density of the binary stars whose combined light gives a spectrum of the solar type, and 0·021 as the mean density of the binaries whose combined light gives a Sirian type of spectrum —that is, solar binaries are, on the average, about fifteen times as dense as Sirian binaries. There is, however, one striking exception [1] to this rule; γ Leonis, which appears from the table to be the least dense of all the binaries whose orbits are satisfactorily known, is a solar and not a Sirian binary. Prof.

brightness of solar binaries. Mr. Maunder remarks that, on this supposition as to equal density, one-third of the Sirian binaries are brighter than the brightest solar binary, and one-half of the solar binaries are fainter than the faintest Sirian binary. As to the relative probability of the two assumptions—viz. equality of density and equality of brightness (see p. 748).

[1] The exceptional character of γ Leonis cannot be explained by assuming that its light is diminished by nebulosity around the star, or by absorption of the light on its way to us, for this would require us to assume a still greater brightness for the star compared with the sun than that referred to on p. 779. If we adopt the theory that the photospheres of the sun and γ Leonis are of equal brightness, we must assume that the surface of the photosphere of γ Leonis has a far greater extent relatively to the mass of the star than the solar photosphere has as compared with the sun's mass. The difference is so great that it can hardly be accounted for by assuming that each of the components of γ Leonis is a cluster of small stars, for one would need to assume that each of the components is a cluster composed of many million stars. Thus, if a spherical star were broken up into eight spherical stars of similar density and surface-brightness, the eight small stars would—if they did not eclipse one another—give double the light of the large star. If the large star were broken up into sixty-four small stars of similar brightness and density, arranged so as not to eclipse one another, the sixty-four small stars would give four times as much light as the parent star. But in order to account for a surface-brightness 300 times greater than that of the sun, we should need to assume that the parent star was broken up into nearly seventeen and a half million smaller stars, arranged so as not to eclipse one another. Apparently the only other alternative left us is to assume an exceptionally small density for this star, which would give it a relative diameter about seventeen and a half times (17·37) as great as that of the sun. That is, we must assume that this star is in the condition that our sun was, according to the ordinary assumptions of the nebular hypothesis, long after the birth of Mercury, when its diameter (if the sun was spherical) was fifteen million miles—in other words, when its diameter was a little more than a fifth of the diameter of the orbit of Mercury. There is probably not an error of 50 per cent. in any of the data used for the above calculations. Thus, the stellar magnitude of the components of γ Leonis is known, with a probable error of certainly less than 50 per cent. Hind, in 1852, determined the period of γ Leonis as 296 years; and Doberck, in 1879, determined it as 407 years. The components of γ Leonis have now described an arc of more than 40° about one another since the date of the first measures used. The star is an easy double, the components being now nearly three and a half seconds apart, so that it cannot be supposed that there is an error of 50 per cent. in the determination of the mean distance between the components.

Pickering gives its spectrum as belonging to his Class K, which ranks as somewhat intermediate between the solar type and Secchi's third type of spectrum. It will be noticed that, according to this table, there is a much greater range of densities among the solar than amongst the Sirian binaries, as if the class of stars which we rank as solar included a greater variety of types of stars than the Sirian class—that is, as if the solar class included stars having a greater diversity of life histories and origins than the Sirian class.

(1448.) If we examine the class of stars which show large proper motions, we find that these stars must in general be classed as exhibiting the second or solar type of spectrum. Prof. Pickering has expended some years in making a catalogue of the spectra of over ten thousand of the brighter stars in the northern heavens. It is published under the title of the 'Draper Catalogue,'[1] in vol. xxvii. of the 'Annals of Harvard College Observatory,' and includes all the naked eye stars to the north of $-25°$ of declination, with many of smaller magnitude, though, as Prof. Pickering remarks, the classification of the spectra of stars below the $6\frac{1}{2}$ magnitude must not be relied upon. Prof. Pickering divides Secchi's first type into four classes, which he designates by the letters A, B, C, and D. Secchi's second type he divides into seven subdivisions, which he designates by the letters E, F, G, H, I, K, and L. He denotes the spectra of Secchi's third type by the letter M. The following table shows the type of spectrum of fifty-one stars having large proper motion. They follow in the order of Bossert's list (see p. 752), but many of the stars given by Bossert are not to be found in the Draper Catalogue, as they are either to the south of declination $-25°$, or are too small to be satisfactorily classed. Prof. Pickering remarks that it was difficult for him to classify with certainty the spectra of stars below the 6·5 magnitude of the photometric scale.[2] In other cases of doubt he has added a ? after the letter denoting the class of spectrum exhibited. I have included α Centauri in the list, although it is not within the region comprised in the Draper Catalogue. Its spectrum is classified as solar on the authority of Mr. Gore. It will be seen that out of the fifty-one most swiftly moving stars whose spectra we have been able to classify, only five are ranked by Prof. Pickering as belonging to the A or Sirian type, and of these he has marked three, including Sirius, as doubtful or differing in some way from the ordinary spectrum of the A type. The other two stars which he has ranked as of the A type are both small (6·53 mag. and 6·31 mag.), and near to the

[1] This important Catalogue has been made from a series of 633 photographic plates, taken with a Voigtlander camera of 44 inches focus, and a glass prism with a refracting angle of 13° placed in front of the lens, so that the stellar images thrown upon the plate are drawn out into spectra.

[2] See p. iv. of the Preface of the *Draper Catalogue*, vol. xxvii. of the *Annals of Harvard College Observatory*.

limiting magnitude beyond which Prof. Pickering states it is difficult to classify stellar spectra with certainty. It is therefore somewhat doubtful

LIST SHOWING THE TYPE OF SPECTRUM EXHIBITED BY FIFTY-ONE STARS HAVING LARGE PROPER MOTIONS

Star	R.A. 1900	Declination 1900	Photometric Magnitude	Annual Proper motion	Spectrum	
					Secchi	Pickering
	h. m.	° '				
1830 Groomb.	11 44·5	+35 29	5·95	7·05	II	F
61 Cygni (double)	21 2·1	+38 13	6·23	5·14	II	H
O² Eridani (triple)	4 7	− 7 6	4·52	4·05	II	F ?
μ Cassiopeiæ	1 1·2	+54 27	5·64	3·73	II	H
α Centauri (binary)	14 32	−60 22	1	3·62	according to Gore, 2nd type	
Arcturus	14 11·1	+19 44	B	2·28	II	K
123 Piazzi II.	2 30·8	+ 6 0	6·09	2·19	II	E
3077 Bradley	23 8	+56 36	6·09	2·08	II	H
τ Ceti	1 39·5	−16 28	4·63	1·96	II	G ?
σ Draconis	19 32·5	+69 31	5·50	1·84	II	I ?
61 Verginis	13 13·2	−17 45	5·60	1·52	II	H
1643 Fedorenco	10 4	+50 0	6·64	1·43	II	H
147 Lac.	0 32·1	−25 19	5·95	1·41	II	F
189 Piazzi (0ʰ)	0 43·1	+ 4 46	6·14	1·32	II	H ?
Sirius (binary)	6 40·7	−16 34	B	1·32	I ?	A ?
γ Serpenti	15 51·8	+16 0	4·54	1·32	II	F
27744 Lal.	15 8·9	− 0 58	6·46	1·31	II	H
ι Persei	3 1·8	+49 14	4·88	1·30	II	F
85 Pegasi (binary)	23 59·8	+26 6	6·60	1·29	II	H
Procyon	7 34·1	+ 5 30	B	1·26	II	F
36 Ophiuchi (double)	17 9·2	−26 27	5·32	1·26	II	I
43 Comæ	13 7·3	+28 22	5·16	1·20	II	F
η Cassiopeiæ (binary)	0 43·0	+57 17	4·73	1·19	II	F
δ Trianguli	2 10·8	+33 46	5·63	1·14	II	F
1189 Weisse	4 57·1	− 5 39	6·53	1·14	I	A
70 Ophiuchus (binary)	18 0·4	+ 2 32	5·07	1·13	II	K
θ Ursæ Majoris (binary)	9 26·2	+52 8	4·14	1·12	II	F
72 Herculis	17 17·1	+32 47	6·54	1·04	II	H
16304 Lal.	8 13·7	−12 18	5·88	1·01	II	E
ε Eridani	3 28·2	− 9 48	4·83	0·99	II	K
1646 Groomb.	10 21·9	+49 21	6·50	0·97	I ?	A ?
b Aquilæ	19 20·2	+11 43	6·02	0·97	II	H
241 Weisse II.	13 14·5	+35 39	6·31	0·96	I	A
127 Piazzi XIV.	14 31·7	−11 53	6·48	0·91	II	E
η Serpenti	18 16·1	− 2 56	4·62	0·91	II	K
38380 Lal.	19 59·5	+29 38	6·42	0·91	II	H
υ Andromediæ	1 35·7	+42 7	5·68	0·89	II	F
λ Aurigæ	5 12·1	+40 2	5·24	0·85	II	F
43492 Lal.	22 12·2	+12 23	6·57	0·84	I ?	A ?
μ Herculis	17 42·6	+27 48	4·39	0·82	II	I ?
η Cephei	20 43·2	+61 26	4·88	0·82	II	K
15950 Lal.	8 5·5	+32 49	6·84	0·81	II	E
ρ Coronæ Bor.	15 57·2	+33 37	6·13	0·81	II	F
1045 Lal.	0 35·8	+39 36	6·64	0·80	II	E
83 Leonis	11 21·7	+ 3 34	6·28	0·79	II	I
γ Herculis	15 49·2	+42 43	5·32	0·79	II	F
β Canes Venatici	12 29·0	+41 53	5·21	0·78	II	F
β Verginis	11 45·5	+ 2 20	4·40	0·77	II	G
49 Libri	15 54·7	−16 12	5·52	0·77	II	F ?
11 Leo. Min.	9 29·7	+36 19	6·17	0·76	II	H
δ Eridani	3 38·5	−10 6	4·81	0·75	II	I

whether any of the swiftly-moving stars have spectra of the A type; but admitting that these five stars all belong to the A type, we should have less

than 10 per cent. of the swiftly-moving stars giving spectra of the A type; whilst amongst the larger stars distributed over the heavens about 61 per cent.[1] may, according to Prof. Pickering, be ranked as having spectra of the A type. This can be no mere chance coincidence, and it alone proves that the swiftly-moving stars have either a different origin or a different history from the stars of the Milky Way system, to which, as we have seen, a large proportion of the lucid stars apparently belong, for they are arranged conformably with the Milky Way or with a belt of stars associated with it.

Of the remaining forty-seven stars in our list, 14 are classified as belonging to Prof. Pickering's class F, 2 as F ?, 11 as H, 1 as H ?, 5 as K, 5 as E, 1 as G, 1 as G ?, 2 as I, 2 as I ?, and α Centauri is not contained in the 'Draper Catalogue.' In other words, about 32 per cent. of the swiftly-moving stars probably fall into the class F, 24 per cent. into the class H, 10 per cent. into the class K, 10 per cent. into the class E, 4 per cent. into the class I, and 2 per cent. into the class G. Whereas, according to Prof. Pickering, 96 per cent. of the brighter stars belong to the classes A, F, G, and K, and are distributed in the following proportions—viz. A, 61 per cent.; F, 12 per cent.; G, 5 per cent.; and K, 18 per cent. Of the remaining 4 per cent. a half, or 2 per cent., belong to the class B; 1·3 per cent. belong to the class M, which corresponds to Secchi's third type; and the remaining ·7 per cent. contains the stars whose spectra have been classified in the Draper Catalogue as peculiar.

(1449.) Prof. Pickering has divided the northern heavens into a number of equal areas, and has counted the number of stars of the various types given in the 'Draper Catalogue' within each of the equal areas. In summing up his discussion he says:[2] 'It appears that the number of stars of the second and third type is nearly the same in the Milky Way as in other parts of the sky. Considering, therefore, only the stars whose spectra resemble that of our sun, we find them nearly equally distributed in the sky.' The stars of Class A, on the other hand, are twice as numerous in the region of the Milky Way as in the rest of the sky, and in Class B this ratio exceeds four. 'The Milky Way is therefore due to an aggregation of stars of the first type, a class to which our sun seems to bear no resemblance as regards its spectrum.'

The Milky Way is not, however, a uniform cluster of stars of the first type. In some regions it is rich in red stars, in others there are many stars showing the Orion type of spectrum. Each region of the Milky Way seems to have a character of its own. In some parts the stellar aggregation is coarser than

[1] See *Annals of Harvard College Observatory*, vol. xxvi. p. 151.

[2] See 'Discussion of the Draper Catalogue,' *Annals of Harvard College Observatory*, vol. xxvi. p. 152.

in others. In some the stars appear upon a black background, while in others the background is nebulous. Stars of the Wolf-Rayet type, with spectra which consist mainly of bright lines, seem to be distributed along the central region of the Milky Way. Prof. Pickering, who has discovered nineteen of them, says:[1] 'Only fourteen stars of this class were previously known. They appear to fall very nearly in the central line of the Milky Way, and this fact served to detect several not previously known.' Stars of Prof. Pickering's Class B are also evidently associated with the Milky Way, as well as variables of short period. The intimate association that is possible between blue stars and red, orange, and green stars is illustrated by many of the coloured double stars, as well as by a curious group of blue, red, and orange stars about κ Crucis.[2]

Since stars of various types are evidently associated with the Milky Way, and some types of stars have a tendency to cluster in groups, it might be suggested that our sun belongs to a cluster of solar type stars situated near to the centre of the Milky Way system; and it would no doubt be urged by anyone adopting such a theory that the fact that stars having large proper motion generally exhibit the solar type of spectrum is a new piece of evidence tending to confirm Dr. Gould's assumption as to the existence of a solar cluster (see pp. 714 and 749). Large proper motion no doubt affords a rough criterion of distance, for the apparent angular motion of a star must decrease as the distance from which the motion is observed increases. But it is impossible to suppose that a cluster of such swiftly-moving stars could be permanently held together by the mutual attractions of the stars[3] we see (p. 763), and we are forced to accept one of two conclusions—either our sun and the swiftly-moving solar stars form a cluster which is held together by the attraction of some dark body or bodies of enormous mass compared with our sun and the other luminous stars whose mass we have been able to

[1] *Annals of Harvard College Observatory*, vol. xxvi., Introduction, p. xiv.

[2] See the *Monthly Notices*, December 1872, p. 66.

[3] Gould estimated that his cluster contained some 400 stars, ranging from the 1st to the 4½ magnitude (750–51). If we suppose each of these to be equal in mass to our sun, and the whole attracting mass of the cluster to act as if the stars were condensed into one attracting mass at a distance equal to the distance of α Centauri from our sun, such an attracting body would only be capable of giving a velocity of a little less than one mile (more exactly ·9963 of a mile) per second to our sun if it had fallen towards it from rest at an infinite distance; and the velocity in a circular orbit of our sun about such an attracting body at the distance of α Centauri would be only about seven-tenths of a mile per second. If we suppose each of the stars of the cluster to have ten times the mass of our sun, the attracting mass of the cluster would be only sufficient to give a velocity of 3·15 miles per second to our sun in falling towards it from an infinite distance; and our sun's velocity in a circular orbit about such an attracting mass at the distance of α Centauri would be only 2·228 miles per second. Such a cluster would, therefore, entirely fail to control the motion which our sun is known to have in space, and many of the swiftly-moving stars must be flying through space much more rapidly than our sun.

estimate, or they are strangers to the galactic system, and will in a time which is comparatively short compared with geologic epochs pass across the galactic system and out into space beyond.

A dark attracting mass capable of controlling the sun's motion in space, and as it were anchoring the swiftly-moving stars to the central region of the galactic system, might, if it were as dense as our sun, easily escape observation. At a distance equal to that of α Centauri it would certainly not be observed by the light reflected from our sun (p. 681, note), and its angular diameter would be too small to render its presence obvious by the frequent eclipse of more distant stars. If we assume the velocity of 1830 Groombridge to be 100 miles a second (see p. 762), and its distance from the dark attracting body to be equal to twice the distance of α Centauri from our sun, an attracting mass equal to 16,113,000 times the mass of our sun would be sufficient to retain 1830 Groombridge moving about it in a circular orbit.[1] Such a mass, having a density equal to that of our sun, would, if as near to us as α Centauri, only have an angular diameter a little over one second and three-quarters.

But if there were such a dark attracting mass controlling the motions of a group of swiftly-moving stars in the sun's neighbourhood, we should expect the existence of the attracting central body to reveal itself in a very unmistakable manner, by the greater average rapidity of the proper motions in the neighbourhood of the attracting body, and by the relatively sluggish proper motions of the solar stars in the opposite hemisphere ; but the stars exhibiting the largest proper motions are not grouped together in any region of the heavens. Thus 1830 Groombridge, 61 Cygni, and ε Indi are situated in widely separated parts of the heavens, and there can be little doubt that they are each of them moving through space faster than the sun.

	R.A.	Dec.	Annual Proper Motion
	h. m.	° ′	″
1830 Groombridge	11 46	+38 34	7·05
61 Cygni	21 1	+38 12	5·20
ε Indi	21 55	−57 14	4·60

It is evident that a cluster at the centre of the Milky Way could not be held together by the attraction of a ring or stream of stars or nebulous matter surrounding it. We are compelled, therefore, to abandon the idea that the swiftly-moving solar stars form a permanent cluster or system occupying the central region of the Milky Way. And it cannot be supposed that the stars near to us are moving in closed orbits about different parts of the Milky Way after

[1] This is on the assumption that the action of gravity extends to stellar distances. The facts observed with regard to the motions of binary stars have not *proved* this, though they have rendered the assumption very probable.

the manner of the particles in a smoke ring; for we should then expect to observe a symmetry in their motions with regard to the general plane of the Milky Way which we do not find.[1] Added to this the cleft character of the Milky Way, as well as its outlying streams and associated zone of large stars, seem to negative the idea that the stars and nebulous matter of the galactic system can be in motion like the parts of a vortex ring; though the general symmetry of the whole structure leaves no room to doubt that it forms a system,[2] and that the velocity of its parts cannot be sufficient to carry them

[1] Our sun and 61 Cygni appear to be moving in directions which are but slightly inclined to the general plane of the Milky Way, while the direction of motion of 1830 Groombridge, as seen in projection, is nearly perpendicular to the plane of the Milky Way.

[2] The existence of a stream or group of stars too numerous to be conceived of as only apparently in proximity to one another, implies the existence of an actual cluster or system of stars, which must be either a permanent system, or the stars must be separating and their proximity must be taken as evidence of a comparatively recent cataclysm, such as the collision of oppositely moving masses, or an explosion. It will hardly be contended that such a stream of stars as the Milky Way has been distributed in space by collisions or explosions all along the axis of the stream; and if the stream is not the result of a comparatively recent cataclysm or series of cataclysms the stars of the Milky Way must form a permanent system, and it follows that they cannot have velocities, relatively to one another, which would carry them entirely away from the system, though the centre of gravity of the whole system may have any velocity in space.

Mr. Monck has suggested (*Astron. and Astro-Physics*, October 1892) that, since Sirian stars give on the average more light than solar stars of the same mass, Sirian stars would be visible at distances where corresponding solar stars are invisible with the same instruments, and that, consequently, if the Milky Way is distant, we should expect it to appear to consist chiefly of Sirian stars. But if the Milky Way were a cluster of stars exhibiting the same types of spectra as prevail in other parts of the heavens, we should expect to find no type of stellar spectrum uniformly distributed over the heavens, and stars of the solar type would be more thickly grouped in the region of the Milky Way, in the same proportion as stars of the Sirian type; for if as seems probable (see the next note)—stars of the Sirian type give on the average about six times as much light as solar stars of the same mass, they would appear about two photometric magnitudes brighter than a neighbouring solar star of similar mass, from whatever distance they were observed; and the number of Sirian stars of any apparent magnitude would correspond with the number of solar stars two magnitudes fainter whatever the distance of the stars, or however sparsely or thickly they may be aggregated, provided the proportion of solar and Sirian stars remains the same, and the number of stars of different masses remains in the same proportion. But, according to Prof. Pickering, the number of stars of the second and third type is nearly the same in the Milky Way as in other parts of the sky.

The fact that the stars of the Milky Way differ in type from the stars distributed with apparent uniformity over the rest of the heavens may, therefore, be taken as strong corroborative evidence tending to prove that the stars of the Milky Way form a permanent system, having a different origin or history from the stars distributed with apparent uniformity over the rest of space. Mr. Monck's chief reason for doubting this seems to be that on examining a list of the proper motions of fifty-one large stars in the line of sight, recently published by Dr. Vogel in the *Monthly Notices* for June 1892, he finds that the average velocity of the thirty-one Sirian stars of the list, does not differ greatly from the average velocity in the line of sight of the remaining twenty solar stars. Up to the present time there have been very serious differences in the estimates made by experienced observers of the amount and direction of motion of stars in the line of sight; but assuming that Dr. Vogel has made a great advance on the accuracy of his predecessors, it would follow that there are wandering stars (that is, stars which do not belong to the galactic system) of the Sirian as well as of the solar type, and that the evidence derived from spectroscopic measures of motion in the line of sight is more conclusive as to this than the evidence derived from proper motions across the line of sight. In dealing with Dr. Vogel's measures it should be remembered that the sun's own motion in space will affect the motion in the line of sight of stars near to the apex and anti-apex of the sun's way;

away and permanently separate them from the system. But the solar stars are not symmetrically arranged with respect to the Milky Way, and the above considerations seem to show that they are not permanently connected with the galactic system.

The evidence collected at pp. 758–759 with regard to the direction of the sun's motion in space seems to render it probable that the Milky Way system has a drift of its own in space, for the early determinations of the position of the apex of the sun's way, which were derived from a discussion of the motions of a comparatively small number of swiftly-moving stars, lie to the south-west of the group of positions of the apex derived from a discussion of the motions of a much greater number of small stars, many of which probably belong to the galactic system. The direction of the drift of the galactic system through space may some day be more definitely determined when a separate investigation of the sun's drift is made by considering the proper motions of each type of stars independently of other types.

If the sun is moving in space with a velocity of, say, eighteen miles a second relatively to the galactic system, it seems probable that in less than a million years, if no untoward accident should happen to the solar system in passing through or close to the Milky Way, the astronomer of the future may be able to study the galactic system from the outside; for with a velocity of eighteen miles a second the sun would in 450,000 years be carried across a space equal to ten times the distance between the sun and α Centauri, and unless the stars associated with the Milky Way are on a gigantic scale compared with our sun and the Sirian [1] and solar stars whose mass we have been able to estimate, the galactic ring cannot have a diameter twenty times as great as the distance separating the sun and α Centauri.

Judging by their proper motions, it seems that the solar stars are moving in all directions in space, and if the conclusions arrived at above are correct, it follows that we have under our observation stars derived from very widely separated regions of space. We should, therefore, expect to find amongst them stars representing widely different periods of stellar development, even though the epoch when the matter of the universe was uniformly distributed

but of the stars which are not situated far from the equator of the sun's way, there are five of the Sirian type which, according to Dr. Vogel, exhibit rapid motion in the line of sight—viz. α Andromedæ, + 28 miles a second; β Ursæ Maj. (brightest component), – 18·2 miles a second; ζ Ursæ Maj., – 19·4 miles a second; Castor, – 18·4 miles a second; and η Ursæ Maj., – 16·3 miles a second.

[1] The relative densities of the solar and Sirian binary stars, discussed on pp. 796–7, seem to indicate that solar stars are, on the average, fifteen times as dense as Sirian stars—that is, that a Sirian star of the same mass as a solar star would (if we assume their photospheres to be of equal brightness) have, on the average, a diameter nearly two and a half (2·466) times as great as the diameter of the solar star, and it would consequently give a little more than six times the light of the solar star, and would appear equally bright with it if situated at 2·466 times the distance from the observer as the solar star.

in space may be vastly distant, and though the process of condensation may have begun at about the same time in all parts of space. For with different masses of matter under different conditions as to cooling, collisions, &c., the periods of stellar development would differ greatly in duration, and, according to the assumptions of the nebular hypothesis, we should expect to find many systems within the region which we can examine with the telescope in the condition in which our sun was when the outer planets commenced to have a separate existence.

A shining mass having a diameter as large as the diameter of the orbit of Neptune would present a very recognisable disc in a large telescope, even though it were at a hundred times the distance of α Centauri from us; but we have hitherto failed to detect any stars exhibiting a recognisable diameter. We are, therefore, compelled to assume that, if the nebular hypothesis is true, condensing masses do not commence to shine like stars until they are much more condensed than the solar mass was when the outer planets, or even our Earth, commenced to have a separate existence. γ Leonis (the least dense of the binary stars whose orbits have been determined and densities estimated) seems to have a density considerably greater than our sun had when, according to the usual assumptions of the nebular hypothesis, Mercury commenced to have a separate existence (see p. 797 note). If condensing masses commence to emit light and shine as faintly luminous nebulæ at a period when they are less dense than the solar mass was before the birth of the outer planets, we ought to see many such nebulous masses compared with the number of stellar bodies; for, according to the ordinary assumptions of the nebular hypothesis, it can hardly be doubted that the period of nebular condensation would greatly exceed in duration the period during which the mass would have sufficiently condensed to shine like a star; but the number of brightly-shining stellar points greatly exceeds the number of nebular masses hitherto observed. To my mind this apparent contradiction is most simply accounted for by assuming that condensing masses do not become sufficiently heated to be luminous[1] until their density has become greater than the density of the solar mass during the period when the plants were separated; and it follows that the nebulæ we observe in the heavens do not correspond to the nebular masses conceived of by La Place.

[1] According to this assumption there are probably many more non-luminous masses in space than luminous stars, and these would no doubt tend to absorb the light of distant stars.

CONSTANTS OF THE SOLAR SYSTEM

Name	Mean Distance from Sun in Miles	Diameter in Miles	Mass	Domain within which Planet's Attraction surpasses Sun's: Radius in Miles	Domain within which Planet's Attraction surpasses Sun's: Volume in Billions of Cubic Miles	Velocity in Miles per Second for Motion in Circular Orbit	Velocity in Miles per Second for Motion in Parabolic Orbit	—
The Sun . . .	—	865,240	330,500,000	—	—	270·52 At sun's surface	382·5	382·57
Mercury . . .	35,915,000	3,010	66	16,060	4·144	29·690	41·988	2·901
Venus . . .	67,110,000	7,707	778	102,940	1000·870	21·720	30·717	6·218
The Earth . .	92,780,000	7,927	1,000	161,390	4200·533	18·472	26·124	6·953
Mars	141,368,000	4,247	112	82,310	557·598	14·965	21·164	3·179
The Asteroids (mean)	259,780,000	—	50		—	11·039	15·612	—
Jupiter . . .	482,716,000	86,520	315,393	14,912,000	3,315,939,000	8·098	11·453	37·373
Saturn . .	885,015,000	70,930	94,384	14,956,000	3,315,427,000	5·981	8·458	22·580
Uranus . . .	1,779,834,000	31,900	14,624	11,839,000	1,659,495,000	4·218	5·965	13·255
Neptune . . .	2,788,445,000	34,700	17,053	20,030,000	8,036,390,000	3·370	4·765	13·721
The Moon . .	92,780,000	2,160	12·38	17,960	5·793	18·472	26·124	1·482

These are also the velocities with which bodies drawn to the sun from indefinitely great distances, under his sole attraction, would travel when at the distances of the several planets from him.

Velocities (miles per second) with which bodies supposed to be drawn from an indefinitely great distance towards each several member of the solar system, under its sole attraction, would reach the surface of the body attracting them.

Name	Masses	Mean Angular Semi-diameters	At dist.	Angular Diameters at Distance Unity: Polar	Angular Diameters at Distance Unity: Equatorial	Mean Diameter in Miles	Density Water = 1	Density Earth = 1	Axial Rotation	Gravity at Surface	Periodic Time	Orbital Velocity in Miles per Second
				′ ″	′ ″						Days	
Sun . . .	Unity	960·0	1·00	32 0·00	32 0·00	860,000	1·444	0·2552	25 to 27 days	27·71		--
Mercury .	[illegible] (?)	3·34		0 6·68	0 6·68	2,992	6·85	1·21	24h 5m (?)	0·46	87·97	29·55
Venus . .	[illegible]	8·55		0 17·10	0 17·10	7,660	4·81	0·850	23h 21m (?)	0·82	224·70	21·61
Earth . .	[illegible]	8·84		0 17·64	0 17·70	7,918	5·66	1·000	23h 56m 4·09	1·00	365·26	18·38
Mars . .	[illegible]	4·69	—	0 9·36	0 9·12	4,211	4·17	0·737	24h 37m 22·7	0·39	686·98	14·99
											Years	
Jupiter .	[illegible]	18·26	5·20	0 184·2	0 195·8	86,000	1·378	0·2435	9h 55m 20·0	2·64	11·86	8·06
Saturn . .	[illegible]	8·10	9·54	0 146·3	0 162·8	70,500	0·750	0·1325	10h 14m	1·18	29·46	5·95
Uranus .	[illegible]	1·84	19·2	0 70·7	0 70·7	31,700	1·28	0·226	Unknown	0·90	84·02	4·20
Neptune .	[illegible]	1·28	30·0	0 77·0	0 77·0	34,500	1·15	0·204	Unknown	0·89	164·78	3·36

Name	Mean Motion in 365¼ days	Mean Distance from the Sun: Astronomical Units	Mean Distance from the Sun: Millions of Miles	Eccentricity of Orbit	Elements of the Orbits for 1850: Longitude of Perihelion	Inclination to Ecliptic	Longitude of the Node	Mean Longitude of Planet, 1849. Declination, 31·0	Authority
	″				° ′ ″	° ′ ″	° ′ ″	° ′ ″	
Mercury .	5381016·2925	0·3870988	35⅞	·20560478	75 7 13·8	7 0 7·71	46 33 8·6	323 11 23·53	Leverrier
Venus .	2106641·3980	0·7233322	66¾	·00684331	129 27 14·4	3 23 34·83	75 19 52·2	243 57 44·34	Leverrier
	2106641·3040	0·723		·00684311	129 27 42·9	3 23 35·01	75 19 53·1	213 57 43·82	G. W. Hill
Earth .	1295977·4260	1·0	92⅞	·01677110	100 21 21·4			99 48 18·66	Leverrier
	1295977·4212	1·0	-	·01677120	100 21 41·0		—	99 48 17·71	Hansen
Mars . .	689050·8013	1·5236914	141	·09326113	333 17 53·5	1 51 2·28	48 23 53·0	83 9 16·92	Leverrier
Jupiter .	109256·6197	5·202800	480	·0482519	11 54 58·2	1 18 41·37	98 56 16·9	159 56 12·94	Leverrier
Saturn .	43996·0508	9·538852	881	·0559428	90 6 56·5	2 29 39·80	112 20 52·9	14 50 28·49	Leverrier
	43996·209	9·5388		·0560470	90 3 59·8	2 29 39·20	112 20 0·0	14 49 43·50	G.W. Hill
Uranus .	15424·797	19·18338	1771	·0463592	170 38 48·7	0 46 20·92	73 14 37·6	29 12 43·73	Newcomb
Neptune .	7865·862	30·05437	2775	·0089903	46 9 13·1	1 46 58·75	130 7 18·3	334 30 5·75	Newcomb

Velocity of light, 186,616 miles per second.

Distance of α Centauri (assumed parallax, 0·75″), 274,930 astronomical units.

Logarithm of the number of seconds in a year, 7·49909.

Logarithm of the astronomical unit in miles, 7·96745.

Density of air at standard pressure and temperature, water unity, $\frac{1}{769{\cdot}8}$.

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